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ABSTRACT

Simple theoretical models of job search can imply that a higher minimum wage increases the number of job seekers for affected jobs. Researchers defending or explaining nonnegative estimated employment effects of minimum wages often appeal to these models, and sometimes claim that this is the most plausible prediction. We use novel data on job search in U.S. states to examine the effect of minimum wage increases on the number of job seekers for low-skilled positions. We find little if any evidence that higher minimum wages increase job search for low-skilled jobs, and more evidence that higher minimum wages decrease the number of workers seeking employment in these jobs.

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1 Introduction

Analyses of the effects of minimum wages in job search models have focused on job search by the unemployed, which – in some simple formulations of job search models – is predicted to increase in response to a higher minimum wage. The job search response among the unemployed could lead to higher employment, because it could lead firms to open up more job vacancies and fill more jobs (e.g., Flinn, 2006; Manning, 2021). However, this prediction does not necessarily hold in search models where the job-finding rate is endogenous (e.g., Mincer, 1976), and the prediction is also undermined by models that expand job characteristics beyond the wage (Clemens, 2021). Moreover, Faberman et al. (2022) show that job search while on the job is widespread and constitutes a large majority of search activity for low-skilled occupations, and the predicted effects of minimum wage increases on search while employed may be negative – an additional reason a higher minimum wage may not increase search or employment. Thus, understanding how minimum wages impact job search is informative about the potential for higher minimum wages to generate positive employment effects, or for job search models to rationalize the absence of evidence of non-negative employment effects of the minimum wage.

We use novel data from one of the three largest job search platforms in the United States covering January 2021 to September 2023 to estimate the effect of unscheduled minimum wage increases on the number of job seekers for low-skilled occupations.[1] While the existing empirical literature on search behavior predominantly relies on self-reported survey data (the Current Population Survey (CPS), the Survey of Income and Program Participation (SIPP), or the American Time Use Survey (ATUS)), our dataset is unique in that we directly observe job seekers' behavior on the platform via tracking user interactions.[2]

There are three main advantages of our search measure data over existing survey data. First, our search measure captures actual behavior through clicks, thereby avoiding the problems that come with self-reported survey data.[3] Second, while our measure focuses on specific low-

[1] By "unscheduled," we mean the increase was not a subsequent step legislated earlier, or an indexed increase.

[2] Our job search measure represents the number of individuals (identified via IP addresses) who clicked on at least one job posting for a given occupation. Thus, we do not measure search effort or intensity (the number of clicks, or applications submitted), but what we interpret as the number of job searchers.

[3] We are unable to observe search intensity at the individual level. The platform identifies unique users with tracking cookies, but the data we receive are aggregated at the job title × month level. Within each job title, the cookie prevents double-counting (so a person clicking multiple postings with the same title in a month is counted

wage occupations within a single platform – and we cannot observe whether reduced search in these categories is offset by increased search elsewhere – this specificity on low-wage occupations provides a core advantage over existing studies. Unlike surveys that capture aggregate search effort across all job types, our data directly measure search for precisely those occupations most likely to be affected by minimum wage changes. The consistency between our findings and those of Adams et al. (2018) provides complementary evidence: while their survey data can only measure aggregate search behavior (stratifying by education provides only a rough approximation of affected workers), our behavioral data more precisely captures search in minimum-wage-relevant occupations. Third, our data capture search behavior regardless of employment status – particularly important given that Faberman et al. (2022) show that the majority of job search comes from already-employed workers. The CPS data used by Adams et al. (2018) do not capture any search activity by employed workers (who are not asked about search), thereby excluding all on-the-job search, while their SIPP data show only 2.6% of employed workers searching – a likely undercount. As the authors note, even the ATUS time-use data understate on-the-job search since workers record only their primary activity, meaning someone searching while at work would be coded as working rather than searching.

By directly observing search behavior – regardless of employment status – we provide a more accurate view of search responses than previously available data. In particular, our research focuses on two unscheduled minimum wage increases – in Nebraska and Hawaii – using a difference-in-differences strategy that compares changes in job search in the treated states to changes in other states whose minimum wages did not change. In these core analyses we find no evidence that higher minimum wages increase job search, and instead that, if anything, minimum wage increases are associated with substantial decreases in the number of job seekers for low-skilled occupations. Including additional analyses of other state minimum wage increases – although these provide less clean "experiments" because the increases are scheduled and hence known in advance – we continue to find essentially no any evidence that higher minimum wages increase search for low-skilled jobs, and more evidence pointing to negative

only once). However, we cannot track individuals across different job titles, nor can we observe how many total postings each person viewed. Thus, while our measure accurately captures the number of distinct job seekers in each occupation, it does not permit construction of a measure of per-person search effort across occupations.

effects.

The neoclassical theory of labor demand predicts that a higher minimum wage will reduce employment of the least-skilled workers. There is a great deal of evidence consistent with this prediction, but there are also some estimates in the research literature that point to no effects or even positive effects on low-skill employment (Neumark and Shirley, 2022). Some researchers interpret the overall body of evidence, then, as inconsistent with the neoclassical model. They argue, instead, that frictions in labor markets give rise to a monopsony-type result where a higher minimum wage can increase low-skilled employment (e.g., Manning, 2005, 2020; Schmitt, 2015; Wiltshire et al., 2024). Studies that cite monopsony-like explanations of positive or non-negative minimum wage effects include, for example, Card and Krueger (1994), Derenoncourt and Montialoux (2021), Dube (2019), and Giuliano (2013).

According to this argument, in the presence of labor market search frictions, "not all workers who want a job manage to get one, and not all employers who want to hire a worker manage to find one" (Manning, 2021, p. 21). Under such conditions, both unemployment and job vacancies can exist in equilibrium. A higher minimum wage reduces labor demand, ceteris paribus, but it can also increase labor supply – i.e., the number of job seekers. Under certain conditions, the increased number of workers searching for jobs might induce firms to post more job vacancies if they believe there is an increased probability of filling them with productive workers, and the net effect may be to increase low-skilled employment, in contrast with the neoclassical model.[4] Search models in this framework are often referred to as models of "dynamic monopsony."

There are also other models that can create upward-sloping labor supply curves to the firm, and hence generate monopsony-like responses that can lead to a higher minimum wage increasing employment. Rebitzer and Taylor (1995) generate this response in an efficiency wage model. In their model, firms have two ways to create incentives for workers: they can monitor carefully and fire a worker if they "shirk," or they can monitor less, lowering the probability of detection of shirking, but pay an above-market wage so that the value of the job, relative to another job or being unemployed, creates incentives not to shirk. Suppose there is only one

[4] A more formal treatment is provided in, e.g., Flinn (2006). For an accessible discussion of search-and-matching models, see Daly et al. (2012).

manager monitoring workers. If the firm expands employment, the manager's ability to monitor each individual worker declines, and the firm may thus raise the wage to deter shirking. This creates the same phenomenon as in the classical monopsony model – that the marginal cost of labor curve is steeper than the labor supply curve – in which case an aptly chosen minimum wage can increase employment (Stigler, 1946).

Another explanation of monopsony-type effects is unique to the "table-service" part of the restaurant industry, where part of workers' compensation comes as tips (Wessels, 1997). If a restaurant adds a waiter, then on the margin (i.e., with no other changes) the tips earned per waiter decrease. In order to maintain the elevated level of employment, the restaurant would need to pay a higher wage to all tipped workers, again implying that a higher minimum wage can increase employment. A third explanation is static, explaining employer labor market power as resulting from workers' idiosyncratic tastes for different combinations of job amenities that are offered by small numbers of firms, and which are not fully priced into wages (Manning, 2020).

Each of these alternative models may apply in some contexts. However, we believe it is fair to say that a model of search and matching is the canonical model that economists rely on to generate the behavioral prediction that a higher minimum wage may increase employment and to rationalize findings of non-negative employment effects. For example, in recent work Hurst et al. (2022) use a search and matching model to analyze the distributional and long- and short-run effects of the minimum wage. Wiltshire et al. (2024) similarly emphasize search and matching frictions in their recent study of the fast-food industry. There are scores of other papers that use search and matching models to analyze minimum wage effects. The core implication of these models is that higher minimum wages, under certain conditions, can induce more search, leading to more vacancies posted and/or filled, and therefore potentially higher employment.

Moreover, the argument is often quite explicit:

> "Non-competitive labor market models recently have received increased attention in the minimum wage literature, in part because of the positive employment effects reported in Card and Krueger (1994) and their dynamic monopsony explanation

Card and Krueger (1995).” (Dube et al., 2007, p. 524)

> “The claim that the employment effect might not be negative continues to be met with incredulity in some quarters or to be euphemistically described as “unconventional.” But as soon as one acknowledges that efficiency wage effects might be important or that labor markets have frictions – ideas that appear in mainstream introductory-level textbooks and can hardly be described as unconventional – one has to acknowledge that the impact of the minimum wage on employment is theoretically ambiguous.” (Manning, 2021, p. 22)

Furthermore, search responses to minimum wages are used more broadly in the labor economics and public finance research—not only to rationalize or explain non-negative employment effects. Job search responses to minimum wages are routinely invoked in theoretical models of the minimum wage (e.g., Gregory and Zierahn, 2022; Blömer et al., 2024; Gautier and Moraga-González, 2018). In the public finance literature, search responses are used in work aimed at determining an optimal minimum wage (e.g., Chéron et al. (2008)) and modeling of the minimum wage as part of optimal tax and transfer policy (e.g., Lavecchia, 2020, and Hungerbühler and Lehmann, 2009).

