Effect of minimum wage reform on reservation wages

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Abstract

The aggregate effect of minimum wages on the economy remains a topic of intense debate, both among academics and the broader public, while its effects on the micro level remain relatively understudied. In this paper I use quasi-experimental techniques to identify the effects of minimum wage increases in the reservation wages of the unemployed, using state level variation in the minimum wage from late 2003 to late 2021. I find that for low wage workers, the increase in minimum wages is associated with a 5.1 to 7.5 percent increase in reservation wages. These results strongly suggest that reservation wages rise in response to minimum wage increases and the ensuing effects on labour supply may partially explain why the literature has struggled to find significant disemployment in the US labour market as a resulting from minimum wages.

1 Introduction

Competitive equilibrium theory argues that minimum wages are a type of price control, and they distort labour market demand, ultimately leading to higher unemployment in the low wage sector. On the other hand, the literature has several paper that find minimum wage standards do not affect employment significantly: e.g. Card and Krueger, 1993 and Dube et al., 2010[1](discussed in more detail in Section 1.1). While the employment effects of minimum wages are framed around labour demand response, the behavioral response of minimum wages on labour supply has long been understudied. In this paper I use quasi-experimental methods, along with nationally relevant data to identify the causal impact of a minimum wage on reservation wages.

I present results that show minimum wage increases have a positive effect on reservation wages using survey data made available by the Department of Labor. This survey, called the Benefits Accuracy Measurement (henceforth "BAM") is a repeated cross-section done on unemployment insurance (henceforth "UI") claimants to detect errors and fraud in the US' largest UI systems. The main challenges to identification lie in differences in demographics, and socio-economic realities across the states, and thus any differences in reservation wages could simply reflect the co-evolution in population demographics, cost-of-living changes, etc. The rich data available in BAM, along with its high-frequency allows me to control for a variety of confounding variables, and incorporate quasi-experimental designs to tease out a causal relationship. In particular, comparing the difference in the reservation wages of the above and below median pre-separation wage earners, with spells starting before and after a minimum wage increase yields (after the inclusion of the appropriate lead and lag terms) a stacked event study model that suggests an average 5.7% increase in hourly reservation wages following a minimum wage increase (associated with average minimum wage increases of averaging 9.06%, or about 0.56 cents per hour). The above estimates represent the dynamic treatment effects of minimum wage increases. Standard errors are clustered at the state level. As such, the stacked event study design reports the average response to minimum wage increases at the state-level.

It's possible that these estimates are biased due to confounding factors, e.g. shocks to the national economy that percolate to the states over divergent timelines and magnitudes. I can address these structural disparities between the economies of various states using a triple-diff design, with the three differences being between (1) low and high wage earners, (2) unemployed workers entering their spells before and after a minimum wage change goes into effect, and (3) states that raise their minimum wages against those that don't. More precisely, I compare reservation wages of job-seekers in states that raised their minimum wages against the reservation wages of job-seekers in states that didn't (a state which raises its minimum wage is compared to states that did not raise their

[1]Still other researchers e.g. economist David Cooper find that a higher minimum wage would "[result] in the creation of roughly 85,000 net new jobs": https://www.epi.org/publication/raising-federal-minimum-wage-to-1010/

minimum wage 6 weeks before or after the minimum wage was increased), and find that states that did raise their minimum wages saw an average of 5.7% rise in hourly reservation wages compared to the states that didn't. These results remain robust to the inclusion of several demographic, and industry controls.

Notably, BAM has pre-separation wage information as well, which allows me to address the possibility of composition effects in my quasi-experimental designs. Across the states, and over the time period covered by the BAM sample, I am able to analyse 439 minimum wage increases, across two recessions. Furthermore since my data is weekly, I can control for anticipation effects as well. As a hypothetical: we may be concerned that employers might fear higher costs as a result of a minimum wage increase in the near future and fire their least productive employees. This would be an example of an anticipation effect and could bias the sample with unemployed workers whose reservation wages are closer to the minimum wage. This compositional shift of the pool of unemployed biases the magnitude of treatment effects downwards (see Section 4.2 for a more formal discussion). To obviate this concern, I drop all observations that enter unemployment just before and just after a minimum wage change goes into effect, thereby ensuring my treatment and control groups remain comparable. I also test the robustness of these results by analysing different treatment and control cohorts.

This analysis bears significant relevance for policy makers because many of the beneficial effects of raising the minimum wage are contingent on the presence of employers wielding monopsony power in the labour market [2]. In particular the idea is that by weakening the monopsony tax, the minimum wage can induce higher labour force participation. While there is broad consensus that minimum wages affect the wage distribution, it is not clear what the behavioral response from job-seekers is while setting their reservation wages. Since reservation wages can be passed on to actual wages via wage setting, especially in tight labour markets, the need to consider the reservation wage response channel is both topical and obvious[3].

Despite the acute emphasis policy makers place on the minimum wage, the direct impact of such policies on reservation wages remains understudied [4]. This void in the literature remains unaddressed due to a number of reasons. First is a paucity in the data on the reservation wages of job-seekers. Second, minimum wage policies are not exogenous, especially at the state level. As noted in Fishback

[2]E.g. noted in Manning, 1995 and Flinn, 2003. Labour markets with search frictions and match-specific capital can generate such monopsonies.

[3]For instance dubbed the Great Resignation, the post COVID-19 labour market saw a lot churn and one of the tightest labour markets in US history. Many employers responded by raising salaries to attract talent: https://www.wsj.com/articles/stay-for-pay-companies-offer-big-raises-to-retain-workers-11672607138. Many workers cited low pay as a reason for leaving their jobs: https://www.pewresearch.org/short-reads/2022/03/09/majority-of-workers-who-quit-a-job-in-2021-cite-low-pay-no-opportunities-for-advancement-feeling-disrespected/.

[4]The indirect impact of minimum wage policies, e.g. the impact of minimum wage policies on human capital accumulation decisions have been looked at, e.g. in Schanzenbach et al. (2023), Lee (2020), and Mattila et al. (1981)

and Seltzer (2021), states have led the way in raising minimum rates of pay since the 1980s. The major differences between the political-economy of the states motivate the disparate histories of the minimum wage policies at the state level, as well as their disparate wage-offer distributions. E.g. the living wage in MA is almost 90% higher than the living wage in MS [5]- it is very unlikely that MA coincidentally also has one of the highest minimum wages in the country. Third, the entire wage distribution is endogenous, either as a result of compression of relative wages, or due to truncation of low skill workers from the labour pool (due to the presence of a wage floor). Additionally, researchers face reverse causality, e.g. if some confounding factor drives wage dispersion and the median wage in a state, the Kaitz index [6] will fall and researchers may erronusly conclude that a less binding minimum wage leads to higher wage dispersion.

Furthermore, it is worth noting that economic theory equivocates on the predicted response of reservation wages following a minimum wage increase. Neoclassical theory in competitive labour markets predicts that labour supply will be pushed up by an income effect, and pushed down by a substitution effect. A McCall job-search model says the net effect on reservation wages depends on the effect on offer arrival rates, the effect on the wage-offer distribution [7]. As a thought experiment consider the effect of an increase in the minimum wage. Does this cause the reservation wages to rise since workers notice that the worst possible offer is now better, so its worthwhile to wait for better offers? Or does it fall as employers respond to a higher wage-floor by posting fewer vacancies, which in turn dampens the offer arrival rate?

The results of this analysis show that reservation wages are sensitive to the wage-offer distribution and therefore the data reported in BAM represents a real behavioral response to minimum wage policy. We might be concerned about the informational valance of the reservation wage information in BAM. The results of a Mincer type regression, tabulated in C1 show that reservation wages respond to confounders in the expected direction and the coefficients are precisely estimated.

1.1 Related literature

While the direct labour supply effects of minimum wages remains under explored in the literature [8], the effects of the same on low-wage employment overall have been, and continue to be debated at length. Beginning with one of the earliest papers using natural experiment designs, Card and Krueger (1993) uses the

[5]The living wage in MA is $87,909, while it's $45,906 in MS: https://www.cnbc.com/2023/08/29/the-salary-a-single-person-needs-to-get-by-in-every-us-state.html.

[6]Defined as the ratio of the nominal minimum wage and median wage.

[7]A more formal treatment of the McCall model is given in Section A.1

[8]For example several papers study the effects of minimum wages on skill acquisition, on high school enrollment and completion in particular. Mattila et al. (1981), Ehrenberg and Marcus (1982), Neumark and Wascher (1995) study high school enrollment. Lee (2020) and Schanzenbach et al. (2023) address post-secondary enrollment. Broadly these papers confirm the hypothesis that minimum wage policies are associated with lower rates of enrollment.

difference in minimum wages between New Jersey and Pennsylvania to find the state with the higher minimum wages (NJ) saw a 13% rise in employment in the fast-foods sector. This finding triggered a flow of subsequent papers in the literature addressing the combination of trivial or negative wage truncation effects and offer distribution contraction effects [9].

Dube et al. (2010) generalises the case study design and exploits policy variation at the state border to find no adverse employment effects due minimum wages using all local differences across the country from 1990 to 2006. In contrast Neumark and Wascher (2004) use a pooled cross-section time series data on 17 OECD countries with minimum wages from 1975-2000 to find minimum wages are consistent with job losses among young workers. Neumark et al. (2014) revist the minimum vs employment debate by assessing new studies that argue negative employment response can be explained by a failure to account for spatial heterogeneity. The authors evaluate Allegretto et al. (2011) and Dube et al. (2010) and test the designs' assumptions about constructing control groups. Furthermore using methods that the data identify the appropriate controls Neumark et al., 2014 show evidence of negative employment effects, with teen employment elasticities near -0.15. Allegretto et al. (2017) use data from 1979 to 2014 to contradict the findings in Neumark et al. (2014). In particular, they use a LASSO to correct for state trends and find a teen employment elasticity an order of magnitude smaller compared to Neumark et al. (2014). Neumark and Wascher (2017) respond to Allegretto et al. (2017) by pointing out (1) Allegretto et al. (2017) do not address the criticisms raised in Neumark et al. (2014), (2) emphasize the need to use "close controls [10]," but close controls do not generate large differences in the findings and finally, (3) Allegretto et al. (2011) dismiss a growing number of studies that contradict their results.

Autor et al. (2016) use two decades of data to show that the minimum wage reduces inequality in the lower tail of the wage distribution. The authors test, and are unable to reject the null hypothesis that minimum wage effect spillovers are due to measurement errors. If this hypothesis is true, the implied minimum wage effects on the wage distribution are smaller than measured. In Clemens and Wither (2019), the authors use SIPP to find the federal minimum wage increase in 2009 had significant disemployment effects among low-skilled workers. Cengiz et al. (2019) use wage data to estimate the disemployment effect of minimum wage increases by comparing the number of excess jobs paying at or just above the minimum wage to the number of missing jobs paying less than it in a stacked event-study design. Although they find the disemployment effect to be insignificant, Chen and Teulings (2022) argue that this conclusion cannot be drawn without strong functional form assumptions i.e. Cengiz et al., 2019 assume the fall in wage dispersion is due to wage distribution compression and

[9] E.g. Flinn (2003) uses an extension of the McCall model to show employment might rise with minimum wage due to monopsony in labour markets. Also see Engbom and Moser (2022) and Machin et al. (2003) for monopsony models with search frictions that reach similar conclusions in a non-American context.

[10] close controls refer to control groups within the same county as the treatment groups

not wage distribution truncation. Chen and Teulings (2022) argue that Cengiz et al. (2019) do not justify this assumption sufficiently.

All of the papers cited so far focus on the demand side of the debate. Turning our attention to labour supply effects, as manifested, by duration of unemployment spells, we may first consider Fortin and Lacroix (1997) use a natural experiment design induced via repeal of age based discrimination in unemployment benefits (in Canada), in which the authors find that while minimum wages have an adverse spell-duration effect on the unemployed aged 18-24, it has a positive effect on those aged 25-29. Similarly, Pedace and Rohn (2011) uses the Displaced Worker Survey to find higher minimum wages are correlated with shorter spells for older men, and longer spells for high school dropouts, and older females in low-skill occupations. None of these papers mention reservation wages.

The empirical literature on reservation wages is still evolving, but research from Europe suggests reservation wages are subject to labour market policy pressures, as we might expect. Consider Brown and Taylor (2015), in which the authors use the British Household Panel Survey (BHPS) to draw a curve representing the relationship between reservation wages and local unemployment. In particular, the authors find that reservation wages fall with rise in local unemployment. Koenig et al. (2016) also use the BHPS, and the German Socio Economic Panel Survey (SEOP) to attempt to resolve the Shimer puzzle [11] and argue that workers use previous earnings and minimum wages as reference points while setting wage expectations, and thus don't react to market turbulence rationally. While the former paper uses the unexpected introduction of a tax credit to identify the effect on reservation wages, the latter uses a DMP framework to estimate reservation wage elasticity with respect to the business cycle (neither paper comments on the reservation wage response to shocks induced by minimum wages).

In a lab experiment, Falk et al. (2006) ask "why do [profit-maximizing] employers pay more than the required minimum for those workers who earned less than the new minimum wage before it was introduced?" And answer: the wage-floor cements "entitlement effects" which causes would-be employees to revise their reservation wages upwards, and this effect persists even after the minimum wage has been repealed. In contrast to these findings however, Sousounis and Lanot (2022) (using BHPS data; from 1998 to 2008) use an RDD and are unable to reject the null hypothesis that reservation wages are not significantly revised upwards as a result of minimum wage shocks, stating: "[the behavioral response is] too small to be extracted from the variability in reservation wages." That said, the authors mention that their results are not precise and the authors conclude "because of their lack of precision and despite not being directly comparable, out findings do not contradict the very limited available empirical evidence in the literature."

[11] Shimer (2005) argues that search and matching model predictions overestimate wage volatility in the face of business cycles; see Cardullo (2010) for more details.

Using the SOEP, Fedorets and Shupe (2021) study the impact of the introduction of a federal minimum wage in Germany in 2015 on reservation wages. Using a fuzzy RDD, the authors find a 16% increase in reservation wages caused by the introduction of a minimum wage. However, wage expectations revert to pre-reform levels over time, suggesting null effects on labour force participation. This paper is thematically closest to Fedorets and Shupe (2021) but differs from their analysis in several key aspects: First, their data uses a yearly frequency, due to which they aren't able to control for anticipation effects. Second, their natural experiment is the introduction of a nationally binding minimum wage, while I compare over 400 state-level minimum wage increases with a much richer dataset, which allows me to account for a wider set of confounding variables, e.g. level of UI entitlements. Furthermore, the authors note that "roughly 60% of the sample answered the survey question regarding their reservation wage... reservation wages wage information is not missing entirely at random... the share of women, married individuals and those with a university degree is higher among the non-response group." This might cause a bias their results; my dataset, BAM, doesn't have this issue (see Section 2.2 for more details), with a greater than 99% response rate (4,732 missing values out of 504,339 responses).

Due to the lack of reservation wage data in the US, most of the papers looking at reservation wages empirically have come from Europe. Thus Krueger and Mueller (2016) maybe regarded as the first empirically motivated work on reservation wages in an American context; the authors interview 6000 NJ workers for 24 weeks and ask for their reservation wages. They find that compared to a calibrated model, reservation wages start out too high and do not fall sufficiently fast enough over the course of an unemployment spell. Interestingly, they note that reservation wages are, at best, modestly affected by UI. The authors speculate that this maybe because unemployed workers view drawing from UI differently, as opposed to drawing from personal savings, rely on social networks for support, or have inflated wage-offer expectations. Whatever the case maybe, we must consider the possibility that UI props up reservation wages. This in turn might translate to longer spells, and a weaker labour supply than we might otherwise expect. The above chain of effects could partially explain the muted employment effects highlighted by some of the papers discussed in this literature review. If we hypothesize that minimum wages also bolster reservation wages, and similarly, depress labour supply at low wage rates, the unemployment effects of minimum wages may not be as large as predicted (as has been noted by many paper in the literature).

The rest of this paper is organized as follows: Section 2 describes the data, presents some stylized facts, and empirical strategy used in estimation, Section 3 discusses the results and discusses various impact channels of the same, while Section 4 discusses robustness checks, and finally Section 5 concludes.