Although a positive search response is commonly assumed or invoked to try to explain non-negative employment effects of minimum wages, it is not always assumed in this broader literature. Richer search models – in particular those with an endogenous job finding rate – do not necessarily make this prediction. And consideration of non-wage attributes that may deteriorate when minimum wages increase (as emphasized by Clemens, 2021) can also rationalize non-negative employment effects without a positive search response.[5] Finally, most of this work is based on search among the unemployed only, for whom a positive response is plausible and perhaps most likely. However, one point we emphasize in this paper is the large amount of job search by the employed (based on Faberman et al., 2022), and why this search is likely to decline in response to a higher minimum wage.

Despite the many papers using search and matching models, there is very little direct evidence that higher minimum wages increase search for low-skilled jobs. Some research provides

[5] For related evidence, see Nagler and Winkler (2026).

indirect evidence, such as whether individual labor force participation responds positively to a higher minimum wage. Boffy-Ramirez (2019) finds that labor force participation decreases in response to a higher minimum wage, which could imply reduced search. Dube et al. (2016) report evidence that a higher minimum wage reduces both separations and accessions among affected workers. They argue that this is consistent with a model with search frictions – in particular a job ladder model where higher minimum wages reduce the arrival rate of better-paying jobs. Wursten and Reich (2023) present evidence that higher minimum wages increase commuting via automobile by Black workers, which they interpret as reflecting a wider job search radius. We do not regard any of this evidence as establishing decisively whether aggregate job search increases in response to higher minimum wages.

We have identified two papers that provide more direct evidence on time spent searching for jobs – on both the intensive and extensive margins. Piqueras (2023) uses combined data from the CPS and the ATUS and reports that higher minimum wages increase intensive search effort (time spent searching). However, he finds that individuals who increased search effort did not find jobs faster. Through the lens of a search model, he interprets this as reflecting a corresponding reduction in labor market tightness. Adams et al. (2018) also use CPS and ATUS data, as well as data from the SIPP. They find no impact of higher minimum wages on the likelihood of searching (their estimate is sufficiently precise to rule out even small increases) and only short-term spikes in search effort by those already looking for work.[6]

These last two papers provide perhaps a little support to the idea that increased job search because of a minimum wage increase could, in principle, lead to rising employment. However, Piqueras (2023) fails to find an employment effect, and Adams et al. (2018)'s results do not provide evidence of the sustained increase in search that would seem necessary to motivate firms to create additional jobs. Moreover, neither paper directly measures changes in job applications. Also, time-use data cannot distinguish whether more time spent searching reflects job seekers applying to more jobs in an expanded set of job postings, or additional search due to increased difficulty in obtaining a job offer (because the minimum wage reduces the number of jobs posted). In search models, the first effect could indicate that minimum wage increases

[6]In the published version of this paper, Adams et al. (2022) use only the ATUS data.

lead to more job seekers that could induce firms to post more vacancies and hire more workers, whereas the latter effect could simply reflect neoclassical labor demand effects. Finally, as noted above, both Adams et al. (2018) and Piqueras (2023) rely on survey data that does not fully capture search by employed workers – who make up the majority of job seekers in low-wage labor markets – thereby missing important behavioral responses among those already working in minimum wage jobs.

In this paper, we use new, behavioral data ('clicks' rather than self-reported search) to study directly whether higher minimum wages increase job applications for low-skilled jobs. Our job application data come from a large online job search platform for narrow categories of low-skilled jobs, at the state-by-month level. The responses we can measure with these data correspond more closely to changes in search behavior that could – depending on the actual responses – lead firms to post more vacancies and potentially increase employment.

In our core results, we find that the 18.8% increase in the minimum wage in Hawaii caused an estimated 11% decline in job search for retail and cleaning occupations and an estimated 13.1% decline for food-related occupations on average over the following 17 months.[7] Similarly, we find that the 16.7% increase in the minimum wage in Nebraska caused an estimated 9.4% decline in job search for retail and cleaning occupations and an estimated 8.7% decline for food-related occupations on average over the following 11 months. These conclusions are robust to numerous alternative analyses, including those accounting for recent critiques of standard two-way fixed effects specifications.

These findings challenge a core contention in the minimum wage literature – that a higher minimum wage induces a positive search response among unemployed workers, which can in turn generate non-negative employment effects. Across many analyses, we do not find any evidence of statistically significant positive search responses, and the evidence points more to negative effects. Although we cannot parse the reasons (i.e., we cannot rule out a negative responses owing to lower returns to search for the unemployed, or a responses to deteriorated job characteristics), we believe that the contradictory evidence may be most attributable to on-the-job workers being responsible for a large part of job search efforts (consistent with

[7]Retail and cleaning occupations include cashier, retail salespersons, janitor, cleaner, and receptionist job listings. Food-related occupations include crew member, cook, dishwasher, and host job listings.

Faberman et al., 2022). Employed job seekers likely outnumber unemployed job seekers, and they may be especially inclined to remain in their current jobs and reduce search activity when the minimum wage increases.

2 Conceptual Framework

The dominant focus of models of the minimum wage and job search, and of the arguments that search-and-matching models can explain non-negative employment effects of minimum wages, is on job seekers who are currently jobless (e.g., Gavrel et al., 2010; Gorry, 2013; Lavecchia, 2020). The effects of minimum wages in search models depend on the endogenous search responses of job seekers (Acemoglu, 2001). In simple formulations, search-and-matching models predict that a higher minimum wage increases the number of job seekers, all else equal. This prediction is clearest in a context where only jobless workers search. Leading examples of such models include Flinn (2006) and Manning (2005, Chapter 12). By increasing labor supply in a labor market equilibrium assumed to have unmatched job seekers and job vacancies, increases and improvements in job matches may mitigate or even offset the negative employment effects of the minimum wage. This theoretical argument is the basis for search-related explanations of the empirical findings from the minimum wage employment literature that sometimes point to no effect or even positive effects of minimum wages on employment (the employment effects do not always have to be non-negative in the search-and-matching framework).

However, the theoretical prediction is actually ambiguous except in restrictive models. When the job finding/job arrival rate is fixed and exogenous, a higher minimum wage increases the reward to working but does not impact the likelihood of finding a job, and hence also increases the return to search. However, when the job finding rate is endogenous, the returns to search can change – and in particular, decline. For example, Mincer (1976) models the probability of finding a job in the covered sector (where the minimum wage binds) as a function of the separation rate, employment in the covered sector, and unemployed workers searching for minimum wage jobs (which can increase in response to a higher minimum wage). In his model labor can flow out of the covered sector (and not increase as much in the uncovered sector because the wage there falls), potentially reducing overall employment and making job search less attractive. In the model in Flinn (2006), the effects of a higher minimum wage differ substantially depending on whether the "contact" rate is assumed exogenous (and Flinn notes that a general equilibrium perspective implies an endogenous contact rate, p. 1059).[8] Lavecchia's (2020) model is perhaps clearest, modeling the externality from search congestion, which can lower the job finding rate when the minimum wage increases, depending on how the higher minimum wage also impacts the job vacancy matching rate (which increases hiring and output).

In addition, recent work (Clemens, 2021; Clemens et al., 2021) emphasizes the role of non-wage compensation and job amenities in mediating the effects of minimum wages. The core idea is that a higher minimum wage can lead to lower benefits, perhaps most notably health insurance – consistent with the evidence in Marks (2011) and Clemens et al. (2018). A higher minimum wage can also change job amenities and conditions towards those that are more favorable to employers and less favorable to workers – like higher effort requirements, as can occur in the model of Coviello et al. (2022) and Ku (2022), less scheduling flexibility for workers, or diminished workplace safety (Liu et al., 2024; Davies et al., 2025). The demand curve shifts out from these changes (because they reduce labor costs). But the changes in job characteristics to the detriment of workers would lead to the labor supply curve shifting in. The net effect is that the low-skilled labor market can arrive at a new equilibrium in which employment is not necessarily lower, and fewer workers are interested in minimum wage jobs – i.e., search is lower, not higher. While these models of other responses to minimum wages are not search models per se, they provide another reason there can be non-negative employment effects of minimum wages without having to appeal to search models that could – under some circumstances – explain the same evidence.

Thus, there is theoretical ambiguity as to whether search frictions imply that a higher minimum wage increases job search by the unemployed. And consideration of a richer set of job attributes than just the wage – as emphasized by Clemens (2021) – also does not generate a clear prediction that higher minimum wages increase search.

Finally, once we allow for on-the-job search, which Faberman et al. (2022) show is the

[8] Flinn (2006) does not find evidence that the contact rate changed, but notes that this evidence is limited to a small policy intervention (minimum wage change).

bulk of job search, the prediction that a higher minimum wage increases search is further undermined.[9] Moreover, they show that search effort from the employed is also more elastic. Thus, the net effect of minimum wage changes on the overall number of job seekers will likely depend on its effect on this largest and most responsive group of potential job seekers.

In particular, the model in Liu (2022) suggests that job search from low-wage workers will decline following a minimum wage increase. This result comes from two channels that are modeled as occupational mobility but carry over to search more generally (Liu, 2022). The first is a "wage compression" channel: minimum wage increases reduce wage dispersion, decreasing the benefit from switching jobs and thereby reducing the number of job transitions.[10] The second is via employment effects: Minimum wage increases reduce job postings, and the reduction in the likelihood of job accession reduces the incentive for individuals to engage in job search. However, in many search models, as discussed above, postings can increase in response to a higher minimum wage.

Thus, there is an additional theoretical reason – via wage compression – that workers in minimum wage jobs will reduce job search in response to a minimum wage increase. Given the evidence from Faberman et al. (2022) that most low-skilled job search comes from already-employed individuals, the overall effect of a minimum wage increase may be more likely to decrease rather than increase search.[11]

Therefore, while the argument is often made that a positive job search response can help us understand non-negative employment effects of the minimum wage, there may in fact be a weak case for predicting a positive search response, and the actual job search response is an empirical question that depends on the sizes of the two groups of potential job seekers, and the magnitudes (and directions) of their responses to higher minimum wages.[12] In other words,

[9] This is also noted in Flinn (2006) and Flinn and Mabli (2008).

[10] Although not relevant to our inquiry, this effect also leads to greater occupational mismatch.

[11] These search responses – both from wage compression affecting employed workers and from reduced job-finding rates affecting unemployed workers – may vary with labor market conditions. Effects are likely smaller in tight markets, where job-finding prospects remain strong, and larger in slack markets, where the ratio of job seekers to vacancies increases. This remains an important area for future research (see Clemens and Strain (2021) for evidence on employment effects varying by labor market conditions).

[12] In addition to jobless individuals and those employed in minimum wage jobs, a third group of labor market participants comprises those employed in non-minimum wage jobs. While our data cannot distinguish between these worker types, the literature acknowledges their relevance. For example, Phelan (2019) demonstrates that minimum wage increases can alter the composition of compensation by changing the mix of wages and amenities, potentially making certain lower-wage jobs more attractive to workers previously employed in jobs paying

ultimately the impact of a higher minimum wage on job search is an empirical question – one that the novel data in this paper are uniquely situated to answer.

Our empirical findings of reduced search activity following minimum wage increases can thereby be interpreted through this conceptual framework. The negative search response is inconsistent with theoretical predictions of increased search from higher wages and instead suggests that other mechanisms play a more important role in determining search behavior. As discussed above, this evidence is consistent with either: (1) reductions in job vacancy rates that lowers the expected returns to search for unemployed workers; (2) wage compression effects that reduce job search incentives for currently employed workers, who constitute the majority of searchers in low-wage occupations (Faberman et al., 2022), or (3) deterioration in non-wage job characteristics that make minimum wage employment less attractive overall.

3 Data

3.1 Data from Online Job Search Platform

Our primary data source is one of the three largest job search platforms in the United States, with hundreds of millions of monthly visits and millions of job postings across all major occupational categories. The platform contains job postings compiled through a combination of direct employer submissions, sponsored listings, and aggregating (scraping) publicly available job ads from company career pages and other job boards. It reaches over 90 percent of online U.S. job seekers, suggesting that any substitution across platforms is likely to be modest in scale.

We obtained monthly data at the state level on the number of job seekers accessing individual job postings from January 2021 through September 2023. On this platform, job postings are sorted into occupational groups based on the job search platform's algorithm. This should ensure similarity across states. We target occupations likely to be affected by the minimum wage, paralleling the existing literature on minimum wages and employment.

In particular, we focus on occupations with a low hourly wage to make it more likely that minimum wages would affect wages in those jobs. We identified the occupations with mean

somewhat above the minimum wage.

hourly wages below $18 per hour using the May, 2023, National Occupational Employment and Wage Estimates (U.S. Bureau of Labor Statistics, 2023a). Table A.2 in the Appendix shows the occupations with lowest mean hourly wages.

We collected data on job seekers for job titles that match with low-wage occupations and that have at least 10,000 monthly job seekers nationwide in the platform. Based on the job categories listed separately on this platform, we obtained job search data for the following job titles: cashier, retail salespersons, janitor, cleaner, receptionist, crew member,[13] cook, dishwasher, and host. To avoid unnecessary specificity, our primary analysis aggregates these occupations into either retail and cleaning or food-related occupational categories, as follows:[14]

- Retail and Cleaning Occupations: cashier, retail salespersons, janitor, cleaner, receptionist
- Food-related Occupations: crew member, cook, dishwasher, host

For any given state-month observation, our data provide a measure of search activity on the extensive margin, indicated by the number of unique job seekers for job listings in the given occupational category. The number of job seekers is measured by counting the unique platform users – identified via tracking cookies – who clicked on a job posting's thumbnail. Clicking provides additional information about the job, which we use as evidence of individual-level job search activity for that particular job. The tracking cookies ensure that in the data we do not double-count a platform user who clicks on multiple job postings with the same title; within each job title, each unique job seeker is only counted once. However, our aggregation of job titles within occupational classes may lead to limited multiple-counting of the same unique user if, for example, that person clicked on job postings with the crew member, cook, and dishwasher job titles within the same month. We anticipate any such effect would be relatively constant across space and time and should not impact our estimated effects of minimum wages.

Our data cover January 2021 through September 2023. The start date chosen avoids using data from the first year of the Covid-19 pandemic, and coincides with the beginning of the labor market recovery from the pandemic. The seasonally-adjusted U.S. labor force participation

[13] Crew-members typically refer to individuals employed in entry-level positions within the food service industry. Their responsibilities often include customer service, basic operational tasks, and maintenance duties. Nine out of the 10 top employers in the United States hiring workers with this job title were fast-food restaurants.

[14] Table A.3 shows the comparison of the job titles and the occupations (and wages) associated with them.

rate of 61.3% in January 2021 was the lowest value of the pandemic, apart from the immediate crash and rebound experienced in April 2020. Starting from January 2021, the U.S. labor force participation rate increased at a generally steady rate until September 2023, when it peaked at 62.8% (U.S. Bureau of Labor Statistics, 2023b).[15]

While it is true that smaller independent establishments in low-wage industries such as restaurants still rely on “help wanted” signs and onsite applications (Azar et al., 2024), recent evidence suggests that the sector is rapidly shifting toward online recruitment channels. Reports from the National Restaurant Association, including full-service and fast-food restaurant operators, indicate that major chain restaurants (especially multi-unit operators) have largely migrated to online systems to handle applications and speed up hiring (National Restaurant Association, 2025). For example, in 2024, 86 percent of applicants for Southern Rock Restaurants, which owns McAlister’s Deli franchises, used their mobile devices to apply for a job. Chain restaurants like Wendy’s and McDonald’s (especially corporate-owned or larger franchise groups) state that prospective employees should apply through their official career websites.

3.2 Minimum Wages, Background and Treatment Timing

During the analysis period, Hawaii and Nebraska were the two states with newly-legislated minimum wage increases that also had sufficient pre-treatment data for analysis. We regard these as "unscheduled" minimum wage increases. In contrast, we define "scheduled" minimum wage changes to include both increases that had been previously established by specific legislative or ballot initiative language, typically mandating a multi-year set of increases, as well as changes – typically much smaller – that result from indexing the minimum wage level.[16]

Hawaii’s minimum wage increase was the result of state legislation, HB2510, which made the state the first to legislate a minimum wage of $18 per hour (slated to reach this level in 2028) (Pepper, 2022). The last previous minimum wage increase from $9.25 to $10.10 per hour had occurred on January 1, 2018 (that increase was the last of several steps that had been legislated

[15] Descriptive statistics are reported in Table A.1.

[16] For discussion of and evidence on indexed minimum wages, see Clemens and Strain (2018). Virginia passed legislation to increase their minimum wage in February 2020. We do not include Virginia in our analysis since we would not have pre-treatment data.

in 2014).[17] HB2510 was introduced on January 26, 2022, passed the House on March 8, passed the Senate in substantially different form on April 12, passed out of conference committee on April 29, and passed full votes of both chambers in early May (Hawai'i State Legislature, 2022). Hawaii's governor had already indicated his support and signed the legislation into law on June 22. The new minimum wage of $12.00 per hour (an increase of 18.8%) became effective on October 1, 2022.

This timeline suggests that workers had several months' advance notice that minimum wages might rise. Relative to the previous legislative actions, the conference committee vote attracted the most media attention and likely served as the clearest signal of the coming minimum wage increase.[18] Because expectations of a change in minimum wages in the near future could affect job search in the present (Karabarbounis et al., 2022; Jha et al., 2024), we use May as the treatment month for Hawaii.