2 Empirical reasoning

2.1 Institutional background

At the time of writing this paper, the federal minimum wage for non-exempt employees is $7.25 per hour [12], having being raised to that figure on July 24, 2009, and 20 states either don't have a minimum wage, or set it below the federal level (in which case the binding minimum wage is set to the federal rate). In general, states can set their own minimum wages, and when the state and federal minimum wages differ, the higher number prevails. In Figure 1 we can see snapshots of the minimum wage across the states in 2008 and 2018. In 2008 the minimum wage was $6.55 at the Federal level, and was raised to $7.25 in 2009 (where it has been held till date). In the intervening 10 years, the federal minimum wage was just raised once in 2009, but several local changes were implemented at the state level. Figure 1 highlights the fact that movements in the minimum wages have been primarily driven at the state level in the last couple of decades and also shows how heterogeneity across the states in terms of the local political-economy result in substantial differences in the minimum wage rate. Indeed, the binding minimum wage in Texas was set to the federal level, at $6.55 in 2008 and $7.25 in 2018, while in Washington state it was $8.07 in 2008 and $11.00 in 2018(in fact, WA has raised the state minimum wage every year since 2001, and was raised to $15.74 in January 2023).

The role of states as the central drivers of minimum wage policies can be seen once again in Figure 2, which shows the minimum wage by state (states listed in alphabetical order) for the past 12 years. In contrast to states like Washington, 25 states have passed preemption laws that prohibit local governments from setting their own minimum wages. Interestingly enough, WA has one of the highest median hourly wages as well ($22 per hour, making it the 2nd highest) as of 2020. Washington's high median wages, and high minimum wages serves as example to highlight, the (at least) partial endogeneity of the states' minimum wage policies and their labour market outcomes. As noted in Chen and Teulings, 2022: most research uses the Kaitz index as a measure of the bindingness of the minimum wage, but the minimum wage is endogeneous, and some outside force might drive wage dispersion and the median wage. In such a case, researchers may erroneoulsy conclude that a less binding minimum wage leads to more wage dispersion. The high frequency data in BAM allows me to compare contemporaneous reservation wages in states which raise minimum wages with reservation wages in states that don't, thus enabling me to identify causal effects previous research couldn't.

[12] Exempt employees include workers employed as bona fide executive, administrative, professional and outside sales employees. Such exemptions are determined on the basis of certain tests. Find more details here: https://www.dol.gov/agencies/whd/fact-sheets/17a-overtime

[12] See https://www.epi.org/preemption-map/ for more details.

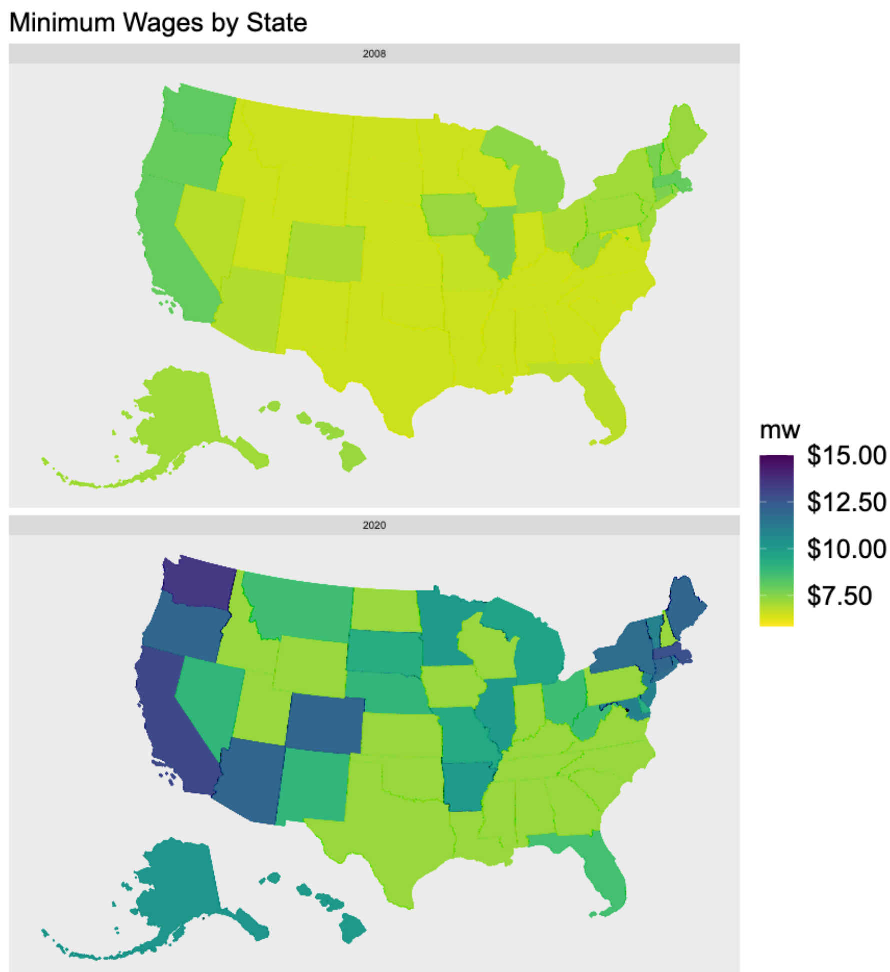

Figure 1: Compare the nominal minimum wages across the states in 2008 vs 2019.

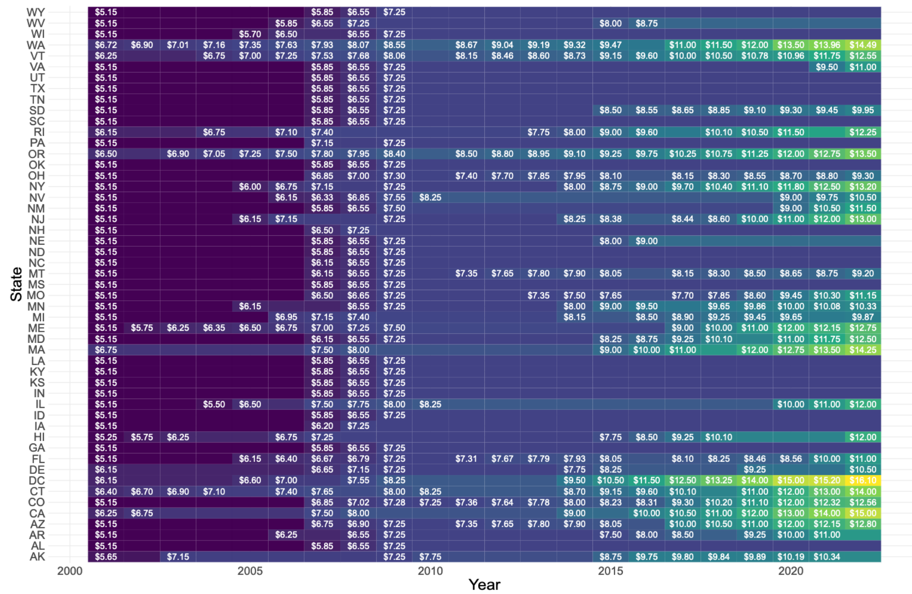

Figure 2: Minimum wage by state and year; cells contain the binding state minimum wage.

2.2 Benefits Accuracy Measurement

My main source of data for reservation wage information is the Benefits Accuracy Measurement survey conducted by the Department of Labor. By reconstructing the claims process for samples of weekly paid and denied claims with data verified by trained investigators, BAM seeks to assess and improve the accuracy of the three major Unemployment Insurance programs. These three programs are: how many in each?

- State UI system
- Unemployment Compensation for Federal Employees
- Unemployment Compensation for Ex-Service Members

Each week a random sample of the UI claimant population from each state is selected for an audit. Sample sizes range from 360 to 480 per week across the country, depending on the size of the UI caseload. BAM data is available for paid and denied UI claims for all 50 states and PR since 2001. This results in a repeated cross-sectional dataset of over 504,339 observations and about 110 data elements for each respondent. These elements detail a claimant's pre-separation wages, UI entitlement, state of residence, demographic information, industry and occupation codes, education level, and vocational training. In Table 1 I outline how this sample get cut down due to sample restrictions imposed in the final analyses. In particular, I focus on job-seekers living in the US except Puerto Rico (PR), with valid wage information. After dropping individuals with

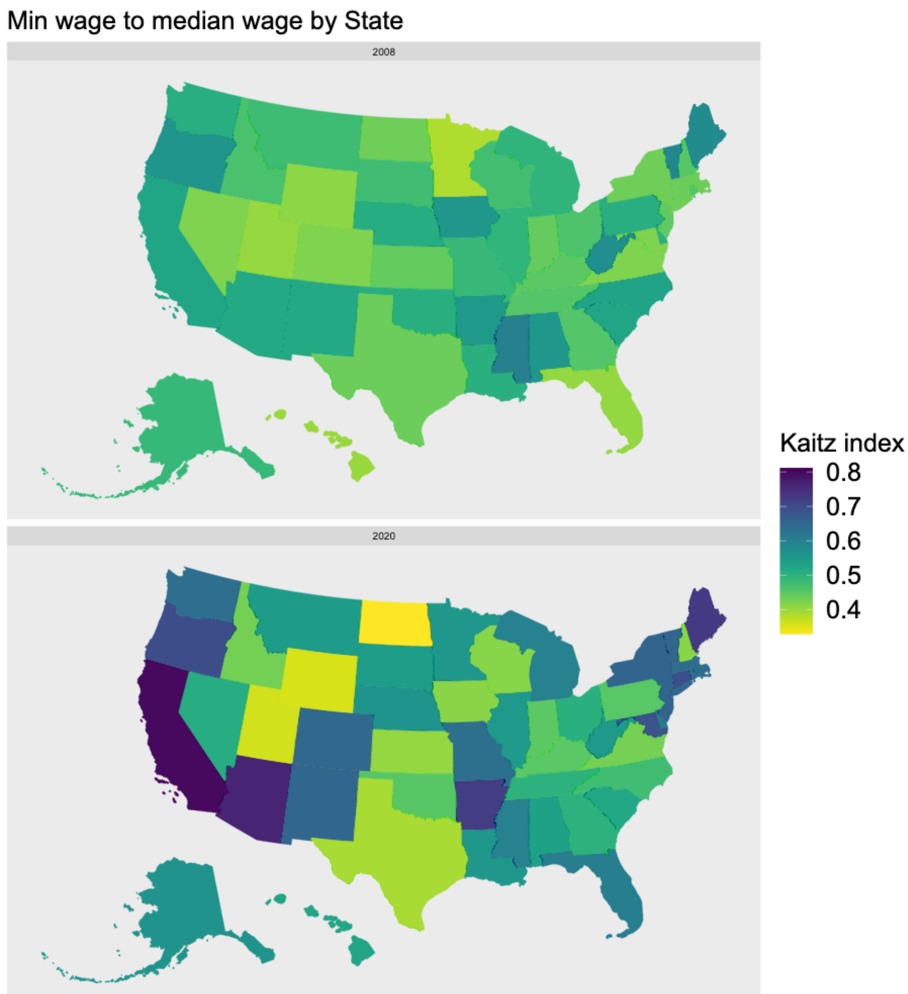

Figure 3: The above plots show the Kaitz index for the states in 2006 (above) and in 2016 (below) using hourly wage data from CPS MORG and minimum wage data from David Neumark.

pre-separation wages below minimum wages, and missing information, in the prime ages (aged 24-55), the sample is left with 373,978. Outliers with responses above the 99th percentile, and below the 1st percentile in each year have been winsorized.

Table 1: BAM sample size by restriction

Total sample	504,339
Dropping PR, and individuals with missing wage information	493,046
Pre-separation wages greater than the minimum wage	491,267
Not missing education information	483,361
Not in armed-forces	479,428
Unemployment spell duration < 35 weeks	473,741
In prime age (24-55)	373,978
Final sample	373,978

Source: Benefits Accuracy Measurement (BAM) program 2004-2021. The above table shows the decrease in the sample size resulting from each sampling restriction used in the research design.

With the above restrictions in place I plot the median pre-separation wage for the 50 states in 2008 and 2019 in Figure 5. The relative movements in the minimum wage and median pre-separation wage can be seen by comparing Figures 1 and 5. Although the state minimum wage for TX did not budge in the intervening 11 years (although the federal minimum wage was increased in 2009), the median pre-separation wage rose from \$16.52 to \$22.78 per hour. Over the course of the same time period, the median pre-separation wage in CA rose from \$17.71 in 2008 to \$20.63 in 2018, while the state minimum age in CA rose from \$8 per hour in 2008 to \$11.00 per hour in 2018.

In Table 2 I present descriptive statistics of the difference in logs between the reservation wage and pre-separation wage for various demographic groups. After dropping all observations with missing information, I have a sample of 373,978 claimants.

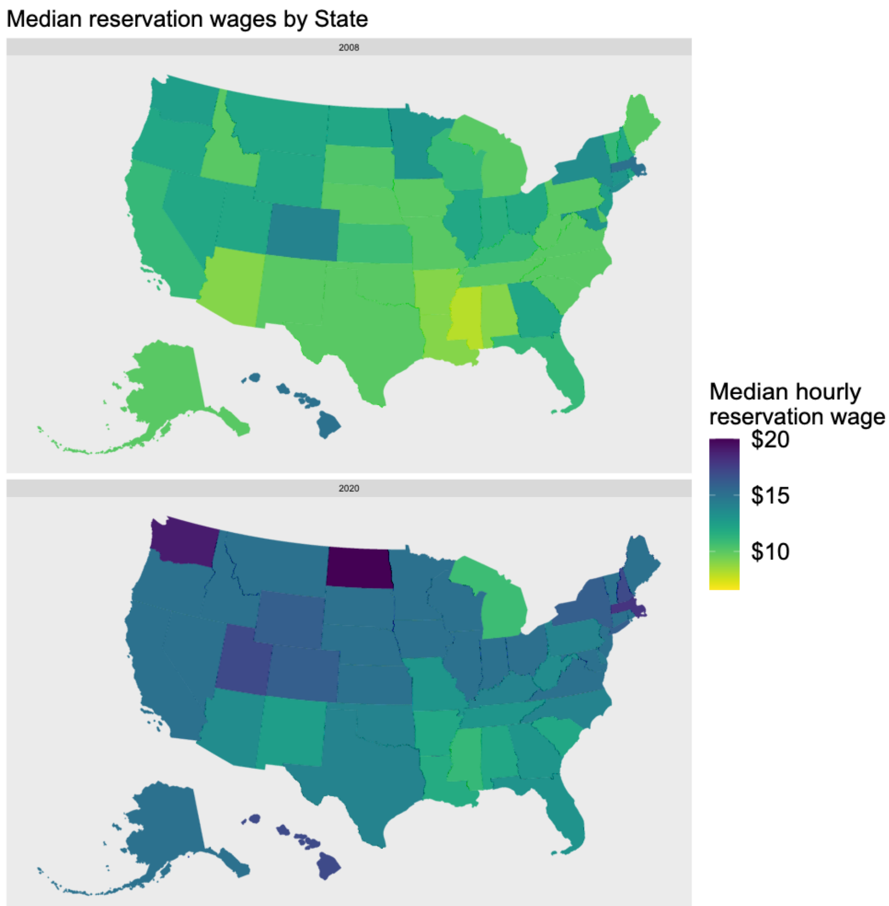

Figure 4: Median reservation wages by state in 2008 and 2018

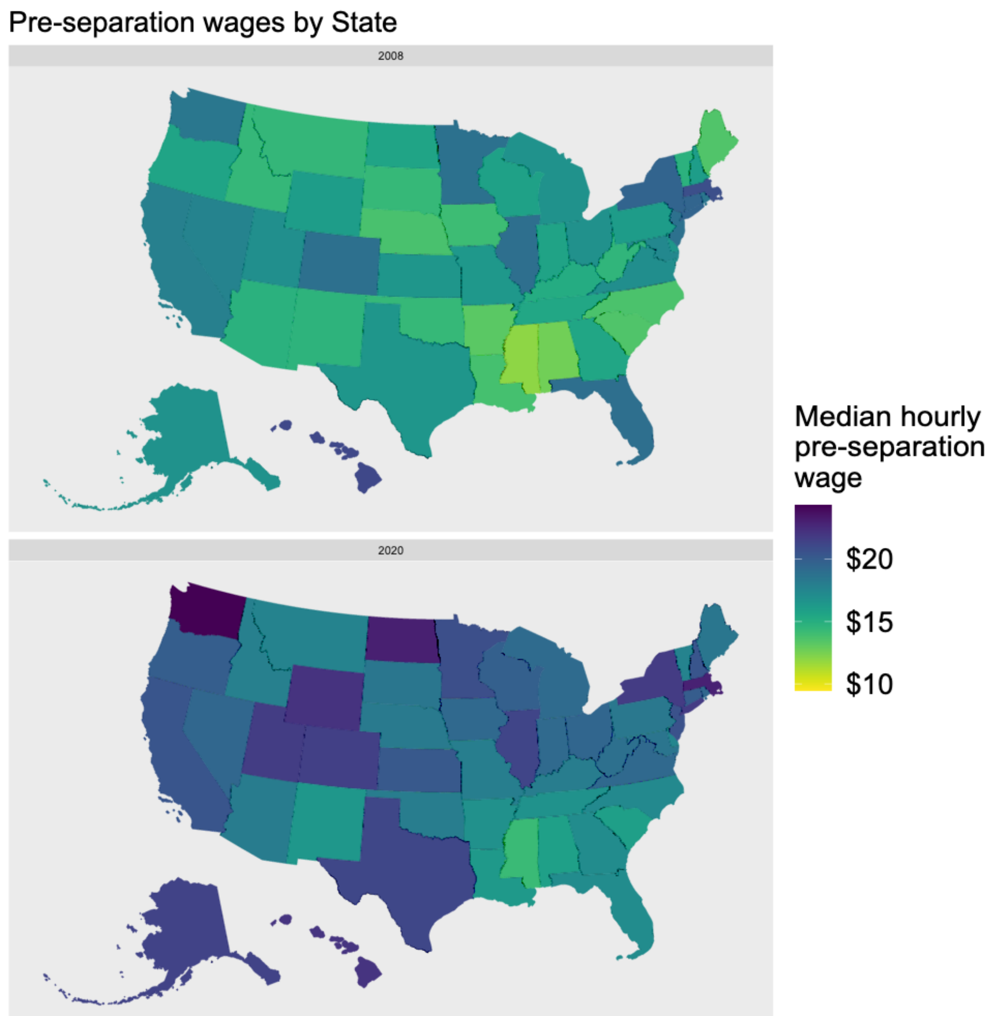

Figure 5: The above plots show the median pre-separation wages for each state in 2008 and 2019.