Nebraska's minimum wage increase was the result of a ballot initiative (Nebraska Initiative 433, 2022) that passed on November 8, 2022, with 386,756 votes (58.66% of the total) (Ballotpedia, 2022). Initiative 433 was filed on August 20, 2021, and initiative organizers claimed to submit 152,000 signatures on July 7, 2022 (Ballotpedia, 2022). On September 6, 2022, the Nebraska secretary of state reported the campaign had submitted 97,245 valid signatures (out of 86,776 needed) and was eligible for the November ballot (Ballotpedia, 2022). Since there was no way for workers to know for sure that the ballot initiative would pass before November, we use November 2022 as the treatment month for Nebraska.[19]

Initiative 433 raised Nebraska's minimum wage 16.7%, from $9.00 per hour to $10.50 per hour, on January 1, 2023.[20] The last previous minimum wage increase was from $8.00 per hour

[17] At the time of HB2510's passage, Hawaii had already mostly eliminated the tipped credit. The minimum wage for workers receiving gratuities rose from $9.35 per hour to $11.00 per hour on October 1, 2022. HB2510 also scheduled subsequent increases for the non-tipped minimum wage of $14 per hour on January 1, 2024, $16 per hour on January 1, 2026, and $18 per hour on January 1, 2028. It scheduled increases for the tipped minimum wage of $12.75 per hour on January 1, 2024, $14.75 per hour on January 1, 2026, and $16.50 per hour on January 1, 2028 (Hawai'i State Legislature, 2014).

[18] Many news articles covered the approval of HB2510 in early May 2022. Hawaii Public Radio, for example, stated that "The House and Senate approved the measure by wide margins. The bill now goes to Gov. David Ige, who has said he supports an $18 minimum wage." See more here: https://www.hawaiipublicradio.org/local-news/2022-05-03/lawmakers-vote-to-raise-minimum-wage-from-10-10-for-the-first-time-in-over-4-years.

[19] Economists or politically-knowledgeable readers might reply that the vast majority of ballot initiatives to increase the minimum wage are successful. However, we do not assume that the average minimum wage worker would know and act on this information.

[20] Initiative 433 did not change Nebraska's tipped wage credit. The minimum wage for workers receiving gra-

to \$9.00 per hour on January 1, 2016, resulting from the November 4, 2014, passage of another ballot initiative – Initiative 425, which passed with 59.47% of the vote (Ballotpedia, 2014).

Our primary analysis involves a difference-in-differences strategy that compares the changes in low-wage job search in Hawaii and Nebraska to a group of control states that have not experienced a nominal minimum wage change since the last federal minimum wage increase in 2009. We focus on these control states with no recent history of minimum wage changes to avoid potential contamination of the controls by longer-term effects of the minimum wages, which have been highlighted in some recent research (Meer and West, 2016; Aaronson et al., 2018).[21]

Minimum wage increases in some of the remaining states are incorporated into additional analyses discussed below. But we exclude them from our core analyses of the Hawaii and Nebraska minimum wages because they had previously scheduled minimum wage changes during the period of analysis, changed their minimum wage laws a few years before the time period of our analysis, or had an unscheduled minimum wage increase right before the period of analysis (this is only the case for Virginia).[22]

4 Empirical Strategy

4.1 Baseline Analysis

We first consider an ordinary least squares regression with two-way fixed effects (state and month-year) where all states are included. We examine the effect of changes in minimum wages on the number of job seekers for low-skilled jobs. We estimate the specification:

$$Log(\text{JobSeekers})_{it} = \beta_1 \times Log(\text{MinimumWage})_{it} + \gamma_i + \sigma_t + e_{it}, \tag{1}$$

where i indexes the state and t indexes each unique month-year. $Log(\text{JobSeekers})_{it}$ represents the natural log of the number of unique job seekers in each of our two occupation categories in

tuities remains \$2.13 per hour (equal to the federal minimum wage for such workers). It also scheduled additional minimum wage increases to \$12.00 per hour on January 1, 2024, \$13.50 per hour on January 1, 2025, and \$15.00 per hour on January 1, 2026. It indexed further annual increases to the CPI-U of the Midwest Region.

[21]There was inflation during our sample period, which eroded the value of minimum wages in both treatment and control states. However, as long as this inflation is similar across states, which seems true to the first-order, inflation should not lead to any parallel trends violation.

[22]Table A.4 in the appendix shows the minimum wages for each state from 2018 to 2023 and indicates how a given state is classified for our different analyses.

a given state in a given month-year.[23] $Log(\text{MinimumWage})_{it}$ is the natural log of the nominal minimum wage for each state in each month-year. γ_i represents a vector of state fixed effects, σ_t represents a vector of month-year fixed effects, and e_{it} represents the error term.

In this initial analysis, we include all 50 states regardless of when legislation was passed to change their minimum wages and regardless of whether the changes in minimum wages were scheduled or not. There are potential complications in this analysis. For example, the effects of scheduled minimum wage increases may differ because expectations of job seekers in these states could lead to changes in behavior well before the actual minimum wage increases occur. In addition, estimation of minimum wages effects using this approach could be prone to biases highlighted in the recent difference-in-differences econometrics literature (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021), such as dynamic treatment effects contaminating later untreated observations that serve as controls. There are solutions to these issues applicable to the simpler analyses we do of the Hawaii and Nebraska minimum wage increases, which are not applicable to the more general setting with frequent and continuous minimum wage changes. Nonetheless, this evidence is useful as a point of comparison with our primary analyses, and because past studies of the effects of minimum wages on other outcomes (mainly employment) use this empirical strategy (Gopalan et al., 2021).

4.2 Primary Analyses

Our primary analyses consist of two-way fixed effects (TWFE) specifications for Hawaii and Nebraska. We examine each treatment separately to avoid any biases that may be caused by heterogeneous treatment effects. The minimum wage increases in these states were not scheduled prior to the analysis period and sufficient data exists to examine pre-treatment changes or trends for each state. We estimate ordinary least squares regressions with state and month-year fixed effects, using as untreated observations the 20 states without prior scheduled minimum wage changes during the analysis period. We use the specification:

$$Log(\text{JobSeekers})_{it} = \beta_1 \times \text{Treated}_{it} + \gamma_i + \sigma_t + e_{it}. \quad (2)$$

[23] We use a logged dependent variable because the scale of the number of job seekers varies greatly based on the population size of each state.

Treated is an indicator variable that takes a value of 1 for the time periods after which a state is treated with a minimum wage increase. We determine the start of the treatment period as the point in time at which job search behavior is likely to have changed in response to the coming minimum wage change (this is prior to the new minimum wage's implementation date, see Section 3.2). We analyze the treatments in Hawaii and Nebraska separately because the magnitude and duration of the minimum wage change (i.e., scheduled later increases) differs between the states. The estimator from this specification provides the average causal effect of the minimum change on job search behavior over the treatment period, meaning that combining differing treatment periods may impede accurate inference. Because we only consider one treated state at time in our primary analysis, we do not face any issues associated with heterogeneity in treatment timing described by the recent difference-in-differences econometrics literature.[24]

Because these regressions have only one treated cluster, we use randomization inference to obtain more accurate information on statistical significance and confidence intervals than we obtain from standard cluster-robust standard errors (MacKinnon and Webb, 2019, 2020). This follows recent minimum wage research by Clemens et al. (2025). Specifically, we re-assign the timing of minimum wage increases to control states, re-estimate our baseline specifications, and compare the distribution of these estimates (the "empirical randomization distribution") to the treatment effects we obtain. This approach has two advantages. (1) First, it directly reflects the assignment mechanism we exploit in our empirical design, yielding exact finite-sample confidence intervals under the null of no effect. (2) Second, randomization inference does not rely on asymptotic approximations with respect to the number of clusters, making it especially well-suited to settings like ours with few treated units.

We report both asymptotic and empirical confidence intervals. Asymptotic confidence intervals are centered on the placebo mean from the empirical randomization distribution, which is constructed under the assumption that the placebo estimates are approximately normally

[24] Although not reported in the paper, we have estimated our core specifications with demographic controls, including the median age of respondents, the share of female respondents, the share of white respondents, the share of households residing in metropolitan areas, and the mean number of children per household. Descriptive statistics for these controls are reported in Table A.1. Results are nearly identical with the inclusion of these controls.

distributed, so that the Central Limit Theorem applies. Accordingly, the interval is computed as $\mathrm{CI}_{90} = \bar{\beta}_{\text{placebo}} \pm 1.64 \times SD_{\text{placebo}}$, where *Mean Placebo ATT* is $\bar{\beta}_{\text{placebo}}$ and *Placebo SD* is SD_{placebo}. *Mean Placebo ATT* the mean of the 20 placebo estimates and *Placebo SD* is the standard deviation of the distribution of ATT estimates. This CI interval reflects uncertainty under the randomization distribution rather than sampling variation. Empirical confidence intervals are constructed using the order statistics of the 20 placebo ATT estimates. Specifically, they span from the second-lowest to the second-highest estimate, corresponding to the central 90% of the empirical randomization distribution.[25]

4.3 Pooled Analysis

Next, we include data from both states in our analysis, enhancing the efficiency of our estimates. We begin with a TWFE difference-in-differences estimation identical to specification (2), but with the inclusion of both Hawaii and Nebraska as treated states. However, various studies have demonstrated the potential limitations of TWFE in estimating the average treatment effect on the treated (ATT) in scenarios like ours where treatment adoption is staggered over time and treatment effects may vary across groups and over time (De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Our primary analyses are robust to these potential problems since we consider one treated state at a time. When including both treated states in the analysis, however, we need to consider possible biases highlighted in this recent work. Thus, we provide a robustness check by comparing our results with those produced by alternate estimators.

We use the doubly robust (augmented inverse probability weighting) method described in Callaway and Sant'Anna (2021) (C&S) and the two-way Mundlak (TWM) method described in Wooldridge (2021) as alternatives for the results produced from standard TWFE estimation. The Callaway and Sant'Anna estimator allows for estimation and inference of interpretable causal parameters even in the presence of arbitrary treatment effect heterogeneity and dynamic effects, circumventing the potential problems arising from interpreting the results of standard two-way fixed effects regressions as causal effects. The TWM estimator also allows unbiased

[25] With only 20 placebo estimates, no estimate can be outside of the empirical (two-sided) 95% confidence interval.

estimation in the presence of heterogeneous effects across different treatment intensities and varying treatment timings. We implement these estimators in an extension of specification (2) that includes both treated states (Hawaii and Nebraska) and the control states. We also implement these estimators in a extension of specification (2) that examines each occupation individually (described later). We compare the results obtained from the Callaway and Sant'Anna estimator and the TVM estimator with those obtained using our standard TWFE specification (with fixed effects for month-year and state).

In each case, we again implement randomization inference. In this case, we extend the randomization inference framework to include both treated states in our analysis, accounting for the fact that Hawaii and Nebraska were treated at separate times. We construct a "joint empirical randomization distribution," using the pool of 20 control states (excluding Hawaii and Nebraska) and iteratively selecting all possible unique pairs of these controls. In each iteration, one control state is assigned to take the timing of Hawaii's treatment (May 2022) and the other to take Nebraska's timing (November 2022). The model is then re-estimated using these two placebo-treated states, and the resulting coefficient is stored as a joint placebo ATT. Since the procedure considers all unordered combinations of two unique control states from the 20 available, it generates $\binom{20}{2} = 190$ unique placebo ATT estimates, which together form the joint empirical randomization distribution against which the observed pooled estimate is compared. We do this for each of the three estimators.

4.4 Additional Treated States

Finally, although we focus on the large and unscheduled Nebraska and Hawaii minimum wage increases, it is of course useful to know what we obtain from applying similar methods to the other states with minimum wage increases in the period covered by our data, to see if we generally obtain similar estimates from other states. We therefore select additional treated states that implement a discrete, relatively large minimum wage increase between January 2021 and September 2023. To ensure a well-defined event, the increase must be preceded by a period with either no change or only very small increases (approximately 1–2 percent), and any larger prior increase must have occurred more than one year earlier. We treat the implementation of the initial large increase as the event of interest, since some of these were scheduled increases.[26] This list includes eight states: Delaware, Florida, Maine, Montana, Ohio, Rhode Island, South Dakota, and Virginia.[27] Table 1 lists treatment month that corresponds to the large minimum wage increase in the sample period, the dollar amount of said increase, the percentage change, and the type of minimum wage increase observed. For this analysis, we use the same methods of estimation and inference as for the primary analyses of the Nebraska and Hawaii minimum wage increases.

5 Results

5.1 Baseline Analysis: All States

Table 2 provides the results associated with specification (1) and include all U.S. states. We regard this specification as providing an imperfect national baseline that connects our study to prior research.

The baseline results suggest that minimum wage increases are associated with decreased job search in low-wage occupations. The elasticity of job search for retail and cleaning jobs with respect to changes in the minimum wage is estimated to be -0.144 during our period of analysis. The elasticity of job search for food-related jobs is estimated to be -0.276. Both estimates are economically meaningful and display moderate precision, although only the estimate for retail and cleaning occupations is significant (at the 10% level).

5.2 Primary Analysis: Hawaii

This subsection examines the causal effect of the increase in the minimum wage in Hawaii from \$10.10 to \$12.00 per hour (an 18.8% increase). Table 3 provides the results from a simple 2x2 difference-in-differences approach where we compare the average logged number of job seekers per month in each occupational class in Hawaii before and after the effective announcement of the coming change in minimum wage laws (May 2022) to the same metric

[26] See the details of these minimum wage increases in Table A.4.

[27] Virginia's May 2021 minimum wage increase is also included in the analysis of additional treated states. Although the increase was unscheduled, it was announced immediately prior to the start of our sample period, which precludes Virginia from the primary analysis. However, the implementation occurs within the sample window and is observed with pre-implementation data. Since treatment timing is defined by the implementation date for analysis of additional treated states, we include Virginia in this analysis.

averaged across the control group.[28] These initial results indicate that the number of job seekers for retail and cleaning jobs decreased by about 9.2% on average relative to the control group over the 17 months after the minimum wage increase was signaled. Job seekers for food-related jobs decreased by 13.0%. Both of these results suggest that the increase in minimum wages in Hawaii had a negative effect on job search.

In Figure 2, the logged total monthly job seekers in each occupational classification in Hawaii is compared against the same metric averaged across control states prior to and following the effective announcement of the change in Hawaii's minimum wage laws (May 2022). The series for the treated and control observations are indexed to 100 at the last pre-treatment observation. Figure 2 shows that prior to the treatment, the logged total monthly job seekers in Hawaii in each occupational classifications followed a trend similar to that of the control group. In the post-treatment period Hawaii continued to follow a similar overall trend as the control group, but with visual and measurable differences indicating declines in search. This visual examination helps to illustrate how Hawaii's minimum wage increase led to a decrease in the number of job seekers for low-skilled jobs in both occupational classifications, although the decline in retail and cleaning occupations appears to occur more quickly.

Table 4 provides the results of the two-way fixed effects estimation associated with specification (2). This estimation does not suffer any biases that could be caused by heterogeneity in treatment timing or effect because we consider solely the effect of a single minimum wage increase in Hawaii relative to the control states, which experienced no nominal minimum wage changes since the last federal minimum wage increase in 2009.

The results suggest that the increase in minimum wages in Hawaii caused a decline of around 11% and 13% in the number of job seekers in retail and cleaning occupations and food-related occupations across the period from April 2022-September 2023. All estimates are statistically significant at the 1% level. The computed elasticity of the change in the number of job seekers relative to the minimum wage change is -0.61 for retail and cleaning occupations and -0.72 for food-related occupations.[29]

[28] May 2022 and all months thereafter are part of the treatment period. The months prior to May 2022 are included in the pre-treatment average.

[29] These values are calculated as arc elasticities. We convert the difference-in-difference log-point estimates to percentage changes and divide by the arc percent change in Hawaii's minimum wage.

Table 4 also reports details on the statistical inference. The first entry below each estimate is the cluster-robust standard error, which we know is too small. Next, we report the rank of the estimate (from most to least negative) for the treatment state and the controls. For example, for retail and cleaning occupations, this rank is 2, meaning that only one control state had a more negative estimate (New Hampshire, with an estimate of -0.117), while 19 states had estimates that were smaller negative, or positive (ranging from -0.076 to 0.101). These ranks correspond to exact p-values, so with 21 estimates, a rank of 2 would imply statistical significance at the 10% level but not the 5% level. Finally, the last three rows report the mean placebo ATT and the two confidence intervals. In this case, we see that the -0.110 estimate for Hawaii retail and cleaning occupations is significant at the 10% level in both cases (with the empirical confidence interval corresponding to the conclusion just noted based on the rank of the estimate).

5.3 Primary Analysis: Nebraska

We next examine the causal effects of the increase in the minimum wage in Nebraska from $9.00 to $10.50 per hour (a 16.7% increase). The analysis is otherwise the same as the preceding one for Hawaii. Table 5 provides the results from a simple 2x2 difference-in-differences approach where we compare the logged total monthly job seekers in each occupational class in Nebraska before and after the month of the effective announcement of the coming change in minimum wage laws (November 2022) to the same metric averaged across the control group. This initial analysis suggests that the average monthly number of job seekers in Nebraska decreased by about 8.4% for both occupational classifications over the 11 months after the minimum wage increase was signaled.

In Figure 3, the logged total monthly job seekers in each occupational class in Nebraska is compared to the same value averaged across control states, as before. This graph illustrates that prior to the treatment the number of low-wage job seekers in Nebraska followed a similar trend to the average trend of the control group. After treatment there is a small but noticeable negative deviation between the estimated counterfactual line and the actual number of job seekers for both food-related and retail and cleaning occupations.