Table 2: Summary statistics of difference in logs of pre-separation wages and reservation wages

Age Bins	Mean Reservation wage	Mean Pre-separation wage	Mean log gap	SD	Median log gap	25th percentile	75th percentile
18-25	11.27	13.53	0.17	0.25	0.11	0.00	0.27
26-35	12.91	15.86	0.20	0.26	0.14	0.00	0.30
36-45	13.72	16.99	0.21	0.27	0.15	0.00	0.32
46-55	14.00	17.25	0.21	0.27	0.16	0.00	0.30
56-65	14.55	17.61	0.20	0.26	0.15	0.00	0.29
66-85	13.60	16.10	0.17	0.25	0.10	0.00	0.24
Sex							
Male	14.41	17.92	0.21	0.27	0.15	0.00	0.31
Female	12.01	14.55	0.19	0.25	0.13	0.00	0.29
Education levels							
LTHS	11.54	13.99	0.17	0.24	0.11	0.00	0.26
HS	13.25	16.31	0.20	0.26	0.13	0.00	0.30
COLL	16.94	21.24	0.22	0.28	0.19	0.03	0.32
Race and ethnicity							
White, non-hispanic	15.23	18.80	0.21	0.27	0.15	0.00	0.31
White, hispanic	12.26	14.93	0.19	0.24	0.13	0.00	0.29
White, unknown	16.74	20.21	0.18	0.25	0.13	0.00	0.25
Black, non-hispanic	13.37	16.40	0.19	0.25	0.13	0.00	0.29
Black, hispanic	13.76	16.77	0.19	0.27	0.11	0.00	0.29
Black, unknown	15.65	18.86	0.17	0.24	0.14	0.00	0.24
Asian, non-hispanic	14.01	17.55	0.23	0.34	0.14	0.00	0.30
Asian, unknown	15.97	19.28	0.18	0.26	0.12	0.00	0.22
Observations							373,978

Notes: The mean and median log gaps are calculated as the mean and median difference in logs of the pre-separation wages and reservation wages for each cohort. The last two columns report the 25^{th} and 75^{th} percentiles of the log difference between pre-separation wages and reservation wages.

It is worth noting that the distribution is skewed to the right (as the median of difference in logs of pre-separation wage and reservation wages is greater than the mean of the same). There doesn't appear to be an obvious trend in the log gap between pre-separation wages and reservation wages across demographic groups. However, considering this same gap by pre-separation wage quintiles paints a different picture; see Figure 6, which plots the trend in the log difference of pre-separation wages and reservation wages by pre-separation wage quintiles. In the lowest quintile, the difference between pre-separation wages and reservation wages doesn't vary with spell duration, but in the top quintile, it rises before levelling off- suggesting selection effects at play. Most states provide 26 weeks of UI, and the confidence bands widen as spell duration approaches 30 weeks, reflecting fewer observations with spells that long.

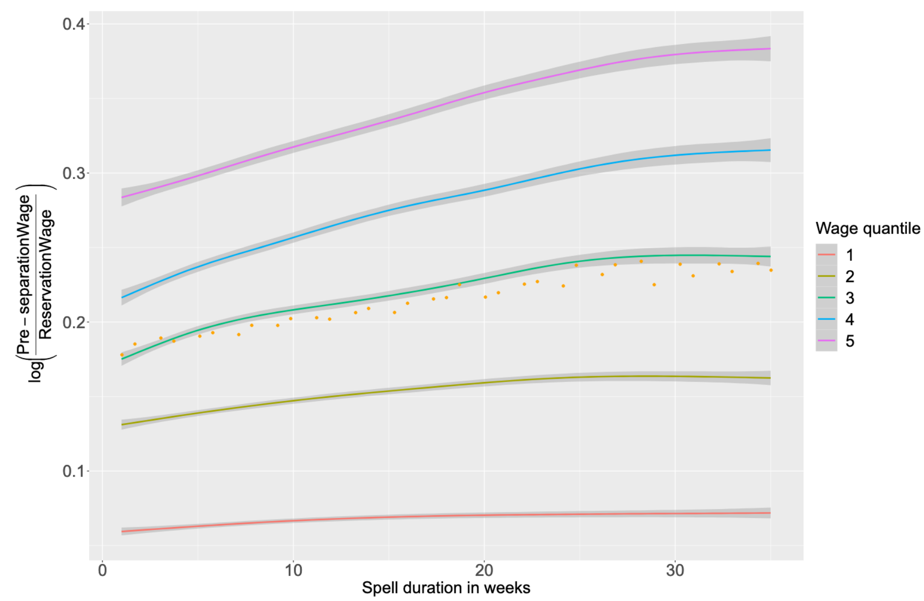

Figure 6: Log difference between pre-separation wages and reservation wages against the spell duration of job-seekers, for the 5 pre-separation wage quintiles (orange dots show the same difference for the whole sample pooled).

Furthermore, BAM audit respondents are asked to report their reservation wages. In particular, respondents are asked *What is the lowest rate of pay you would accept for a job?*, to be answered in dollars per hour, which constitutes their reservation wage.

This information should be considered reliable and valid, as auditors corroborate all responses with claimants, employers, and relevant third parties. Auditors complete investigations for over 99% of the claims they sample, and according to the General Accounting Office: *[Department of] Labor's benefit accuracy* *measurement data are …based on a statistically valid examination of a sample of paid and denied claims.*

We might be concerned that the reservation wages are a noisy signal of the pre-separation wages. However, the results of this paper show that BAM's data contains meaningful information about unemployed worker's behavior. In order to bolster confidence in the data, I estimate a state level event study design and report the results in 3. The results show a clear significant effect on reservation wages at the state level. In Figure 6 I plot the log difference between pre-separation wages and reservation wages against spell duration. As the spells get longer, we see the difference in pre-separation wages and reservation wages widen, suggesting reservation wages have a negative duration dependence, which is exactly what theory would predict. Furthermore, it should be noted that when running a Mincer type regression on reservation wages, the covariates have the expected sign, and are precisely estimated. Similarly, theory predicts the amount of benefits a job-seeker receives would be positively correlated with their reservation wage, and fall as benefits are exhausted. This table maybe found in the appendix, in Table C1 and it confirms our intuition.

Finally I estimate a state level event study design in which I aggregate my sample (after imposing the restrictions in Table 1) at the state level and generate a state panel over weeks of average reservation wages (results tabulated in Table 3). The results of this model show that at the state level, the treatment effect of raising the minimum wages is visible and significant at $\alpha = 5\%$. Thus we can be confident that the reservation wage data in BAM is not merely noise, but a true reflection of the unemployed workers' expectations/ behavior.

2.3 State minimum wage data

My second source of data was compiled by David Neumark and Peter Shirley for their paper, Neumark and Shirley (2022), which records the monthly minimum wage for each state since 1960. In effect, this dataset is a state panel with the prevailing minimum wage for each state over time. Here, prevailing minimum wage refers to the actual minimum wage faced by a worker in a given state. For example, if a state a has a minimum wage lower than $7.25 in 2010 (the Federal minimum wage was set to $7.25 per hour in July 2009), then the prevailing minimum wage would be $7.25. In contrast, if a state had a minimum wage higher than $7.25 during the same year, the prevailing minimum wage would reflect that higher figure. I further augment this data using Vaghul and Zipperer's "Historical State and Sub-state Minimum Wages [13]", since my reservation wage data contains observations till late 2021, and Neumark and Shirley's minimum wage data ends in late 2019.

[13] https://github.com/benzipperer/historicalminwage/releases/tag/v1.4.0

Table 3: State level event study design

	Dependent variable: Log of reservation wage
State	0.010***
	(0.002)
Post	0.002**
	(0.001)
Log of pre-separation wage	0.978***
	(0.002)
State × *Post*	0.062***
	(0.003)
State × *Lag*(−4)	0.046
	(0.043)
State × *Lag*(−3)	0.005
	(0.009)
State × *Lag*(−2)	0.015*
	(0.008)
State × *Lag*(1)	0.153***
	(0.036)
State × *Lag*(2)	0.146**
	(0.062)
State × *Lag*(3)	0.091**
	(0.039)
State × *Lag*(4)	0.042***
	(0.010)
Constant	0.074***
	(0.007)
Observations	56,386
Adjusted R^2	0.534
Note:	*p<0.1; **p<0.05; ***p<0.01

Notes: State fixed effects and seasonality terms included but not shown.

2.4 Estimation strategy

2.4.1 Stacked event study design

The main challenge in estimating the causal effect of minimum wages on reservation wages lies in addressing the missing counterfactual, i.e. what would the reservation wages of the unemployed look like if the minimum wage hadn't been raised. To this end, I exploit state level variation in the minimum wage policy in a stacked event-study design similar to the approach used in Cengiz et al. (2019) (also used in Autor et al. (2006)). I examine the changes in reservation wages in a 12 week window around 439 minimum wage increases between 2004 and 2021 using this stacked event study design.

In particular, I first code every minimum wage increase in every state as an "event," leaving me with 439 such events. Next, I partition my sample by the median pre-separation wage in the state and year in which the minimum wage was increased. More precisely, I define the control group as the pool of unemployed workers with pre-separation wages greater than the median pre-separation wages in a given state, during the year the minimum wage was raised. Similarly, the treatment group consists of workers with pre-separation wages below the median in the same cohort. The pre-treatment period is defined as 6 weeks before a minimum wage increase went into effect, and the post-treatment period is defined as 6 weeks after the increase went into effect. Workers are sorted into the pre and post treatment periods depending on whether they were interviewed/audited before or after the minimum wage increase respectively. Comparing the difference between the treatment and control groups, before and after treatment, across the 439 events yields the familiar stacked event study design. Due to this attention to a relatively tight window around an event, I can't calculate the long term impact of minimum wages on reservation wages- however, a longer window introduces more confounding variables that make estimating the effect on reservation wages difficult.

I estimate the following regression specification:

$$\begin{aligned} Y_{ist} = {} & X_{ist}\beta \\ & + \sum_{k=-4}^{-2} \gamma_k \times Treat_{istk} + \sum_{k=0}^{4} \gamma_k Treat_{istk} + \sigma_s + \sigma_m + \epsilon_{ist} \end{aligned} \quad (1)$$

where the main outcome of interest, denoted Y_{ist} is the log of reservation of job seeker i in state s on date t. The treatment dummy $Treat_{stk}$ equals 1 if the minimum wage in state s was raised k weeks from date t, and the observation earned less than the state-year median pre-separation wage. $Treat_{stk}$ is always 0 for job seekers with pre-separation wage higher than the state-year median. I assume that job-seekers with pre-separation wages higher than the median are "never treated" as they aren't affected by changes to minimum wage policy. I justify this assumption by relying on past research that suggests labour market is segmented by wages, e.g. as shown in Engbom and Moser (2022). I also include controls for state fixed effects, σ_s and seasonality, σ_m. Finally, the error term is denoted ϵ_{ist}. All standard errors have been clustered at the state level.

The preferred baseline specification in Equation (1) doesn't include demographic controls (denoted by X_{ist}), although it does include pre-separation wages, so that average treatment effects can be interpreted straightforwardly, along the unconditional reservation wage distribution. I separately report results with several demographic controls added.

2.4.2 Triple-diff design

To account for the possibility of other confounding factors (e.g. economic downturns that propagate across the states with different timelines and magnitudes), and to bolster confidence in my results I also use a triple-diff design to estimate the causal effect of minimum wage increases on the log of reservation wage. [14] The first difference compares the log of the reservation wages for the unemployed audited just before the minimum wage is raised with the log reservation wages audited just after the minimum wage was raised.

I code the unemployed with pre-separation wages below the median pre-separation wages in a given state, on a given year as the treatment group, and unemployed with pre-separation wages above the median level for that same state-year cell as the control, for each of the 439 events (as discussed in Section 2.4.1). The procedure outlined thus far is similar to the one discussed in the previous section. Next I define an "experimental state" variable, such that once a state raises its minimum wage, it is compared to all other states that did not raise their minimum wages up to 6 weeks before or after the increase went into effect. The idea is to elicit a comparison between states that raises their minimum wages with ones that did not.

The model specification for the triple-diff design is as follows:

$$\begin{aligned} Y_{ist} = X_{ist}\beta + \delta(Group_{ist} \times Post_{ist} \times State_{ist}) + \lambda(Group_{ist} \times State_{ist}) \\ + \eta(Post_{ist} \times Group_{ist}) + \gamma(Post_{ist} \times State_{ist}) + \epsilon_{ist} \quad (2) \end{aligned}$$

where

- Y_{ist} is the variable of interest and is defined as the log of reservation wage of unemployed worker i in state s with unemployment spell starting on date t
- X_{ist} denotes a vector of controls
- $Group_i$ is an indicator that equals 1 if job-seeker i earned less than the state-year median pre-separation wage in his/her previous job

[14] According to Olden and Møen (2022): "the triple difference estimator requires a parallel trend assumption...relative outcome of group B and group A in the treatment state to trend the same way as the relative outcome of group B and group A in the control state in the absence of treatment."

Table 4: Triple diff design parallel trends test

	Dependent variable: Reservation wage
Treatment	-2.270^{***}
	(0.005)
State	-1.872^{***}
	(0.112)
Week	2.68^{***}
	(0.017)
Treatment $\times$ *State*	-0.658^{***}
	(0.005)
Treatment $\times$ *Week*	-0.075^{***}
	(0.002)
State $\times$ *Week*	0.093
	(0.055)
Treatment $\times$ *Week* $\times$ *State*	-0.00005
	(0.0001)
Constant	27.665^{***}
	(0.001)
Observations	158,455
R^2	0.132
Adjusted R^2	0.331
Note:	*p<0.1; **p<0.05; ***p<0.01

Notes: Column (1) shows the results of the parallel trends test by regressing the dependent variable on group, state, and week dummies and their interaction. Column (2) shows the results of the parallel trends test by regressing the same dependent variable on group and week dummies.