Table 6 shows the results of the two-way fixed effects estimation associated with specification (2). The results suggest that the number of job seekers for retail and cleaning occupations declined by more than 9.0% over the 11 months (November 2022-September 2023) following the signal of the minimum wage increase. Job seekers for food-related occupations decreased by 8.7%. The computed elasticity of the change in the number of job seekers relative to the minimum wage change is -0.58 for retail and cleaning occupations and -0.54 for food-related occupations.[30] Note that the estimated elasticities for retail and cleaning occupations are quite close in Hawaii and Nebraska, differing by only -0.02. In contrast, the elasticity for food-related occupations is a good deal larger in Hawaii (-0.69 vs. -0.51). It is conceivable that this is because Hawaii does not have a lower minimum wage for tipped workers, whereas Nebraska does and the tipped minimum wage did not change when the regular minimum wage went up. Thus, the Nebraska minimum wage may have resulted in somewhat more substitution from the limited-service to the full-service sector, and less of a decline in overall food-related employment.[31]

Table 6 also reports details on the statistical inference. For retail and cleaning occupations, the rank of the estimate for Nebraska is 2, implying statistical significance at the 10% level but not the 5% level. For food-related occupations, the rank is 4, meaning the ATT for these occupations is still relative large (in absolute value) relative to the placebo estimates, but the statistical evidence is weaker (reflected also in the confidence intervals reported in the table).

Notably, the state-specific (Hawaii and Nebraska) primary analyses find larger elasticities than the 50-state baseline analysis. This may be suggestive that large, discrete, unscheduled minimum wage changes produce larger effects than scheduled, discrete changes and marginal, index-driven changes. This could suggest nonlinear effects, since unscheduled minimum wage increases tend to be substantially larger than increases related to cost-of-living index changes. Alternatively, the difference may suggest that anticipatory effects mitigate the measured effect, since scheduled and index-driven changes are known in advance by both workers and employers.

[30] These values are, again, calculated as arc elasticities.

[31] This would be consistent with the evidence on tipped minimum wages in (Neumark and Yen, 2023).

5.4 Pooled Analysis

We now include both treated states in our analysis. We first consider a TWFE difference-in-differences estimation identical to specification (2), but with the inclusion of both Hawaii and Nebraska as treated states. As an alternative, we also utilize the difference-in-differences estimators described in Callaway and Sant'Anna (2021) and Wooldridge (2021). We compare these estimators against the results produced by a traditional two-way fixed effects (TWFE) specification (with fixed effects for year-month and state). In each of these specifications, Hawaii and Nebraska are compared against the previously used group of control states.

Panel A of Table 7 shows the ATT estimated via a standard TWFE analysis, Panel B shows the ATT estimated via the Callaway and Sant'Anna (2021) method, Panel C shows the ATT estimated via the Wooldridge (2021) TVM method. The average minimum wage increase across the treated states was 17.75% (18.8% for Hawaii and 16.7% for Nebraska). It is also worth nothing that the treatment period in Hawaii was 17 months compared to 11 months in Nebraska.

Each of the alternative specifications shows an economically meaningful and precisely-estimated decrease in the number of job seekers associated with the minimum wage increases. The parameters estimated using the Callaway and Sant'Anna method are smaller than those estimated via the TVM method and standard TWFE. However, all estimates show a substantial decline (between 6% and 11%) in the number of job seekers in treated states. The implied elasticity of job seekers with respect to the minimum wage ranges from about -0.32 to -0.67 across specifications and occupation groups when we compute elasticities using the Hawaii and Nebraska minimum-wage changes as denominator bounds.[32]

As for the separate results by state, we again report the results from the randomization inference in Table 7. Recall that in each case, the placebo treatment corresponds to the assignment of two control states as treated with timing corresponding to the Hawaii and Nebraska minimum wage increases. As we found for the two states individually, the statistical evidence

[32]These values are calculated as arc elasticities. We first convert each pooled difference-in-difference log-point estimate into a percent change in job seekers. Because the pooled analysis combines two treated states with different minimum-wage increases, there is no single treatment "dose"; we therefore report a range by scaling the job seeker response using the arc percent changes for both Hawaii and Nebraska separately, which bound the relevant minimum-wage change in the pooled setting.

is stronger for retail and cleaning occupations. In this pooled analysis, the estimated treatment effect's percentile is 1 for all three estimators, implying statistical significance at the 5% level, and the estimate is always outside the 90% confidence intervals (which we report for comparability with the previous tables). For food-related occupations the percentile ranges from 5 to 12, so the estimates, while negative, are not always statistically significant at the 10% level, although they are clearly among the larger (negative) estimates.[33]

Interestingly, the effects for retail and cleaning occupations are more precisely estimated than those for food-related occupations in almost each case. (And this, rather than differences in point estimates, is why the estimates are significant for retail and cleaning occupations but not food-related occupations.) This may reflect some food-related businesses facing relatively more inelastic demand from customers, being better able to pass along the increased labor cost and therefore relatively insensitive to minimum wage increases compared to the businesses hiring for retail and cleaning jobs (consistent with claims in Hirsch et al. (2015)). Regardless, our staggered adoption analysis suggests that the effect of similar, unscheduled minimum wage increases in two relatively dissimilar states was to meaningfully decrease the number of workers searching for low-wage jobs.

We next examine event-study plots associated with the staggered adoption analysis from the Callaway and Sant'Anna method (results shown in Panel B of Table 7). Figure 4 shows the event-study plots for both food-related occupations and retail and cleaning occupations. The red points to the right of the vertical dashed line indicate the value of the estimated ATTs for each post-treatment month. In both occupation classifications we see evidence of a decline in the number of job seekers. There is no apparent trend in the pre-treatment period (shown to the left of the vertical line). This supports the parallel trends assumption necessary for our causal estimation. The estimation of the ATTs shows statistical noise in both the pre- and post-treatment periods. However, the lack of a consistent trend in the pre-treatment period, and the clear decline in the post-treatment period, help bolster the results shown in Table 7.

[33] The full empirical randomization distributions are reported in Figure A.1.

5.5 Additional Treated States

Next, we turn to the evidence from the additional treated states. For this analysis, we also present estimates for Nebraska and Hawaii using implementation dates, to correspond more closely to the definition of the minimum wage treatment for the other states. In Tables 8 and 9 we report the estimates for each state, along with the inference results. Each estimate is from the TWFE estimator for the single treated states, relative to the controls (given that we saw, in Table 7, that the different estimators yield similar results); for additional evidence, we show Callaway and Sant'anna event-study plots below (pooling the states).

The most important finding is that the estimates are quite consistently negative. For retail and cleaning (Table 8), all but three of the estimates are negative, and the positive ones (for Ohio, Florida, and South Dakota) are small (in the 0.02 to 0.03 range) and not statistically significant. For food-related occupations (Table 9), the evidence is qualitatively similar, although there are four positive estimates and one (for Ohio) is a bit larger. Moreover, for the other eight treatment states, a number of the (negative) estimates are larger in absolute value, and some are statistically significant at the 10% level – Maine and Montana for retail and cleaning occupations, and Maine for food-related occupations. Overall, then, we regard the evidence from the other states as consistent with our findings for Nebraska and Hawaii: quite clearly there is no statistical evidence of a positive search response; and there is more evidence of a negative search response.[34] Moreover, we find no indication of positive effects, aside from a handful of very small positive effects for some states. And looking across states, our inference procedure rules out positive effects as large (in absolute value) as the bigger negative effects we estimate – based on the empirical confidence intervals in Table 8 and 9. To be sure, though, when we estimate effects for a single state and a single increase, the estimates are not that precise, and the larger positive estimates imply sizable elasticities.

Finally, Figure 5 shows the Callaway and Sant'anna event-study plots. These event-study plots point to negative effects that are quite persistent post treatment, with (like in Tables 8 and 9) stronger evidence of this for retail and cleaning occupations. Although the estimates are

[34] We have examined event study plots for each of these states, like Figures 2 and 3. For each state, the prior trends look quite parallel. These figures are available from the authors upon request.

smaller and less precise than in Figure 4, we interpret these event-study plots as reinforcing the absence of evidence of positive search responses and as providing suggesting evidence of negative effects, consistent with the results for Hawaii and Nebraska in Figure 4.[35]

5.6 Considering Individual Occupations

Last, we report results for narrower jobs within the food-related and retail and cleaning occupations. This is informative about whether the results are perhaps driven by a narrow set of jobs. This analysis also avoids the potential problem associated with counting the same job seeker more than once, since the data provides the unique number of job seekers for each job title (see discussion in Section 3). Given that the alternative to the TWFE estimator made little difference in the analyses reported in Table 7, here we use the TWFE estimator (continuing to include both states).

For jobs in the food-related classifications, we find a large decline in job seekers for crew member jobs (-0.20).[36] This estimate implies an elasticity of the number of job seekers for crew member jobs relative to changes in the minimum wage of between -1.05 to -1.18.[37] The estimated effects for hosts and cooks are negative but smaller, while the estimate for dishwashers is positive but close to zero; the latter three estimates are insignificant even with cluster-robust standard errors. Most important, there is no statistical evidence in this disaggregated analysis of food-related occupations indicating that higher minimum wages increase search.

The results for jobs in the retail and cleaning classification are even more consistent. The estimate is negative in every case, and for four of the five jobs the estimate is of a similar magnitude to the aggregate estimate.[38]

We also report the average numbers of monthly job seekers for each job. This shows which occupations may play a bigger role in our main estimates. Retail salespersons is the largest category, followed by cashiers, receptionists, crew members, and hosts. For all of these larger categories, the estimates are negative. And the only positive estimate (albeit small), is for

[35] Figures 4 and 5 report 95% confidence intervals based on cluster-robust standard errors.

[36] This is likely the group with lowest wages and more likely to be affected by changes in minimum wages.