- $Post_t$ is an indicator that equals 1 for audits done at least two weeks after, and at most 6 weeks after a minimum wage increase
- $State_s$ is an indicator for a state that raised its minimum wage

In the above specification, the parameter of interest is denoted by δ, while the relevant interactions are captured by λ, η, and γ. The β terms captures the linear terms and the standard errors, captured by ϵ_{ist}

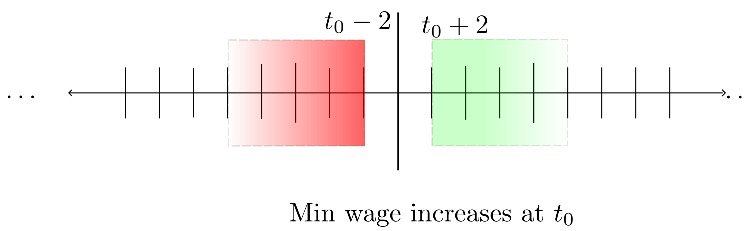

Figure 7: The figure above shows post-treatment (in green) and pre-treatment (in red); in one state with a single minimum wage increase on date t_0, I consider all unemployed with audits between 6 to 2 weeks before the minimum wage was raised as being pre-treatment cohort. Conversely I consider all unemployed with audits 2 to 6 weeks after the minimum wage was increased as being in the post-treatment cohort. The treatment group is composed of job-seekers with pre-separation wages below the median pre-separation wages for that state, during the year of the minimum wage increase (and the control group consists of job-seekers with pre-separation wages above the median pre-separation wages in the same state-year cell as the treatment group).

2.5 Threats to identification

Even with robustness checks, we may be concerned about other threats to identification, namely: violation of parallel trends, composition bias and, anticipation effects. I address the parallel trends assumption first; even though parallel trends cannot be tested directly, the event study design allows me to check for leading effects at the state level. Insignificant coefficients on the interaction term of the weeks leading up to minimum wage increase and treatment would strongly suggest the absence of pre-trends in the difference in reservation wages of the treatment and control groups in the time leading up to a minimum wage increase. In Table 5 I present the results of the event study design and show the lag terms for each event study specification; none of the $\gamma_k : k < 0$ terms are significant.

For the triple diff design I conduct a separate parallel trends test; in Table 4 I test and am unable reject the null hypothesis that parallel trends holds across states [15]. I specify a simple linear regression model in which the reservation is

[15] As before, $Treatment$ is the treatment group indicator that equals 1 if the observation has pre-separation wages below the pre-separation wages for the state-year cell they are in. $Week$ has been coded as a number in the range $[-10, 0]$ representing the number of weeks before the

regressed on the interaction of the *State*, *Week*, and *Treatment* terms. The triple interaction term is insignificant and suggests that the difference in response of treatment and control groups in the experimental state and the difference in response of treatment and control groups in the non-experimental state trend similarly.

To control for anticipation effects (in both quasi-experimental designs), I exploit the high frequency of BAM surveys and drop all observations in my sample whose date of separation falls in a two week window before or after a minimum wage. Another advantage of using this setup is that I don't have to worry about a treated state in, say 2020 being a control state in 2021, as I can look at changes around a minimum wage changes for a span of weeks, and ensure multiple treatments in a state don't overlap. Over the course of time covered by my BAM sample, I can use the event study design to analyse all 439 minimum wage experiments at the state level, and use the triple diff design to compare outcomes in states that raise their minimum wages against states that don't.

Finally, to control for the composition effect, I exploit the rich information in BAM on the composition of the unemployed. For example, if the increase in minimum wages causes relatively productive workers to enter the treatment pool, the reservation wage will mechanically rise, reflecting the higher human capital of the workers in the treatment pool. I am able to address this concern using the pre-separation wage information in BAM. Furthermore, I can control for several indicators of human capital, like age, education level, and industry of occupation. The results of this analysis prove robust to the inclusion of such controls, thus suggesting the effect of minimum wages on reservation wages are significant even after minimising composition effects.

3 Results

3.1 Event study design

The results of Equation (1) are presented below in Table 5. The parameter of interest is the interaction of the treatment dummy ($Treatment$) and the post treatment period dummy ($Post$), in the $Treatment \times Post$ line. The stacked event study estimate suggests that reservation wages rise by 7.5% on average as a result of a minimum wage increase. The results in column (1)Table 5 do not control for demographic characteristics or human capital investment, and only have pre-separation wage controls, seasonality terms, and state fixed effects.

3.2 Triple diff design

The triple diff term indicates that low-wage workers in a state that raises its minimum wage have a reservation wage that is 8.13% higher than their

minimum wage was raised. Finally, *State* is an indicator that equals 1 for all states that raise the minimum wage within 8 weeks of each other.

Table 5: Event study design

	Dependent variable: Log of reservation wage			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
Treatment	0.223***	0.003	0.025***	0.031***
	(0.013)	(0.013)	(0.006)	(0.006)
Post	0.068***	−0.022***	−0.027***	−0.019***
	(0.014)	(0.008)	(0.004)	(0.004)
Log of pre-separation wages	1.124***	0.841***	0.814***	0.818***
	(0.027)	(0.010)	(0.011)	(0.011)
$Treatment \times Post : \gamma_0$	0.075***	0.050***	0.046***	0.040***
	(0.013)	(0.005)	(0.005)	(0.005)
$Treatment \times Lag(-4) : \gamma_{-4}$	−0.001	−0.031	−0.050	−0.041
	(0.009)	(0.025)	(0.037)	(0.061)
$Treatment \times Lag(-3) : \gamma_{-3}$	−0.033	−0.004	−0.026	−0.009
	(0.024)	(0.022)	(0.021)	(0.021)
$Treatment \times Lag(-2) : \gamma_{-2}$	−0.064	0.032	0.036	0.043
	(0.045)	(0.029)	(0.029)	(0.029)
$Treatment \times Lag(1) : \gamma_1$	0.075***	0.032	0.036	0.043**
	(0.024)	(0.029)	(0.029)	(0.025)
$Treatment \times Lag(2) : \gamma_2$	0.046***	0.050***	0.059***	0.060***
	(0.006)	(0.006)	(0.006)	(0.006)
$Treatment \times Lag(3) : \gamma_3$	0.040*	0.030***	0.060***	0.068***
	(0.021)	(0.007)	(0.008)	(0.008)
$Treatment \times Lag(4) : \gamma_4$	0.038	0.038***	0.023***	0.035***
	(0.023)	(0.008)	(0.008)	(0.008)
Constant	0.472***	0.433***	0.367***	0.302***
	(0.030)	(0.040)	(0.040)	(0.042)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	109,216	108,969	108,969	108,969
R^2	0.528	0.536	0.557	0.566
Adjusted R^2	0.527	0.534	0.555	0.563
Residual Std. Error	0.177 (df = 109181)	0.174 (df = 108911)	0.170 (df = 108879)	0.169 (df = 108856)
F Statistic	6309.100*** (df = 34; 109181)	3827.300*** (df = 57; 108911)	2668.900*** (df = 89; 108879)	2192.800*** (df = 112; 108856)
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: This table presents the results of stacked diff-in-diff as specified in Equation (1). State fixed effects and seasonality terms added but not shown.

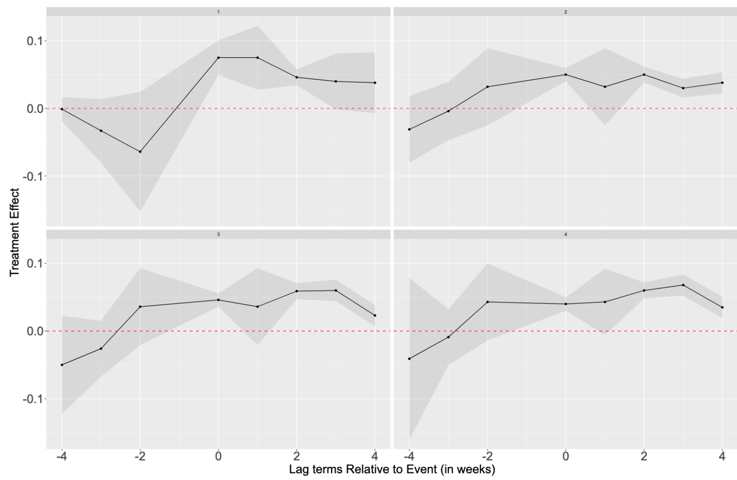

Figure 8: The above plot renders the baseline treatment effect on reservation wages by weeks before and after a minimum wage increase. These results correspond to the values in column (1) of Table 5.

unemployed peers in the same wage-bracket in states that didn't raise their minimum wages.

4 Robustness checks

This section discusses robustness checks for the two table discussed above. Broadly speaking, I redefine the treatment and control cohorts by changing the pre and post treatment window and addressing the treated vs control cohort. I present results for each robustness check with the full set of controls for each design (the event study design, and the triple diff design), comparable in column (4) in Tables 5 and 6.

4.1 State-level event study

I can panalize the date in BAM by aggregating reservation wage information at the state-level over time. This step allows me to perform an event study design comparing the reservation wages in a state that raised its minimum wage against a state that didn't, before and after the minimum wage policy went into effect. In particular, I compare the treated state (i.e. a state that raised its minimum wage) with all other states that did not raise their minimum wage 6 weeks before, or 6 weeks after the treated state did. I restrict my sample to workers with pre-separation wages below the median and estimate the model in Equation (1). Due to aggregation, I lose many of the controls used in column (4) of Table 5, but I do retain pre-separation wages. The results of the state-level event study design are tabulated in Table 7.

Table 6: Triple diff design

	Dependent variable: Log of reservation wage			
	(1)	(2)	(3)	(4)
Treatment	−0.548	−0.006	−0.003***	−0.003
	(0.0008)	(0.0009)	(0.0009)	(0.0009)
Post	0.038***	−0.003	−0.009	−0.006***
	(0.0003)	(0.0007)	(0.0007)	(0.0007)
State	0.003***	0.009***	0.0008	−0.011***
	(0.001)	(0.001)	(0.001)	(0.002)
Log of pres-separation wages	0.817***	0.783***	0.785***	0.765***
	(0.0004)	(0.0001)	(0.0001)	(0.0004)
Treatment × *Post*	0.012***	0.083***	0.068***	0.084***
	(0.001)	(0.0009)	(0.0009)	(0.001)
Treatment × *State*	0.011***	0.0058	0.00000	0.012***
	(0.001)	(0.002)	(0.002)	(0.002)
Post × *State*	0.030***	0.071***	0.017***	0.033***
	(0.004)	(0.003)	(0.003)	(0.003)
Treatment × *Post* × *State*	0.051***	0.058***	0.056***	0.057***
	(0.006)	(0.004)	(0.004)	(0.004)
Constant	2.798***	0.302***	0.307***	0.356***
	(0.001)	(0.002)	(0.002)	(0.002)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	103,905	103,979	103,977	103,977
R^2	0.673	0.686	0.688	0.693
Adjusted R^2	0.673	0.686	0.688	0.693
Note:			*p<0.1; **p<0.05; ***p<0.01	

Notes: Above are the results of the triple diff design, specified in Equation 2, with no demographic controls included (as in the results in Table 5), but with state, seasonality and pre-separation wage terms in the estimation, in column (1). In columns (2) - (4) I include demographic controls, including dummies for race categories, and education level. I also add polynomial terms for age. In the last two columns I add terms for fraction of total benefits consumed, and add dummies for two-digit NAICS codes for previous industry of occupation.

Table 7: State level event study design

	Dependent variable:			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
ExperimentalState	-0.029^{***}	-0.032^{***}	-0.033^{***}	-0.032^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Post	0.009^{***}	0.009^{***}	0.008^{***}	0.005^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wage	0.944^{***}	0.926^{***}	0.925^{***}	0.930^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
$ExperimentalState \times Post : \gamma_0$	0.059^{***}	0.059^{***}	0.057^{***}	0.051^{***}
	(0.004)	(0.004)	(0.004)	(0.004)
$ExpermentalState \times Lag(-4) : \gamma_{-4}$	-0.008^{**}	-0.007^{*}	0.006	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)
$ExperimentalState \times Lag(-3) : \gamma_{-3}$	0.026	0.016	0.016	0.017
	(0.011)	(0.010)	(0.010)	(0.011)
$ExperimentalState \times Lag(-2) : \gamma_{-2}$	0.008	0.002^{*}	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$ExperimentalState \times Lag(1); \gamma_1$	0.035^{***}	0.057^{***}	0.056^{***}	0.053^{***}
	(0.004)	(0.004)	(0.004)	(0.004)
$ExperimentalState \times Lag(2) : \gamma_2$	0.035^{***}	0.036^{***}	0.037^{***}	0.035^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
$ExperimentalState \times Lag(3)$	0.017^{***}	0.009^{**}	0.012^{***}	0.018^{***}
	(0.004)	(0.004)	(0.004)	(0.004)
$ExperimentalState \times Lag(4)$	0.016^{***}	0.016^{***}	0.016^{***}	0.016^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Constant	-0.007^{*}	0.074^{***}	0.072^{***}	0.053^{***}
	(0.004)	(0.007)	(0.007)	(0.007)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	109,216	109,216	108,143	108,143
Adjusted R^2	0.500	0.511	0.512	0.519

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: State fixed effects and seasonality terms included but not shown.

4.2 Selection bias

Regarding the issue of selection bias, we might be concerned that the minimum wage increase induces selection of relatively less productive workers into unemployment. For example, say rising wage costs following a minimum wage increase causes the unemployed pool to be concentrated with workers whose pre-separation wage is closer to the minimum wage. These workers would also have a relatively lower reservation wage. This change in composition will introduce a downward bias on the estimated treatment effect. In Table 8 we can see evidence of this happening. With the inclusion of pre-separation wage control, the magnitude of the treatment effect under the stacked event study design jumps up.

Therefore I include the pre-separation wages in my preferred estimates in both the event study design and the triple diff design. The effect of controlling for pre-separation wages in the event study design can be seen by comparing columns (1) and (2) in Tables 8, which compares the model in Equation (1) without and with pre-separation controls respectively. The coefficient on the pre-separation wage term is significant, and its inclusion in the model is associated with a 229.09% increase in treatment magnitude (from 0.023 to 0.075) and a 39.47% increase in the R-squared (from 36.5%, to 67.3%).

Similarly, in Table 9, I compare the exclusion and inclusion of pre-separation wage controls in Equation (2) in columns (1) and (2) of Table 9, respectively. This time the treatment effect (corresponding to δ in Equation (2)) rises by 292.31% (from 0.013 to 0.051) and the R-squared rises by 84.93% (from 36.6% to 67.3%).

4.3 Treatment effect by minimum wage increase magnitude

In this section I show the results of the event study design and triple diff design after disaggregating the 439 minimum wage increases into quartiles.

As we might predict, the treatment response rises with the magnitude of minimum wage increase, as reflected in the results in Tables 10 and 11. The results of the event study design in particular, show that the reservation wage response is both stronger and more persistent in the fourth quartile of minimum wage increases. In contrast, the first quartile of minimum wage increases is associated with a small rise in reservation wages, which quickly falls off and becomes insignificant. These results clearly demonstrate a robust causal effect of minimum wage increases on reservation wages.

4.4 Alternate pre and post treatment windows

The results in Tables 12 and 13 further attempt to address composition bias by redefining the pre and post treatment windows. In particular, for a given state that raises its minimum wage on a given date, I define the pre-treatment group as the set of job-seekers who are interviewed at least 2 weeks before the

Table 8: Event study design without and with pre-separation wage controls

	Dependent variable: Log of reservation wages	
	(1)	(2)
Treatment	0.263***	0.223***
	(0.007)	(0.013)
Post	0.011**	0.068***
	(0.005)	(0.014)
Log of pre-separation wages		1.124***
		(0.027)
Treatment × *Post*	0.023***	0.075***
	(0.008)	(0.013)
Constant	2.377***	0.472***
	(0.014)	(0.030)
Observations	109,216	109,216
R^2	0.383	0.536
Adjusted R^2	0.380	0.534
Note:		*p<0.1; **p<0.05; ***p<0.01

Notes: The model tabulated in column (1) doesn't include controls for pre-separation wages and the corresponding treatment effect is considerably smaller than the preferred estimate reported in column (2) (which is the same model reported in Table 5, column (1).