[37] These values are calculated as arc elasticities, as described above for the analysis of the combined two treated states.

[38] Our focus here is on the point estimates; we do not report all of the corresponding randomized inference results.

dishwashers, one of the smaller categories. Overall, then, our conclusion is robust. There is no evidence that higher minimum wages increase search, and a good deal of evidence of an adverse effect on search.

6 Conclusion

Since the launch of the New Minimum Wage Research in the early 1990s, some economists have used theoretical job-search models to attempt to explain positive (or non-negative) estimates of minimum wages effects on employment. We present novel evidence on the effects of minimum wages on job search using data from a large online job platform, and focus in particular on the effects of two recent unexpected minimum wage increases on job search activity for low-wage jobs.

We find little if any evidence (and no statistically significant evidence) of the positive effect of higher minimum wages on job search for low-skilled jobs that is often claimed or asserted in appeals to labor market search models to explain non-negative employment effects. Instead, the evidence is more consistent with minimum wage increases lowering job search or perhaps having no detectable impact. These results provide evidence consistent with the wage compression channel in the search model of Liu (2022), wherein a reduction in wage dispersion due to the minimum wage dampens job search activity among the employed. Additionally, these findings align with the empirical results of Faberman et al. (2022), which highlight the prevalence and elasticity of on-the-job search, particularly among low-skilled workers. And if the decline in search occurs for unemployed workers specifically – which we cannot tell in our data – this could also be attributable to the reasons highlighted by Clemens (2021) whereby non-wage characteristics of jobs worsen for workers.

Our most credible results – for the unscheduled minimum wage increases in Hawaii and Nebraska – indicate possibly substantial decreases in low-wage job search activity caused by minimum wage increases. Notably, our results suggest an average elasticity of low-wage job search activity with regard to minimum wage changes that is more negative than -0.3, and likely more negative than -0.5. Such estimates are at least two and perhaps greater than four times as large as the common range of estimates for the employment elasticity (-0.1 to -0.3 (Brown et al., 1982) and -0.148 (Neumark and Shirley, 2022)). Our results for other states with less clean minimum wage "experiments" either point to negative effects or no effects of minimum wages on job search. We find little indication of positive effects, aside from a handful of very small positive effects for some states, and no statistically significant evidence of positive effects.

Our results are limited by the time period for which data were available and by the fact that some job search for low-wage jobs occurs in-person or through social networks, rather than via online job platforms. However, there is little reason to believe that job search activities would sharply diminish on online platforms while remaining constant or increasing via alternate channels. As a result, our work represents a challenge to the search-and-matching models predicting that higher minimum wages increase job search, as well as a challenge to appeals to these models to attempt to explain non-negative estimates in some studies of the effects of minimum wages on employment. Additional research and data will be useful to obtain more definitive evidence on the effects of minimum wages on job search.

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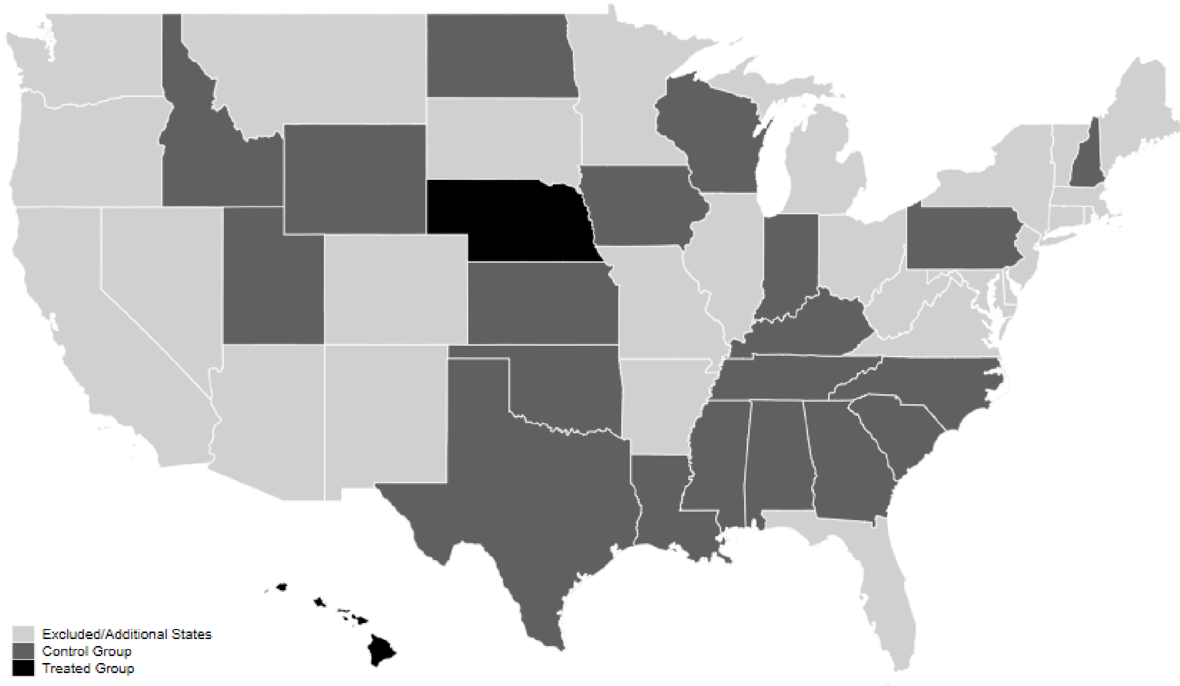
Figures

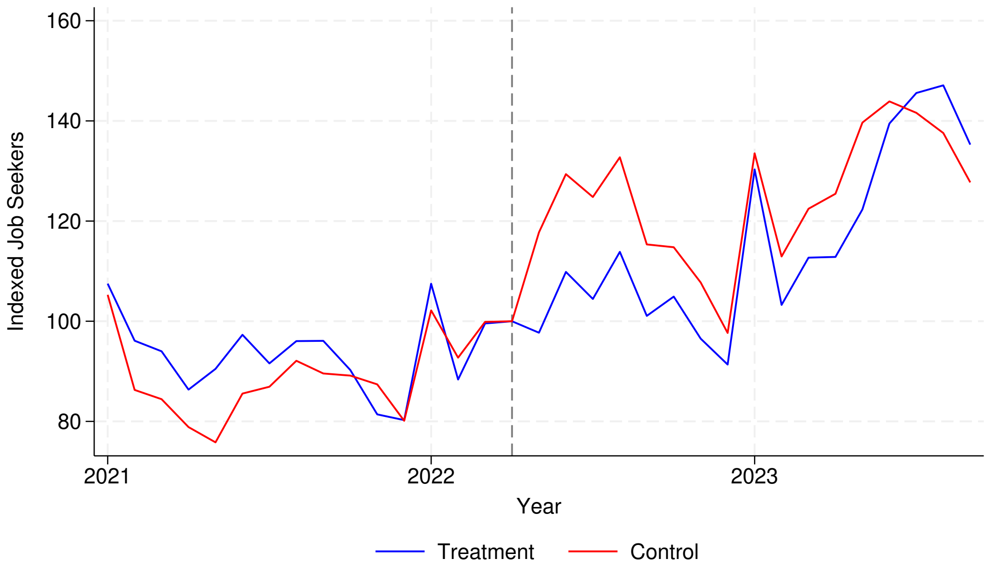
Figure 1: Treated and Control States



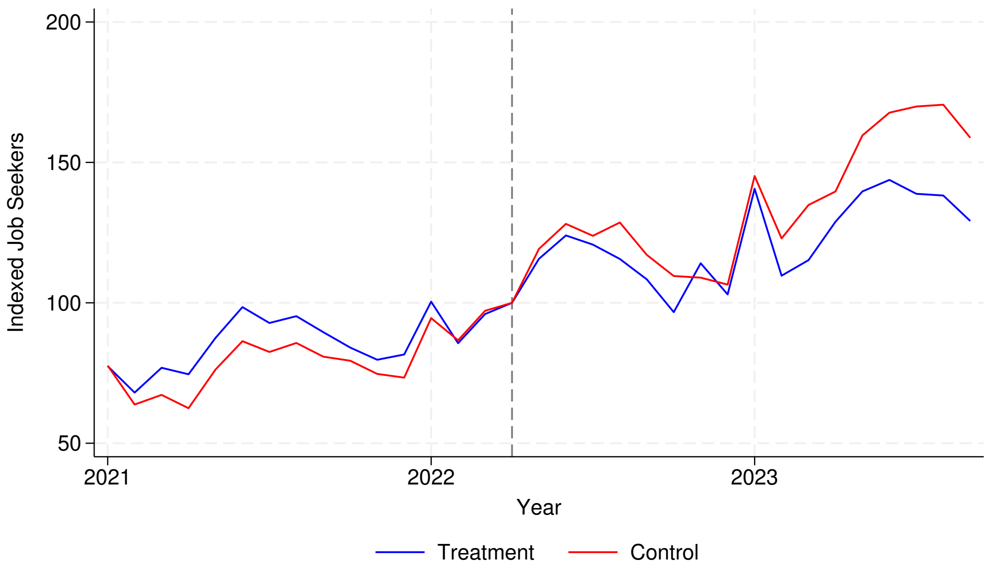
Notes: The map above shows the treated states for our primary analysis (Hawaii and Nebraska). It also shows the control states where the federal minimum wage prevailed throughout the period, and other states with minimum wage increases that we exclude from our primary analysis (including Alaska – not shown) or include in some of our additional analyses.