Table 9: Triple diff design without and with pre-separation wage controls

	Dependent variable:Log of reservation wages	
	(1)	(2)
Treatment	-0.523^{***}	-0.548^{***}
	(0.0005)	(0.0008)
Post	0.022^{***}	0.038^{***}
	(0.0005)	(0.0003)
ExperimentalState	0.024^{***}	0.003^{***}
	(0.001)	(0.001)
Log of pre-separation wages		0.817^{***}
		(0.0004)
Treatment × *Post*	0.010^{***}	0.012^{***}
	(0.001)	(0.001)
Treatment × *ExperimentalState*	0.002	0.011^{***}
	(0.001)	(0.001)
Post × *ExperimentalState*	0.055^{***}	0.030^{***}
	(0.002)	(0.004)
Treatment × *Post* × *ExperimentalState*	0.013^{***}	0.051^{***}
	(0.003)	(0.006)
Constant	2.583^{***}	2.798^{***}
	(0.002)	(0.001)
Observations	103,905	103,905
R^2	0.365	0.673
Adjusted R^2	0.365	0.673
Note:		*p<0.1; **p<0.05; ***p<0.01

Notes: The above table compares the results of the triple diff design without the inclusion of pre-separation wage controls (column (1)) and with the same controls added (column(2), compare with column (1) in Table 6)

Table 10: Stacked event study design by minimum wage increase magnitude

	Dependent variable:			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
Treatment	0.033***	0.016***	0.022***	0.041***
	(0.006)	(0.004)	(0.006)	(0.011)
Post	0.008	0.014***	0.029***	0.035***
	(0.005)	(0.003)	(0.006)	(0.007)
Log of pre-separation wages	0.698***	0.649***	0.708***	0.742***
	(0.007)	(0.005)	(0.007)	(0.011)
Treatment × *Post*	0.011*	0.015***	0.049***	0.061***
	(0.006)	(0.004)	(0.006)	(0.005)
$Treatment \times Lag(-4) : \gamma_{-4}$	0.006	−0.014	0.031	0.026
	(0.023)	(0.015)	(0.022)	(0.032)
$Treatment \times Lag(-3) : \gamma_{-3}$	0.029	0.026	0.081**	0.057
	(0.034)	(0.017)	(0.033)	(0.076)
$Treatment \times Lag(-2) : \gamma_{-2}$	−0.076*	−0.015	−0.079	0.0002
	(0.041)	(0.021)	(0.073)	(0.032)
$Treatment \times Lag(1) : \gamma_{1}$	0.076***	0.058***	0.043***	0.046***
	(0.021)	(0.019)	(0.015)	(0.013)
$Treatment \times Lag(2) : \gamma_{2}$	0.032	0.036*	0.047***	0.056***
	(0.025)	(0.019)	(0.015)	(0.012)
$Treatment \times Lag(3) : \gamma_{3}$	0.015	0.031	0.040*	0.046***
	(0.025)	(0.021)	(0.022)	(0.013)
$Treatment \times Lag(4) : \gamma_{4}$	0.028	0.018	0.048**	0.041**
	(0.064)	(0.017)	(0.024)	(0.019)
Constant	0.533***	0.850***	0.464***	0.162***
	(0.033)	(0.025)	(0.047)	(0.053)
Observations	23,723	51,884	19,204	8,302
Adjusted R^2	0.683	0.744	0.766	0.750
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: The above table estimates the model specified in Equation (1), corresponding to column (4) in Table 5, with a full set of controls. Controls include a polynomial term in age, sex, education, race dummies, controls for number of dependents, fraction of benefits consumed, industry of previous occupation.

Table 11: Triple diff design by minimum wage increase quartile

	Dependent variable:			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
Group	0.031***	0.029***	0.035***	0.041***
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.026***	0.018***	0.019***	0.023***
	(0.001)	(0.001)	(0.001)	(0.001)
ExperiementState	0.011***	0.013***	0.025***	0.032***
	(0.001)	(0.0000)	(0.007)	(0.003)
Log of pre-separation wages	0.711***	0.707***	0.714***	0.721***
	(0.001)	(0.001)	(0.001)	(0.001)
Group × *Post*	0.031***	0.012***	0.021***	0.024***
	(0.001)	(0.001)	(0.001)	(0.001)
Group × *ExperimentState*	0.011***	0.013***	0.031***	0.038***
	(0.001)	(0.001)	(0.004)	(0.006)
Post × *ExperimentState*	0.022***	0.025***	0.021***	0.030***
	(0.005)	(0.002)	(0.004)	(0.003)
Group × *Post* × *ExperimentState*	0.032***	0.030***	0.042***	0.058***
	(0.007)	(0.002)	(0.005)	(0.005)
Constant	0.397***	0.407***	0.380***	0.408***
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	23,701	51,863	19,196	8,302
Adjusted R^2	0.734	0.731	0.732	0.738
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: The above table shows the results of the model specified in Equation (2), corresponding to column (4) in Table 6, with the corresponding full set of controls (age and age squared, number of benefits, fraction of benefits consumed) and dummies (education level, race, gender, and previous industry of occupation).

minimum wage increase goes into effect and the post-treatment group as the pool of job-seekers whose spell beings at least two weeks after. This setup ensures that nobody in the pre-treatment group is interviewed after the minimum wage increase. In order to further ensure the two groups are comparable, I limit both groups to unemployed with spell durations no more than 10 weeks long. As before, I limit anticipation effects by throwing away all observations interviewed in a two week window around the minimum wage increase.

Using these definitions of pre and post treatment periods, I re-estimate Equation (1). I also restrict spell durations to 6 weeks or fewer to maintain comparability between the treatment and controls groups. Compared to the results in Table 5, the treatment magnitude (γ_0) is considerably larger and so is the standard deviation (reflecting the smaller sample due to the added restrictions on pre and post windows, and spell duration), but still significant at $\alpha = 5\%$.

In Table 13 I compare the log of reservation wages of treatment and control groups in a triple diff design, with the two groups completely isolated. I define the pre and post treatment window using the dates of audits and dates of separation, similar to the definitions used in Table 12, and restrict the sample to workers with spell durations up to 6 weeks. Since it is not possible to get reservation wage information for any worker before the date of separation, I can "isolate" the pre treatment cohort from the post treatment cohort by using a threshold defined by the date of audit (i.e. interview) for the former and date of separation threshold for the latter.

Again, the magnitude of the treatment effect is larger, but difference between the results tabulated in Table 6 and Table 13 is not as dramatic as in the stacked event study case (compare Tables 5 and12).

4.5 Treatment and control groups defined relative to minimum wage

In this section I define the treated and cohort group relative to the state minimum wage. In particular, I define the treatment group on the basis of the pre-separation wage relative to the state minimum wage and re-estimate the models specified in Equations (1) and (2). In Table 14I tabulate the results of the event study design. In Table 15 I tabulate the results of the triple diff design. In both tables, each column defines the treated group as job-seekers with pre-separation wages up to a multiple of the state minimum wage, starting with 1.15 times the minimum wage in column (1), up to 1.85 times the minimum wage in column (8).

As we might expect, unemployed workers looking for work with pre-separation wages near the minimum wage have the highest response and as we move rightwards in the pre-separation wage distribution, the treatment effect atrophies in magnitude and significance. Interestingly the highest treatment magnitude is seen when the treatment group is defined as unemployed workers with pre-separation wages up to 125% of the binding minimum wage.

Table 12: Event study design with alternate pre and post treatment windows

	Dependent variable:			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
Treatment	-0.008	-0.020^{**}	-0.026^{***}	-0.032^{***}
	(0.009)	(0.009)	(0.009)	(0.009)
Post	-0.037^{***}	-0.033^{***}	-0.035^{***}	-0.033^{***}
	(0.008)	(0.008)	(0.008)	(0.008)
Log of pre-separation wages	0.836^{***}	0.812^{***}	0.795^{***}	0.790^{***}
	(0.019)	(0.018)	(0.019)	(0.019)
$Treatment \times Post; \gamma_0$	0.233^{***}	0.195^{***}	0.194^{***}	0.164^{***}
	(0.042)	(0.041)	(0.041)	(0.045)
$Treatment \times Lag(-4) : \gamma_{-4}$	0.020	0.009	0.006	0.014
	(0.016)	(0.016)	(0.016)	(0.016)
$Treatment \times Lag(-3) : \gamma_{-3}$	0.051	0.066	0.071	0.081
	(0.061)	(0.061)	(0.061)	(0.061)
$Treatment \times Lag(-2) : \gamma_{-2}$	0.072	0.091	0.092	0.090
	(0.059)	(0.059)	(0.059)	(0.058)
$Treatment \times Lag(1) : \gamma_1$	0.036^{***}	0.039^{***}	0.040^{***}	0.033^{***}
	(0.009)	(0.009)	(0.009)	(0.009)
$Treatment \times Lag(2) : \gamma_2$	0.107^{***}	0.126^{***}	0.099^{***}	0.103^{***}
	(0.029)	(0.028)	(0.029)	(0.028)
$Treatment \times Lag(3) : \gamma_3$	0.030^{**}	0.001	0.030^{**}	0.035^{**}
	(0.013)	(0.013)	(0.014)	(0.014)
$Treatment \times Lag(4) : \gamma_4$	0.060	0.070^{*}	0.066	0.093^{**}
	(0.042)	(0.041)	(0.042)	(0.041)
Constant	0.233^{***}	1.848^{***}	1.862^{***}	2.155^{***}
	(0.051)	(0.206)	(0.206)	(0.207)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	45,708	45,708	45,708	45,708
R^2	0.562	0.595	0.596	0.617
Adjusted R^2	0.558	0.590	0.591	0.611
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: The above table uses an alternate definition of pre and post treatment groups.

Table 13: Triple diff design with alternate pre and post treatment windows

	Dependent variable:			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
Treatment	0.025***	0.024***	0.024***	0.012***
	(0.0005)	(0.0005)	(0.0005)	(0.0004)
Post	0.022***	0.026***	0.025***	0.009***
	(0.0004)	(0.0004)	(0.0004)	(0.0003)
ExperimentalState	0.017***	0.016***	0.016***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)
Log of pre-separation wages	0.826***	0.798***	0.796***	0.761***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Treatment × *Post*	0.019***	0.022***	0.023***	0.030***
	(0.0005)	(0.0005)	(0.0005)	(0.0004)
Treatment × *ExperimentalState*	0.007***	0.007***	0.007***	0.006***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Post × *ExperimentalState*	0.014*	0.013***	0.013***	0.019***
	(0.003)	(0.002)	(0.001)	(0.001)
Treatment × *Post* × *ExperimentalState*	0.063***	0.065***	0.065***	0.063***
	(0.004)	(0.005)	(0.005)	(0.005)
Constant	0.279***	0.332***	0.342***	0.373***
	(0.002)	(0.007)	(0.007)	(0.007)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	68,270	68,270	68,270	68270
R^2	0.693	0.707	0.708	0.711
Adjusted R^2	0.693	0.707	0.708	0.711
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: The above table redefines the pre and post window to isolate the pre-treatment and post-treatment cohort in the triple diff design.

Table 14: Event study design with treatment defined relative to minimum wage

	Dependent variable: Log of reservation wages							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	−0.057***	−0.088***	−0.096***	−0.098***	−0.064***	−0.059***	−0.032***	−0.027***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Post	0.001	0.009**	0.022***	0.017***	0.017***	0.019***	0.015***	0.008***
	(0.006)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Log of pre-separation wages	0.799***	0.874***	0.911***	0.910***	0.851***	0.838***	0.780***	0.761***
	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
Age	−0.002	−0.002*	−0.003*	−0.004***	−0.002*	−0.003**	−0.002	−0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age squared	0.00002	0.00002	0.00003*	0.00005**	0.00003*	0.00003*	0.00003	0.00004**
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
High school	0.041***	0.043***	0.043***	0.045***	0.042***	0.039***	0.042***	0.040***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
College	0.046***	0.050***	0.046***	0.050***	0.049***	0.046***	0.052***	0.048***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Female	0.020***	0.018***	0.013***	0.016***	0.018***	0.018***	0.018***	0.019***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Black	−0.001	−0.006	−0.004	−0.004	−0.004	−0.006	−0.001	−0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Asian	−0.004	−0.006	−0.013	−0.025**	−0.014	−0.010	−0.011	−0.009
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
$Treatment \times Post : \gamma_0$	0.033***	0.088***	0.056***	0.036***	0.016***	0.019***	0.013***	0.002
	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
$Treatment \times Lag(-4) : \gamma_{-4}$	−0.018	−0.009	−0.013	0.004	0.048	0.045	0.047	0.043
	(0.021)	(0.023)	(0.025)	(0.027)	(0.060)	(0.060)	(0.061)	(0.066)
$Treatment \times Lag(-3) : \gamma_{-3}$	−0.029	−0.034	−0.006	−0.051	0.037	0.017	0.013	0.018
	(0.019)	(0.021)	(0.024)	(0.023)	(0.026)	(0.029)	(0.037)	(0.047)
$Treatment \times Lag(-2) : \gamma_{-2}$	−0.042	−0.048*	−0.041	−0.030	−0.018	−0.006	−0.044	−0.046
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.032)	(0.032)
$Treatment \times Lag(1) : \gamma_1$	0.071**	0.062**	0.106**	0.098**	0.067	0.051	0.016	0.005
	(0.032)	(0.036)	(0.043)	(0.043)	(0.060)	(0.061)	(0.065)	(0.063)
$Treatment \times Lag(2) : \gamma_2$	0.073***	0.075***	0.057***	0.069***	0.083***	0.015	0.019	0.009
	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.013)	(0.017)	(0.020)
$Treatment \times Lag(3) : \gamma_3$	0.030***	0.041***	0.020	0.028	0.019	0.015	0.014	0.015
	(0.012)	(0.012)	(0.021)	(0.020)	(0.021)	(0.021)	(0.031)	(0.032)
$Treatment \times Lag(4) : \gamma_4$	0.021**	0.018*	0.003	0.003	0.007	0.014	0.090	0.023
	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)	(0.055)	(0.023)
Constant	0.447***	0.264***	0.163***	0.158***	0.247***	0.282***	0.402***	0.479***
	(0.034)	(0.035)	(0.035)	(0.035)	(0.035)	(0.034)	(0.034)	(0.033)
Observations	108,969	108,969	108,969	108,969	108,969	108,969	108,969	108,969
R^2	0.567	0.574	0.579	0.582	0.578	0.578	0.570	0.565
Adjusted R^2	0.564	0.571	0.577	0.580	0.575	0.575	0.567	0.563
Note:						*p<0.1; **p<0.05; ***p<0.01		

Notes: Results of the event study design with full set of controls with treatment and control groups defined by pre-separation wages relative to state minimum wages. In column (1), the treated group is defined as the job-seekers with pre-separation wages less than or equal to 115% of the state minimum wage (and the control group consists of those earnings more). In columns (2) it is defined as 125% of the minimum wage, and so on, till 185% of the minimum wage in columns (8). The pre-and post period are defined on the basis of the date of separation falling 6 to 2 weeks before, and after the minimum wage increase (as in the preferred estimates reported in Section 3).

Table 15: Triple diff design with treatment group defined relative to minimum wage

	Dependent variable: Log of resevation wages							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.083***	0.079***	0.073***	0.075***	0.071***	0.068***	0.069***	0.062***
	(0.001)	(0.001)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Post	0.009***	0.009***	0.009***	0.008***	0.008***	0.008***	0.007***	0.008***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
State	0.006***	0.006***	0.005***	0.006***	0.006***	0.007***	0.007***	0.005***
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.001)
Age	0.001***	0.001***	0.001***	0.001***	0.001***	0.002***	0.002***	0.002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age squared	0.00001***	0.00000***	0.00000***	0.00000*	0.00000**	0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
High school	0.023***	0.024***	0.024***	0.024***	0.024***	0.024***	0.024***	0.022***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
College	0.082***	0.083***	0.083***	0.083***	0.084***	0.083***	0.083***	0.082***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Female	−0.019***	−0.019***	−0.020***	−0.020***	−0.020***	−0.020***	−0.019***	−0.019***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Black	−0.022***	−0.022***	−0.023***	−0.023***	−0.023***	−0.024***	−0.024***	−0.024***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Asian	0.033***	0.033***	0.033***	0.032***	0.032***	0.031***	0.031***	0.032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Fraction of benefits consumed	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.0003***	−0.0003***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Treatment × Post	0.004***	0.004***	0.003***	0.002***	0.002***	0.004***	0.005***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Treatment × State	0.011***	0.009***	0.002	0.005***	0.004***	0.007***	0.005***	−0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post × State	0.017***	0.016***	0.016***	0.017***	0.020***	0.019***	0.021***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × Post × State	0.059***	0.049***	0.049***	0.030***	0.025**	0.012***	0.012***	0.002
	(0.003)	(0.003)	(0.002)	(0.002)	(0.010)	(0.002)	(0.002)	(0.002)
Constant	0.365***	0.341***	0.329***	0.307***	0.286***	0.272***	0.253***	0.263***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	103,977	103,977	103,977	103,977	103,977	103,977	103,977	103,977
R^2	0.690	0.690	0.690	0.691	0.691	0.691	0.691	0.690
Adjusted R^2	0.690	0.690	0.690	0.691	0.691	0.691	0.691	0.690
Residual Std. Error (df = 103819)	0.250	0.250	0.250	0.250	0.250	0.250	0.250	0.250
Note:								*p<0.1; **p<0.05; ***p<0.01

Notes: Results of the triple diff design with full set of controls with the treatment groups defined by pre-separation wages relative to state minimum wages. In column (1), the treated group is defined by the pre-separation wages being 115% of the state minimum wage. In each succeeding column I define move the threshold in 10% increments, with column (8) having a threshold of 185% of the minimum wage (i.e. the treatment group consists of job-seekers earning 1.85 times the minimum wage, or less).

In Table 15, I tabulate the results of the triple diff design with treatment and control groups defined relative to the binding minimum wage. In column (1) the treatment group consists of workers earning up to 115% of the minimum wage and the control group consists of workers earning more than 115% of the minimum wage. This threshold is moved in increments of 10% from columns (2) though to column (8), with treatment group consisting of workers with pre-separation wages up to 185% of the minimum wage and the control group consisting of workers with higher pre-separation wages. Once again, as we move to the right of the pre-separation wage distribution, treatment effect weakens in magnitude and significance.

4.6 No indexer states

Since 2016, ten states have started indexing minimum wages to inflation. I control for this effect, by re-estimating Equations 1 and 2 after dropping the indexer states from my sample. These results are presented in Tables 16 and 17. Since the minimum wage increase would be fully expected in the indexing states, their exclusion from the sample causes the reservation wage response to be higher than when compared to the results in Table 5. Once again, the triple diff design's results have a larger magnitude following the exclusion of the indexer states. Compared to the results in Table 6, the treatment magnitude ranges from 34% to 40% larger than the model with the indexer states included in sample (compare with Table 6).

4.7 Treatment effect heterogeneity by demographic characteristics

4.7.1 Males vs females

In this section I discuss the heterogeneous effects of minimum wages on reservation wages along various demographic controls. In Tables 18 and 19, the results of estimating the event study design is presented separately for females and males, respectively.

The results of Tables 18 and 19 highlight the difference in reservation wage response of females and males respectively. In particular, the response among males is larger than the females' and remains relatively stable in the period immediately following a minimum wage increase.

In Tables 20 and 21 I present the results of the triple diff design for females and males respectively. In the triple diff design, we again see the treatment magnitude is larger for men than it is for women.

4.7.2 Over 35 and under 35

Since the young are disproportionately likely to work minimum wage jobs, they are also disproportionately likely to be impacted by minimum wage increases. In

Table 16: Event study design without indexer states

	Dependent variable: Log of reservation wages			
	(1)	(2)	(3)	(4)
Treatment	0.035^{***}	0.035^{***}	0.035^{***}	0.038^{***}
	(0.008)	(0.008)	(0.008)	(0.008)
Post	-0.019^{***}	-0.019^{***}	-0.027^{***}	-0.024^{***}
	(0.005)	(0.005)	(0.005)	(0.005)
Log of pre-separation wages	0.839^{***}	0.839^{***}	0.803^{***}	0.808^{***}
	(0.015)	(0.015)	(0.015)	(0.015)
$Treatment \times Post : \gamma_0$	0.055^{***}	0.055^{***}	0.050^{***}	0.046^{***}
	(0.007)	(0.007)	(0.007)	(0.007)
$Treatment \times Lag(-4) : \gamma_{-4}$	-0.001	-0.001	-0.005	-0.001
	(0.009)	(0.009)	(0.009)	(0.009)
$Treatment \times Lag(-3) : \gamma_{-3}$	0.004	0.004	-0.002	0.0001
	(0.030)	(0.030)	(0.029)	(0.029)
$Treatment \times Lag(-2) : \gamma_{-2}$	0.003	0.003	0.007	0.011
	(0.010)	(0.010)	(0.010)	(0.010)
$Treatment \times Lag(1) : \gamma_1$	0.049^{*}	0.049^{*}	0.077^{***}	0.100^{***}
	(0.027)	(0.027)	(0.026)	(0.026)
$Treatment \times Lag(2) : \gamma_2$	0.091^{***}	0.091^{***}	0.078^{***}	0.092^{***}
	(0.026)	(0.026)	(0.025)	(0.025)
$Treatment \times Lag(3) : \gamma_3$	0.054^{**}	0.054^{**}	0.031	0.063^{**}
	(0.026)	(0.026)	(0.026)	(0.026)
$Treatment \times Lag(4) : \gamma_4$	0.044^{***}	0.044^{***}	0.034^{**}	0.045^{***}
	(0.016)	(0.016)	(0.016)	(0.016)
Constant	0.188^{***}	0.188^{***}	0.429^{***}	0.408^{***}
	(0.041)	(0.041)	(0.052)	(0.055)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	81,320	81,320	81,137	81,137
Adjusted R^2	0.541	0.541	0.569	0.579
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: Columns (1) though (4) correspond to their counterparts in Table 5; the results below exclude minimum wage indexer states from 2016 on (which is when states started indexing the minimum wages)

Table 17: Triple diff design without indexer states

	Dependent variable:			
	Log of reservation wages			
	(1)	(2)	(3)	(4)
Treatment	0.103***	0.0121***	0.112***	0.110***
	(0.001)	(0.001)	(0.001)	(0.0005)
Post	0.013***	0.012***	0.012***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
State	0.002**	0.002***	0.001*	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.799***	0.763***	0.762***	0.742***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Treatment × *Post*	0.015***	0.014***	0.013***	0.011***
	(0.001)	(0.0005)	(0.0005)	(0.0005)
Treatment × *State*	−0.006***	−0.006***	−0.005***	−0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Post × *State*	0.010***	0.005***	0.006***	0.005***
	(0.002)	(0.002)	(0.002)	(0.002)
Treatment × *Post* × *State*	0.071***	0.061***	0.061***	0.062***
	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.343***	0.360***	0.369***	0.414***
	(0.002)	(0.003)	(0.003)	(0.003)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	81,320	81,320	81,137	81,137
Adjusted R^2	0.671	0.687	0.687	0.692
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: The results below should be compared with the results in Table 6; the model estimated below excludes minimum wage indexer states from 2016 (when minimum wage indexing started).

Table 18: Females only event study design

	Dependent variable: Log of reservation wages			
	Female only			
	(1)	(2)	(3)	(4)
Treatment	-0.281^{***}	0.038^{***}	0.040^{***}	0.042^{***}
	(0.007)	(0.008)	(0.008)	(0.008)
Post	0.006	0.019^{***}	0.013^{**}	0.026^{***}
	(0.007)	(0.006)	(0.006)	(0.006)
Log of pre-separation wage	0.891^{***}	0.856^{***}	0.859^{***}	0.840^{***}
	(0.017)	(0.017)	(0.016)	(0.016)
$Treatment \times Post$	0.022^{**}	0.025^{***}	0.026^{***}	0.017^{**}
	(0.009)	(0.008)	(0.008)	(0.008)
$Treatment \times Lag(-4)$	0.068	-0.019	-0.025	0.028
	(0.061)	(0.050)	(0.049)	(0.048)
$Treatment \times Lag(-3)$	-0.116^{***}	0.016	0.024	-0.006
	(0.029)	(0.024)	(0.024)	(0.024)
$Treatment \times Lag(-2)$	-0.016	-0.020	0.0003	0.014
	(0.024)	(0.020)	(0.020)	(0.020)
$Treatment \times Lag(1)$	0.108^{**}	0.200^{***}	0.227^{***}	0.237^{***}
	(0.049)	(0.041)	(0.040)	(0.040)
$Treatment \times Lag(2)$	0.088^{***}	0.108^{***}	0.114^{***}	0.120^{***}
	(0.015)	(0.014)	(0.014)	(0.013)
$Treatment \times Lag(3)$	0.096^{***}	0.044^{***}	0.048^{***}	0.063^{***}
	(0.019)	(0.016)	(0.016)	(0.015)
$Treatment \times Lag(4)$	0.023^{*}	0.024^{*}	0.026^{***}	0.026^{***}
	(0.014)	(0.013)	(0.013)	(0.013)
Constant	2.427^{***}	0.318^{***}	0.344^{***}	0.429^{***}
	(0.018)	(0.058)	(0.058)	(0.061)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	55,765	55,740	55,601	55,601
R^2	0.453	0.634	0.643	0.669
Adjusted R^2	0.449	0.630	0.639	0.664
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: State FE and seasonality terms included but not shown.

Table 19: Males only event study design

	Dependent variable: Log of reservation wages			
	Males only			
	(1)	(2)	(3)	(4)
Treatment	0.012	0.010	0.013*	0.024***
	(0.007)	(0.007)	(0.007)	(0.007)
Post	−0.043***	−0.048***	−0.051***	−0.047***
	(0.005)	(0.005)	(0.005)	(0.005)
Log of pre-separation wages	0.793***	0.751***	0.764***	0.785***
	(0.015)	(0.015)	(0.015)	(0.015)
Treatment × *Post*	0.055***	0.049***	0.053***	0.051***
	(0.007)	(0.007)	(0.007)	(0.007)
Treatment × *Lag*(−4)	0.007	0.006	−0.004	−0.012
	(0.013)	(0.014)	(0.014)	(0.014)
Treatment × *Lag*(−3)	−0.039	−0.060	−0.042	−0.013
	(0.039)	(0.038)	(0.038)	(0.038)
Treatment × *Lag*(−2)	0.033	−0.001	0.011	0.011
	(0.029)	(0.029)	(0.029)	(0.029)
Treatment × *Lag*(1)	0.087***	0.104***	0.097***	0.088***
	(0.024)	(0.023)	(0.023)	(0.023)
Treatment × *Lag*(2)	0.037*	0.060***	0.059***	0.044**
	(0.022)	(0.022)	(0.021)	(0.021)
Treatment × *Lag*(3)	0.088***	0.066***	0.063***	0.066***
	(0.017)	(0.018)	(0.017)	(0.017)
Treatment × *Lag*(4)	0.029***	0.008	0.052***	0.070***
	(0.005)	(0.007)	(0.007)	(0.007)
Constant	0.425***	0.479***	0.478***	0.326***
	(0.040)	(0.053)	(0.053)	(0.056)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	53,451	53,369	53,369	53,369
Adjusted R^2	0.546	0.557	0.562	0.572

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 20: Female only triple diff design

	Dependent variable:Log of reservation wages			
	Females only			
	(1)	(2)	(3)	(4)
Treatment	0.017***	0.017***	0.018***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.018***	0.018***	0.018***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)
State	−0.015***	−0.009***	−0.009***	−0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.802***	0.761***	0.760***	0.750***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *Post*	0.003***	0.001**	0.001**	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *State*	0.022***	0.017***	0.017***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)
Post × *State*	0.015***	0.012***	0.012***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
Treatment × *Post* × *State*	0.057***	0.056***	0.056***	0.055***
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.294***	0.270***	0.281***	0.323***
	(0.002)	(0.003)	(0.003)	(0.003)
Age polynomial	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	55,767	55,707	55,531	55,531
Adjusted R^2	0.698	0.710	0.710	0.713
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: State FE and seasonality terms included but not shown

Table 21: Males only triple diff design

	Dependent variable:Log of reservation wages			
	Males only			
	(1)	(2)	(3)	(4)
Treatment	0.015***	0.011***	0.011***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.021***	0.021***	0.020***	0.019***
	(0.001)	(0.001)	(0.001)	(0.001)
State	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.816***	0.785***	0.784***	0.764***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *Post*	0.009***	0.008***	0.008***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *State*	0.008***	0.006***	0.005***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Post × *State*	0.001	0.010***	0.010***	0.012***
	(0.002)	(0.002)	(0.002)	(0.002)
Treatment × *Post* × *State*	0.072***	0.068***	0.068***	0.069***
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.323***	0.347***	0.354***	0.402***
	(0.002)	(0.003)	(0.003)	(0.003)
Age polynomial	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	53,131	53,024	53,024	53,024
Adjusted R^2	0.649	0.664	0.664	0.668

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

this section I present the of the two quasi experimental designs after partitioning my sample by age. In particular, I partition my sample in unemployed workers 35 years old or younger, and those older than 35. In Tables 22 and 23 I present the results of the event study design for the over 35 and under 35 cohorts respectively.

The results in Tables 22 and 23 broadly confirm a larger treatment magnitude among younger workers. Since the sample has been restricted to workers aged 24-55, the sample size of the model in Table 23 is smaller than the one in Table 22. Despite this, the response of young workers to minimum wage increases is larger and remains larger than the response of their older peers in the period following the minimum wage increase.

In Tables 24 and 25 I present the results of the over 35 and under 35 cohorts for the triple diff design respectively.

Again, the results in Table 24 and 25 show us that young workers are more sensitive to minimum wage increases than their older peers, but difference in treatment magnitude is smaller in the triple diff design regime, compared to the stacked event study design regime.

4.7.3 By education level

In this section I partition my sample by education level. In Tables 26-28 I estimate the stacked event study design for unemployed with less than high school qualifications, high school graduates and college graduates respectively.

In Tables 26- 28 we can compare the treatment effect of workers without a high school diploma, with a high school diploma, and college graduates in an event study design. The response of the college graduates falls off quickly, relative to the less-than-high-school and high-school graduates cohort. The latter two cohorts have a comparable response.

In Tables 29-31 I present the results of the triple diff design for unemployed workers without a GED, high school graduates, and college graduates separately.

The results in Tables 29-31 allow us to compare the results of the triple diff design(s) for workers without a high school diploma, high school graduates, college graduates. The treatment effect for all three cohorts is comparable, and unlike the stacked event study design, the three cohorts have a comparable treatment magnitude.

4.8 Treatment effect by labour market tightness

We might speculate that labour market tightness affects treatment magnitude; in a labour market with more vacancies per unemployed, a minimum wage increase might make reservation wages respond more strongly.

Table 22: Over 35 only stacked event study design

	Dependent variable:Log of reservation wages			
	Age > 35			
	(1)	(2)	(3)	(4)
Treatment	0.019**	0.011	0.012	0.009
	(0.007)	(0.007)	(0.007)	(0.007)
Post	−0.0003	−0.007	−0.010**	−0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Log of pre-separation wages	0.803***	0.792***	0.787***	0.782***
	(0.015)	(0.015)	(0.014)	(0.014)
$Treatment \times Post$	0.053***	0.072***	0.063***	0.077***
	(0.020)	(0.020)	(0.020)	(0.020)
$Treatment \times Lag(-4)$	−0.034	−0.042	−0.077	−0.096**
	(0.048)	(0.048)	(0.047)	(0.048)
$Treatment \times Lag(-3)$	0.005	0.012*	0.014**	0.008
	(0.007)	(0.007)	(0.007)	(0.007)
$Treatment \times Lag(-2)$	−0.007	−0.009	−0.005	−0.002
	(0.019)	(0.019)	(0.019)	(0.019)
$Treatment \times Lag(1)$	0.083**	0.059*	0.065*	0.076**
	(0.035)	(0.035)	(0.035)	(0.034)
$Treatment \times Lag(2)$	0.066***	0.076***	0.065***	0.059***
	(0.014)	(0.014)	(0.014)	(0.014)
$Treatment \times Lag(3)$	0.049***	0.055***	0.047***	0.040***
	(0.015)	(0.015)	(0.015)	(0.015)
$Treatment \times Lag(4)$	0.026	0.020	0.024	0.026
	(0.046)	(0.046)	(0.046)	(0.045)
Constant	0.359***	1.136***	1.154***	0.995***
	(0.040)	(0.117)	(0.116)	(0.117)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	59,630	59,630	59,630	59,630
Adjusted R^2	0.552	0.561	0.568	0.579
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 23: 35 year old or younger workers only

	Dependent variable:Log of reservation wages			
	Age $\leq$ 35			
	(1)	(2)	(3)	(4)
Treatment	0.053***	0.044***	0.046***	0.064***
	(0.008)	(0.008)	(0.008)	(0.008)
Post	−0.044***	−0.057***	−0.059***	−0.043***
	(0.006)	(0.006)	(0.006)	(0.006)
Log of pre-separation wages	0.896***	0.866***	0.875***	0.900***
	(0.018)	(0.018)	(0.018)	(0.018)
Treatment × *Post*	0.079***	0.082***	0.083***	0.066***
	(0.008)	(0.008)	(0.008)	(0.008)
Treatment × *Lag*(−4)	−0.036	−0.029	−0.029	−0.030
	(0.025)	(0.025)	(0.025)	(0.024)
Treatment × *Lag*(−3)	−0.044	−0.052*	−0.053*	−0.015
	(0.030)	(0.029)	(0.029)	(0.029)
Treatment × *Lag*(−2)	0.009	0.010	−0.001	0.021
	(0.019)	(0.019)	(0.020)	(0.020)
Treatment × *Lag*(1)	0.085***	0.078***	0.077***	0.052**
	(0.027)	(0.026)	(0.026)	(0.026)
Treatment × *Lag*(2)	0.063***	0.081***	0.085***	0.088***
	(0.023)	(0.023)	(0.023)	(0.023)
Treatment × *Lag*(3)	0.048**	0.072***	0.063***	0.083***
	(0.020)	(0.020)	(0.020)	(0.020)
Treatment × *Lag*(4)	0.052**	0.056***	0.051**	0.057***
	(0.028)	(0.027)	(0.027)	(0.027)
Constant	0.148***	0.272	0.233	0.248
	(0.048)	(0.199)	(0.201)	(0.200)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	48,186	48,186	48,186	48,186
Adjusted R^2	0.571	0.597	0.599	0.627

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 24: Over 35 only triple diff design

	Dependent variable:Log of reservation wages			
	Age > 35			
	(1)	(2)	(3)	(4)
Treatment	0.015***	0.012***	0.012***	0.014***
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.021***	0.019***	0.019***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)
State	−0.002**	0.001	0.0005	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.822***	0.790***	0.789***	0.774***
	(0.0004)	(0.0005)	(0.0005)	(0.0005)
Treatment × *Post*	0.007***	0.005***	0.005***	0.003***
	(0.001)	(0.0005)	(0.0005)	(0.0005)
Treatment × *State*	0.004***	0.002**	0.002**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Post × *State*	0.016***	0.012***	0.011***	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)
Treatment × *Post* × *State*	0.038***	0.037***	0.036***	0.036***
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.292***	0.388***	0.391***	0.427***
	(0.002)	(0.008)	(0.008)	(0.008)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	59,656	59,518	59,518	58,518
Adjusted R^2	0.691	0.703	0.703	0.706
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: State FE and seasonality terms included but not shown

Table 25: 35 or younger only triple diff design

	Dependent variable: Log of reservation wages			
	Age $\leq$ 35			
	(1)	(2)	(3)	(4)
Treatment	0.008***	0.007***	0.006***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.025***	0.026***	0.026***	0.024***
	(0.001)	(0.001)	(0.001)	(0.001)
State	−0.009***	−0.009***	−0.009***	−0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.768***	0.737***	0.736***	0.716***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *Post*	0.008***	0.009***	0.009***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *State*	0.008***	0.010***	0.010***	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
Post × *State*	0.040***	0.046***	0.046***	0.044***
	(0.003)	(0.003)	(0.003)	(0.003)
Treatment × *Post* × *State*	0.059***	0.058***	0.057***	0.053***
	(0.004)	(0.004)	(0.004)	(0.004)
Constant	0.413***	0.494***	0.493***	0.574***
	(0.003)	(0.015)	(0.015)	(0.015)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	48,442	48,283	48,283	48,283
Adjusted R^2	0.620	0.636	0.636	0.643
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 26: Less than high school graduates only (stacked event study design)

	Dependent variable: Log of reservation wages			
	LTHS only			
	(1)	(2)	(3)	(4)
Treatment	0.113***	0.093***	0.085***	0.089***
	(0.013)	(0.013)	(0.013)	(0.013)
Post	−0.071***	−0.071***	−0.071***	−0.057***
	(0.010)	(0.010)	(0.010)	(0.010)
Log of pre-separation wages	0.995***	0.973***	0.930***	0.978***
	(0.026)	(0.027)	(0.026)	(0.027)
Treatment × *Post*	0.066***	0.071***	0.067***	0.075***
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment × *Lag*(−4)	−0.007	−0.006	−0.007	−0.013
	(0.018)	(0.017)	(0.017)	(0.017)
Treament × *Lag*(−3)	−0.017	−0.024	−0.027	−0.024
	(0.027)	(0.029)	(0.028)	(0.028)
Treatment × *Lag*(−2)	0.053	0.063	0.073	0.059
	(0.066)	(0.065)	(0.064)	(0.062)
Treatment × *Lag*(1)	0.064*	0.073***	0.073***	0.098***
	(0.036)	(0.036)	(0.035)	(0.036)
Treatment × *Lag*(2)	0.064***	0.047**	0.078***	0.063***
	(0.023)	(0.023)	(0.024)	(0.025)
Treatment × *Lag*(3)	0.032***	0.042***	0.037***	0.061***
	(0.009)	(0.009)	(0.009)	(0.009)
Treatment × *Lag*(4)	0.008	0.007	0.036***	0.045***
	(0.013)	(0.013)	(0.012)	(0.013)
Constant	−0.180**	0.156	0.269**	0.091
	(0.074)	(0.109)	(0.107)	(0.113)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	13,463	13,463	13,463	13,463
Adjusted R^2	0.615	0.637	0.655	0.685

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 27: High school graduates only (stacked event study design)

	Dependent variable:Log of reservation wages			
	High school only			
	(1)	(2)	(3)	(4)
Treatment	0.029***	0.030***	0.034***	0.043***
	(0.007)	(0.007)	(0.007)	(0.007)
Post	−0.011***	−0.015***	−0.018***	−0.010**
	(0.004)	(0.004)	(0.004)	(0.004)
Log of pre-separation wages	0.796***	0.798***	0.810***	0.804***
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment × *Post*	0.065***	0.054***	0.067***	0.062***
	(0.010)	(0.010)	(0.010)	(0.010)
Treatment × *Lag*(−4)	0.005	0.001	0.005	−0.029
	(0.026)	(0.025)	(0.025)	(0.025)
Treatment × *Lag*(−3)	−0.013	−0.016	−0.014	−0.012
	(0.013)	(0.013)	(0.013)	(0.013)
Treatment × *Lag*(−2)	−0.071	−0.092**	−0.084*	−0.069
	(0.044)	(0.044)	(0.044)	(0.044)
Treatment × *Lag*(1)	0.071***	0.077***	0.070***	0.067***
	(0.015)	(0.015)	(0.015)	(0.015)
Treatment × *Lag*(2)	0.031***	0.032***	0.033***	0.025***
	(0.006)	(0.006)	(0.006)	(0.006)
Treatment × *Lag*(3)	0.034**	0.030**	0.047***	0.040**
	(0.018)	(0.018)	(0.018)	(0.019)
Treatment × *Lag*(4)	0.036***	0.042***	0.044***	0.035***
	(0.013)	(0.013)	(0.014)	(0.014)
Constant	0.415***	0.478***	0.463***	0.342***
	(0.036)	(0.046)	(0.046)	(0.049)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	70,492	70,492	70,492	70,328
Adjusted R^2	0.554	0.562	0.568	0.580
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: State FE and seasonality terms included but not shown

Table 28: College graduates only (stacked event study design)

	Dependent variable: Log of reservation wages			
	College only			
	(1)	(2)	(3)	(4)
Treatment	-0.112^{***}	-0.074^{***}	-0.088^{***}	-0.082^{***}
	(0.017)	(0.018)	(0.019)	(0.019)
Post	-0.025^{**}	-0.034^{***}	-0.056^{***}	-0.071^{***}
	(0.011)	(0.012)	(0.012)	(0.012)
Log of pre-separation wages	0.499^{***}	0.665^{***}	0.655^{***}	0.682^{***}
	(0.038)	(0.041)	(0.043)	(0.043)
Fraction of benefits consumed			0.001^{***}	0.0002
			(0.0002)	(0.0002)
$Treatment \times Post$	0.043^{***}	0.038^{***}	0.052^{***}	0.043^{**}
	(0.022)	(0.023)	(0.024)	(0.025)
$Treatment \times Lag(-4)$	0.018	0.019	-0.037^{**}	-0.007
	(0.017)	(0.017)	(0.018)	(0.017)
$Treatment \times Lag(-3)$	-0.0493	-0.0535	-0.0568	-0.033
	(0.062)	(0.073)	(0.071)	(0.070)
$Treatment \times Lag(-2)$	0.060^{*}	0.042	-0.035	-0.046
	(0.033)	(0.033)	(0.034)	(0.032)
$Treatment \times Lag(1)$	0.067^{***}	0.056^{***}	0.058^{***}	0.096^{***}
	(0.031)	(0.037)	(0.034)	(0.032)
$Treatment \times Lag(2)$	0.058^{**}	0.091^{***}	0.090^{***}	0.086^{***}
	(0.031)	(0.031)	(0.031)	(0.029)
$Treatment \times Lag(3)$	0.019	0.042	0.098^{**}	0.091^{*}
	(0.045)	(0.045)	(0.047)	(0.047)
$Treament \times Lag(4)$	0.044^{*}	0.043	0.035	0.037
	(0.027)	(0.027)	(0.027)	(0.026)
Constant	0.854^{***}	0.756^{***}	0.748^{***}	0.987^{***}
	(0.104)	(0.133)	(0.140)	(0.138)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	23,014	23,014	23,014	23,014
Adjusted R^2	0.694	0.720	0.733	0.804

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Notes: State FE and seasonality terms included but not shown

Table 29: Less than high school graduates only (triple diff design)

	Dependent variable: Log of reservation wages			
	LTHS only			
	(1)	(2)	(3)	(4)
Treament	−0.024***	−0.021***	−0.020***	−0.017***
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.011***	0.012***	0.012***	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
State	0.031***	0.034***	0.035***	0.033***
	(0.002)	(0.002)	(0.002)	(0.002)
Log of pre-separation wages	0.730***	0.707***	0.707***	0.684***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *Post*	0.007***	0.007***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × *State*	0.046***	0.049***	0.050***	0.048***
	(0.002)	(0.002)	(0.002)	(0.002)
Post × *State*	0.028***	0.027***	0.028***	0.033***
	(0.006)	(0.006)	(0.006)	(0.006)
Treatment × *Post* × *State*	0.046***	0.047***	0.047***	0.041***
	(0.007)	(0.007)	(0.007)	(0.006)
Constant	0.530***	0.563***	0.570***	0.618***
	(0.004)	(0.006)	(0.006)	(0.006)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	16,619	16,619	16,619	16,619
Adjusted R^2	0.643	0.648	0.649	0.653
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: State FE and seasonality terms included but not shown

Table 30: High school graduates only (triple diff design)

	Dependent variable:Log of reservation wages			
	High school grads only			
	(1)	(2)	(3)	(4)
Treatment	0.016***	0.018***	0.018***	0.017***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Post	0.029***	0.031***	0.031***	0.028***
	(0.001)	(0.001)	(0.001)	(0.001)
State	−0.006***	−0.005***	−0.006***	−0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.790***	0.771***	0.771***	0.752***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Treatment × *Post*	0.020***	0.021***	0.021***	0.017***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Treatment × *State*	0.013***	0.013***	0.013***	0.013***
	(0.001)	(0.001)	(0.001)	(0.001)
Post × *State*	0.008***	0.011***	0.011***	0.010***
	(0.002)	(0.002)	(0.002)	(0.002)
Treatment × *Post* × *State*	0.045***	0.032***	0.041***	0.045***
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.362***	0.375***	0.381***	0.424***
	(0.002)	(0.003)	(0.003)	(0.003)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	71,759	71,759	71,759	71,759
Adjusted R^2	0.656	0.660	0.661	0.666
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 31: College graduates only (triple diff design)

	Dependent variable:Log of reservation wages			
	College only			
	(1)	(2)	(3)	(4)
$Treatment$	0.014***	0.012***	0.012***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
$Post$	0.009***	0.009***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
$State$	-0.006^{***}	-0.004^{***}	-0.004^{***}	-0.007^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Log of pre-separation wages	0.821***	0.802***	0.800***	0.785***
	(0.001)	(0.001)	(0.001)	(0.001)
$Treatment \times Post$	0.017***	0.016***	0.017***	0.019***
	(0.001)	(0.001)	(0.001)	(0.001)
$Treatment \times State$	0.008***	0.002	-0.00000	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
$Post \times State$	0.008**	0.008**	0.007**	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
$Treatment \times Post \times State$	0.035***	0.039***	0.049***	0.046***
	(0.006)	(0.006)	(0.006)	(0.006)
Constant	0.335***	0.237***	0.251***	0.288***
	(0.003)	(0.006)	(0.006)	(0.006)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	23,923	23,923	23,923	23,923
R^2	0.663	0.668	0.668	0.672
Adjusted R^2	0.663	0.668	0.668	0.672
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes: State FE and seasonality terms included but not shown

Table 32: Reservation wages vs minimum wage increase by state level labour market tightness quartiles

	Dependent variable:			
	response			
	(1)	(2)	(3)	(4)
Group	0.017**	0.025***	0.022***	0.029***
	(0.007)	(0.009)	(0.005)	(0.005)
Post	−0.0001	0.005	0.008**	0.025***
	(0.008)	(0.008)	(0.004)	(0.003)
$Treatment \times Lag(-4) : \gamma_{-4}$	0.031	−0.011	0.036	0.045
	(0.043)	(0.024)	(0.025)	(0.034)
$Treament \times Lag(-3) : \gamma_{-3}$	−0.010	−0.044	0.062*	0.022
	(0.060)	(0.028)	(0.032)	(0.058)
$Treatment \times Lag(-2) : \gamma_{-2}$	−0.011	−0.021	0.019	0.018
	(0.016)	(0.028)	(0.012)	(0.016)
$Treatment \times Post : \gamma_0$	0.002	0.018**	0.078***	0.071***
	(0.008)	(0.009)	(0.006)	(0.005)
$Treatment \times Lag(1) : \gamma_1$	0.035**	0.021	0.063***	0.047***
	(0.017)	(0.031)	(0.011)	(0.016)
$Treatment \times Lag(2) : \gamma_2$	0.010	0.017	0.042***	0.041**
	(0.020)	(0.030)	(0.013)	(0.019)
$Treatment \times Lag(3) : \gamma_3$	0.008	0.003	0.035***	0.044**
	(0.017)	(0.027)	(0.012)	(0.019)
$Treatment \times Lag(4) : \gamma_4$	0.0003	0.007	0.035***	0.043***
	(0.017)	(0.037)	(0.013)	(0.017)
Constant	0.681***	0.557***	0.662***	0.726***
	(0.043)	(0.042)	(0.029)	(0.032)
Observations	11,870	7,258	19,532	27,653
Adjusted R^2	0.679	0.723	0.746	0.757
Note:	*p<0.1; **p<0.05; ***p<0.01			

Notes: Using state level unemployment data from CPS and vacancy data from BGT, the entire BAM sample is sorted into labour market tightness quartiles.

Table 33: Reservation wages vs minimum wage increase by state level labour market tightness quartiles

	Dependent variable:			
	response			
	(1)	(2)	(3)	(4)
Group	0.011***	0.015***	0.067***	0.024***
	(0.002)	(0.002)	(0.001)	(0.001)
Post	0.006***	0.002	0.039***	0.027***
	(0.002)	(0.002)	(0.001)	(0.001)
ExperimentState	0.061***	0.051***	0.113***	0.107***
	(0.010)	(0.009)	(0.008)	(0.018)
Log of pre-separation wages	0.640***	0.734***	0.736***	0.708***
	(0.002)	(0.002)	(0.001)	(0.001)
$Group \times Post$	−0.003	−0.003	0.050***	0.044***
	(0.002)	(0.002)	(0.001)	(0.001)
$Group \times ExperimentalState$	0.097***	0.087***	0.048***	0.053
	(0.011)	(0.012)	(0.013)	(0.024)
$Post \times ExperimentalState$	0.025***	0.022***	0.027***	0.087***
	(0.010)	(0.010)	(0.008)	(0.018)
$Group \times Post \times ExperimentalState$	0.0002	0.008	0.025*	0.085***
	(0.012)	(0.013)	(0.014)	(0.025)
Constant	0.462***	0.450***	0.226***	0.511***
	(0.010)	(0.009)	(0.007)	(0.008)
Observations	11,842	7,614	19,645	27,647
Adjusted R^2	0.685	0.754	0.779	0.750
Note:	*p<0.1; **p<0.05; ***p<0.01			

Theory and intuition predict that in a tight labour market the reservation wage response following a minimum wage increase would be higher than in a slack labour market, as more job-openings per job-seeker would make job-search less costly. The results of the event study design and triple diff design by labour market tightness quartiles are tabulated in Table 32 and 33 respectively, and they confirm our intuition. In particular, tight labour markets are associated with stronger treatment effects in the stacked event study design and triple diff design, and more persistent treatment effects in the event study design.

5 Conclusion

From the academic literature to the news (e.g. debates around a $15/hour minimum wage), the discussion around the effects of minimum wages focus entirely on the demand side for labour. The supply side effects of the minimum wage are discussed in this paper, by analysing the effect of the minimum wage on reservation wages. Since reservation wages are defined as a measure of job-selectivity, impacts on the reservation wage translate to labour supply. Furthermore, this paper is one of a handful that analyses reservation wages empirically, and the first one to do so in an American context. Using a triple-diff design, I am able to show that raising minimum wages causes reservation wages to rise as well. In particular, I find that following a minimum wage increase the reservation wages for low-wage workers rises by 4.0-7.5%. These findings are robust to various demographic and economic controls.

These results offer suggestive evidence of frictions in the labour market, wherein the unemployed adjust their wage-expectations in response to wage floors. The positive impact of minimum wages supports the results of Falk et al. (2006), and Fedorets and Shupe (2021). These findings constitute an important contribution on the discussion around minimum wages since they suggest non-trivial supply responses to minimum wage shocks. Using panel data to estimate the actual post-unemployment spell wages would allow future research to shed more light on the question of the labour-supply effects of minimum wages.

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A.1 Theoretical background

The neoclassical theory of labour supply (without search frictions and match-specific capital) models an individual choosing between consumption (C) and time devoted to leisure ($1 - L$) [16] by optimising the following problem:

$$\begin{aligned} &\max_{C,L} U(C, L) \\ \text{s.t. } &C + wL \leq R_0 \\ &L \in [0, 1] \end{aligned} \tag{3}$$

where L represents share of time spent working, and R_0 is a scaler, representing potential income at $L = 1$. In this model, a solution may be defined as a pair (C^*, L^*) satisfying the constraints in Equation (3) and the tangency condition:

$$w = \frac{U_L}{U_C} \tag{4}$$

and the reservation wage may be defined as the lowest wage (denoted w_{res}) satisfying the tangency condition in Equation (4) such that $L > 0$. Thus we can say

$$w_{res} = \left.\frac{U_L}{U_C}\right|_{L=1}.$$

This relatively simple model shows that labour supply need not monotonically increase with wages (w), due to competing substitution and income effects. In particular, a minimum wage worker, following an increase in the minimum wage, faces a substitution effect that nudges the individual to work more as wages rise, while the income effect induces a larger demand for both consumption and leisure. [17]

It's trivial to show that the neoclassical model cannot account for the imperfect competition we see in the labour market. This shortcoming necessitates the incorporation of search frictions in models of the labour market to determine equilibrium effects. Consider an infinitely lived, risk-neutral agent that maximises lifetime consumption:

$$\int_0^\infty \exp(-\rho t) c_t dt$$

where ρ is the subjective discount factor. Wage offers w are assumed to be *iid* draws from distribution $F(\cdot)$ such that

$$\Pr(W \leq w) = F(w).$$

[16] The total endowment of time is normalized to 1, and so $1 - L$ denotes time spent on leisure, and L denotes time spent working.

[17] As noted in Flinn, 2003: In our view, these recent studies have been particularly useful in indicating that the "textbook" competitive model of the labor market, which has been used as an interpretive framework for the bulk of empirical studies using aggregated time series data, may have serious deficiencies in accounting for minimum wage effects on labor market outcomes when confronted with disaggregated data.

with bounded support: $[w_{\min}, \overline{w}] \subset \mathbb{R}$. In a McCall model environment, there is no job destruction, and the present value of accepting a wage offer w is

$$\frac{w}{\rho}.$$

Starting with the value functions of a McCall model in discrete time; W and V represent :

$$\begin{aligned} W(w) &= w + \beta W(w) \\ V &= z + \beta \int_{w_{min}}^{\overline{w}} \max\{V, W(w)\}\, dF(w) \end{aligned} \tag{5}$$

In steady state

$$\beta = \frac{1}{1+\rho}.$$

Let h denote a fraction of a period, thus offers arrive with probability λh and the value functions in Equations (5) satisfy

$$\begin{aligned} W(w) &= wh + \frac{1}{1+\rho h} W(w) \\ \Rightarrow (1+\rho h)W(w) &= (1+\rho h)wh + W(w) \\ \Rightarrow \rho W(w) &= (1+\rho h)w \end{aligned} \tag{6}$$

and

$$\begin{aligned} V &= zh + \frac{\lambda h}{1+\rho h} \int_{w_{\min}}^{\overline{w}} \max\{V, W(w)\}\, dF(w) \\ \Rightarrow (1+\rho h)V &= (1+\rho h)zh + \lambda h \int_{w_{\min}}^{\overline{w}} \max\{V, W(w)\}\, dF(w) + (1-\lambda h)V \\ \Rightarrow \rho h V &= \left\{ (1+\rho h)zh + \lambda h \int_{w_{\min}}^{\overline{w}} \max\{V, W(w)\}\, dF(w) - \lambda h V \right\} \pm \lambda V \\ \Rightarrow \rho h V &= (1+\rho h)zh + \lambda h \int_{w_{\min}}^{\overline{w}} \max\{0, W(w) - V\}\, dF(w) \\ \Rightarrow \rho V &= (1+\rho h)z + \lambda \int_{w_{\min}}^{\overline{w}} \max\{0, W(w) - V\}\, dF(w) \end{aligned} \tag{7}$$

Finally, evaluate Equations (6) and (7) at $\lim_{h \to 0}$; the former equation becomes $W(w) = \frac{w}{\rho}$, which is the value of accepting a wage offer w. Plug this into the latter equation to get the value function of a job-seeker as in Equation (9):

$$\rho V = z + \lambda \int_{w_{\min}}^{\overline{w}} \max\{0, W(w) - V\}\, dF(w) \tag{8}$$

Assuming offers arrive with probability λ at the start of each period, the value function of a McCall job seeker (in steady state) can be written as

$$\rho V(w) = z + \lambda \int_{w_{\min}}^{\overline{w}} \max\left\{0, \frac{w}{\rho} - V(w)\right\} dF(w) \tag{9}$$

where the LHS, $\rho V(w)$, denotes the flow value of unemployment and z are transfers while unemployed [18]. Two things to note about Equation (9): It is a contraction mapping, and since $\frac{w}{\rho}$ is monotone increasing, there is a unique reservation wage, w_{res} which serves as the optimal stopping rule in the job search process. As a result a McCall job seeker will accept any offer $w^* \geq w_{res}$. Furthermore, at $w = w_{res}$, $\rho V(w_{res}) = w_{res}$. Plugging this into (9), simplifying, and integrating by parts, we get:

$$w_{res} = z + \frac{\lambda}{\rho} \int_{w_{res}}^{\overline{w}} [1 - F(w)]\, dw \tag{10}$$

The impact of a minimum wage on the reservation wage, and therefore on spell duration, and labour supply, depends on whether the partials of Equation (10) wrt λ and $1 - F(w)$ is greater than or less than zero, i.e. responding negatively due to fall in offer frequency or positively due to the rise in wages. In particular these partials maybe written as:

$$\begin{aligned} \frac{\partial w_{res}}{\partial \lambda} &= \frac{1}{\rho} \int_{w_{\text{res}}}^{\overline{w}} [1 - F(w)]\, dF(w) \\ \frac{\partial}{\partial(1 - F(w))} &= -\frac{\lambda}{\rho} [F(\overline{w}) - F(w_{\text{res}})] \end{aligned} \tag{11}$$

Dube et al. (2010) use minimum wage policy variation across state-borders to find no effect on employment, suggesting that the impact on the wage offer distribution is negligible. Cengiz et al. (2019) uses a bunching estimator design to get a similar conclusion, and find that wage-offers are adjusted to meet the wage floor, and not destroyed. On the other hand, Aaronson et al. (2018) find the rate of entry to, and exit from unemployment rises for workers in fast-food restaurants, while there is no change in employment in the industry overall. Furthermore Clemens and Wither (2019) study the effects of the 2009 federal minimum wage hike to find employment fell by at least half a percentage point in states where the minimum wage was binding. In the literature review (in Section 1) I discuss other papers that show employment falls, but it is not obvious that these effects are due to job/vacancy destruction (and discuss still other papers which find insignificant or even positive employment effects). The impact of minimum wages on labour market dynamics (especially as it pertains to offer

[18] See Section A.1 for a detailed derivation.

arrival rates, and the wage distribution overall) remains inconclusive[19]. This theoretical ambiguity highlights the need for analysing the effects of minimum wages on reservation wages, for policy makers and academics alike.

B.2 Parallel trends in the context of a triple diff design

In the context of the triple-diff design, the assumption of parallel trends is modified such that we require the relative outcome of the treated and control groups in the experimental state to trend the same way as the relative outcome of the treated and control groups in the non-experimental state (i.e. the state without treatment). This can be written mathematically as

$$\begin{aligned} &[(E\left[Y|E=1,T=1,Post=1\right]-E\left[Y|E=1,T=1,Post=0\right]) \\ &-(E\left[Y|E=1,T=0,Post=1\right]-E\left[Y|E=1,T=0,Post=0\right])] \\ &-[(E\left[Y|E=0,T=1,Post=1\right]-E\left[Y|E=0,T=1,Post=0\right]) \\ &-(E\left[Y|E=0,T=0,Post=1\right]-E\left[Y|E=0,T=0,Post=0\right])] \end{aligned} \tag{12}$$

where E denotes the experimental state indicator, T denotes the treatment group, and $Post$ denotes the post-treatment implementation period. Using the potential outcomes framework, we can discuss $E[Y_{1,ETPost}]$ ($E[Y_{0,ETPost}]$) as the expected outcome of a treated unit (untreated unit) in state E, treatment group T, at time $Post$. We either observe $\overline{Y}_{1,ETPost}$ or $\overline{Y}_{0,ETPost}$, but never both. Thus the causal effect in a triple diff design (more precisely, the average treatment effect on the treated) in experimental state E, on treatment group T, in treatment period $Post$ can be defined as:

$$E\left[Y_1-Y_0|E=1,T=1,Post=1\right].$$

Rewriting Equation 12 with the potential outcomes notation:

$$\begin{aligned} &[(E\left[Y_1|E=1,T=1,Post=1\right]-E\left[Y_0|E=1,T=1,Post=0\right]) \\ &-(E\left[Y_0|E=1,T=0,Post=1\right]-E\left[Y_0|E=1,T=0,Post=0\right])] \\ &-[(E\left[Y_0|E=0,T=1,Post=1\right]-E\left[Y_0|E=0,T=1,Post=0\right]) \\ &-(E\left[Y_0|E=0,T=0,Post=1\right]-E\left[Y_0|E=0,T=0,Post=0\right])] \end{aligned} \tag{13}$$

We need the differential in outcomes of treated and control groups in the experimental state to trend similarly to the differential in outcomes of treated

[19] In Germany, a high-impact minimum wage was introduced in 2015 and preliminary research suggests a rightward shift in the wage offer distribution. See Burauel et al. (2020), Caliendo et al. (2017), and Bossler and Gerner (2020) for more details. But the translation of this shift in the offer arrival rate is not obvious. Furthermore, it would be imprudent to draw conclusions from the German context to the American one due to the medley of confounding factors.

and control groups in the non-experimental state, in the absence of treatment. Formally:

$$\begin{aligned} &[(E\left[Y_0|E=1,T=1,Post=1\right]-E\left[Y_0|E=1,T=1,Post=0\right]) \\ &-(E\left[Y_0|E=1,T=0,Post=1\right]-E\left[Y_0|E=1,T=0,Post=0\right])] \\ &=[(E\left[Y_0|E=0,T=1,Post=1\right]-E\left[Y_0|E=0,T=1,Post=0\right]) \\ &-(E\left[Y_0|E=0,T=0,Post=1\right]-E\left[Y_0|E=0,T=0,Post=0\right])] \end{aligned} \tag{14}$$

C.3 Additional material

Table C1: Mincer regression or reservation wages with industry controls (using NAICS codes), demographic controls (like age, age-squared, gender, ethnicity, and education levels.), controls for having dependents, and log of pre-separation wages.

	Dependent variable:
	Log of reservation wages
Female dummy	0.0018
	(0.0024)
Black dummy	-0.1289
	(0.1173)
Asian dummy	-0.0831
	(0.1182)
Log pre-separation wage	0.7887
	(0.0028)
Age	0.0008
	(0.0011)
Age ^{2}	-0.0000
	(0.0000)
High school dummy	0.0180
	(0.0027)
College dummy	0.0423
	(0.0038)
Dependents more than 0	-0.0051
	(0.0037)
Weeks worked during base period	0.0001
	(0.0001)
Weeks worked during base period squared	-0.0010
	(0.0019)
Fraction of benefits consumed	-0.037^{***}
	(0.0004)
Log of maximum benefit amount payable	0.079^{***}
	(0.020)
Constant	2.652^{***}
	(0.001)
Observations	311,147
R^2	0.647
Adjusted R^2	0.647
Residual Std. Error	0.457 (df = 311145)
Note:	*p<0.1; **p<0.05; ***p<0.01

Table C2: Log spell duration vs log reservation wages

	Dependent variable:			
	Log of spell duration			
	(1)	(2)	(3)	(4)
Log of reservation wages	−0.213***	−0.221***	−0.117***	−0.104***
	(0.009)	(0.010)	(0.008)	(0.008)
Log of pre-separation wages	0.155***	0.147***	0.059***	0.066***
	(0.010)	(0.010)	(0.008)	(0.009)
Max benefits	0.00000	0.00000	0.00001***	0.00001***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Fraction of benefits consumed			0.016***	0.016***
			(0.0001)	(0.0001)
Constant	2.450***	2.449***	1.863***	1.888***
	(0.021)	(0.036)	(0.029)	(0.034)
Age polynomial	No	Yes	Yes	Yes
Sex dummies	No	Yes	Yes	Yes
Race dummies	No	Yes	Yes	Yes
Education dummies	No	Yes	Yes	Yes
No of dependents	No	No	Yes	Yes
Frac of benefits consumed	No	No	Yes	Yes
Industry dummies	No	No	No	Yes
Observations	98,915	97,909	97,909	97,909
R^2	0.028	0.032	0.375	0.380
Adjusted R^2	0.027	0.032	0.375	0.379
Residual Std. Error	0.758 (df = 98879)	0.756 (df = 97848)	0.607 (df = 97844)	0.605 (df = 97821)
F Statistic	80.400*** (df = 35; 98879)	54.120*** (df = 60; 97848)	919.000*** (df = 64; 97844)	688.900*** (df = 87; 97821)
Note:				*p<0.1; **p<0.05; ***p<0.01

OLS estimates of reservation wages against spell duration. Panel 1 has no controls, Panel 2 controls for pre-separation wages, Panel 3 adds seasonality, and Panel 4 adds state fixed effects.