Figure 2: Indexed Job-Seeker Trends: Hawaii vs. Control States

(a) Retail/Cleaning occupations



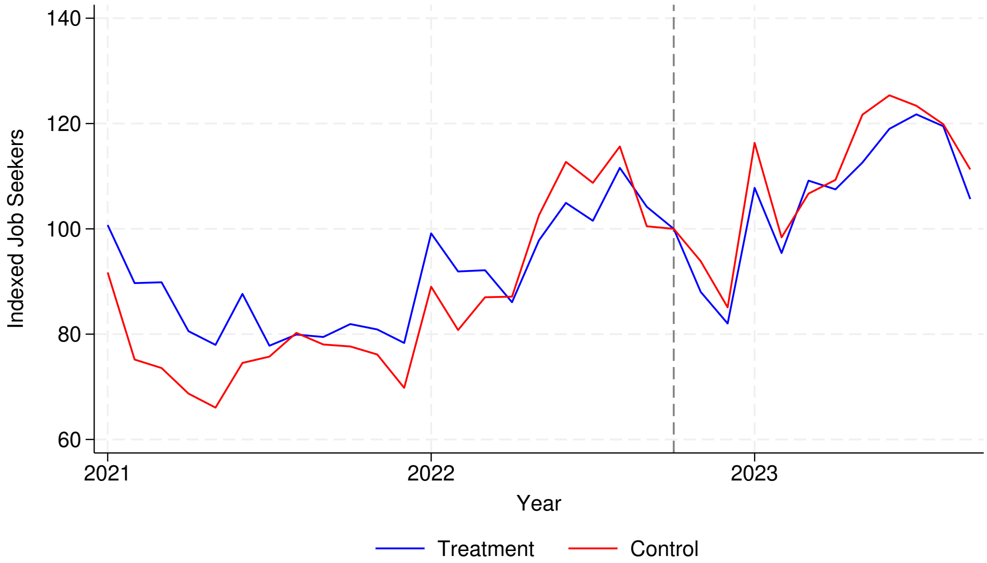
(b) Food-related occupations



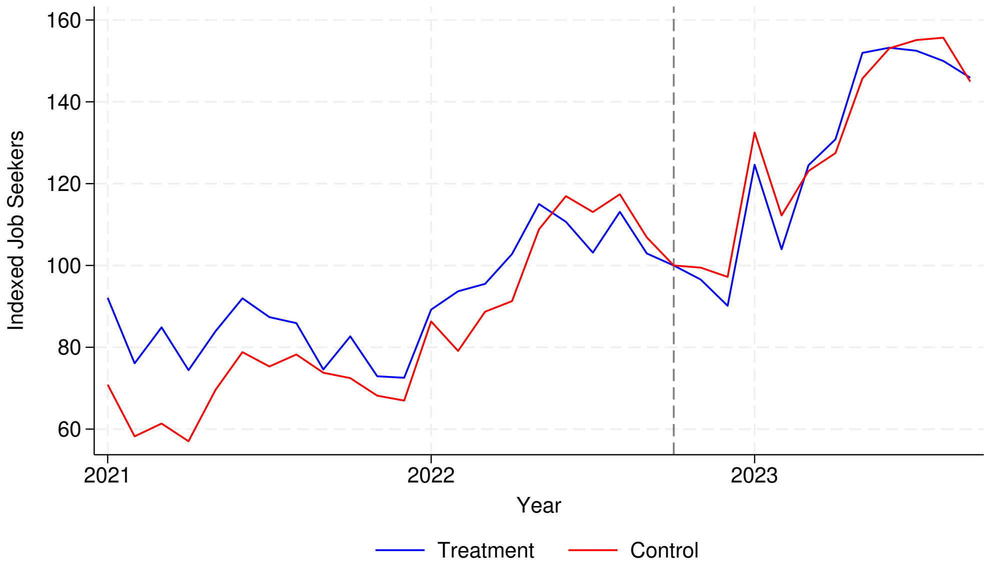
Notes: Each panel plots average monthly job seekers for Hawaii (treatment) and the control states (control), indexed so that each group equals 100 in the last pre-treatment month (vertical dashed line). Treatment timing is based on the May 2022 minimum-wage announcement.

Figure 3: Indexed Job-Seeker Trends: Nebraska vs. Control States

(a) Retail/Cleaning occupations



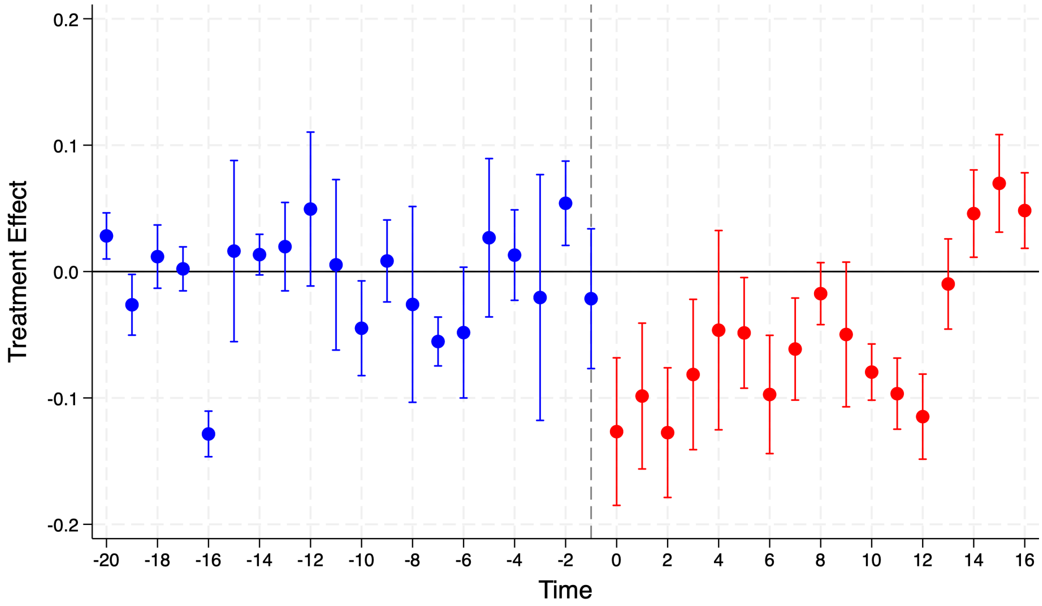
(b) Food-related occupations



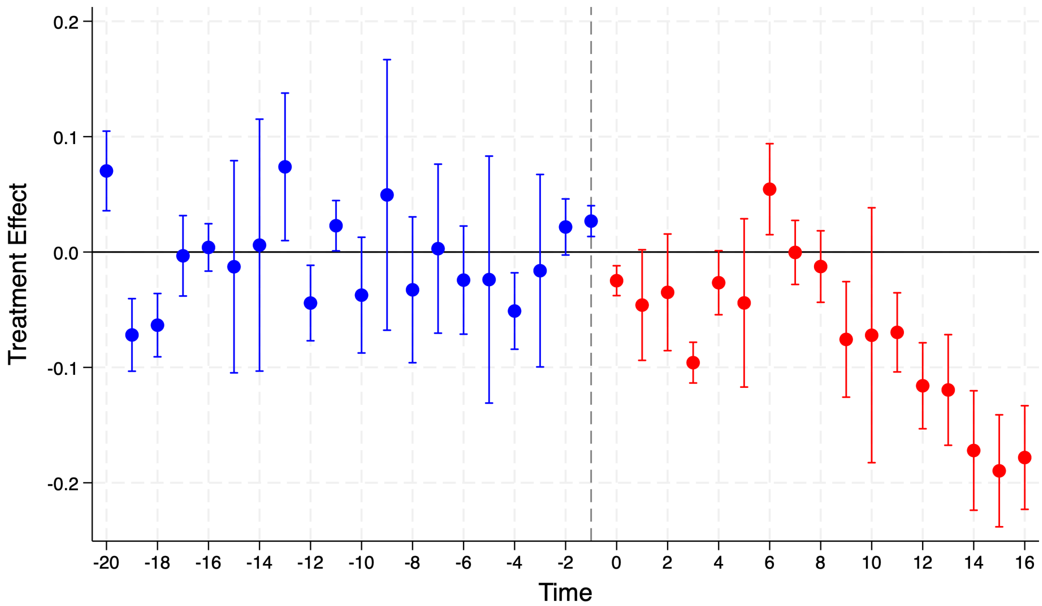
Notes: Each panel plots average monthly job seekers for Nebraska (treatment) and the control states (control), indexed so that each group equals 100 in the last pre-treatment month (vertical dashed line). Treatment timing is based on the November 2022 minimum-wage announcement.

Figure 4: Event Study – Callaway and Sant'anna Method

(a) Retail/Cleaning Occupations



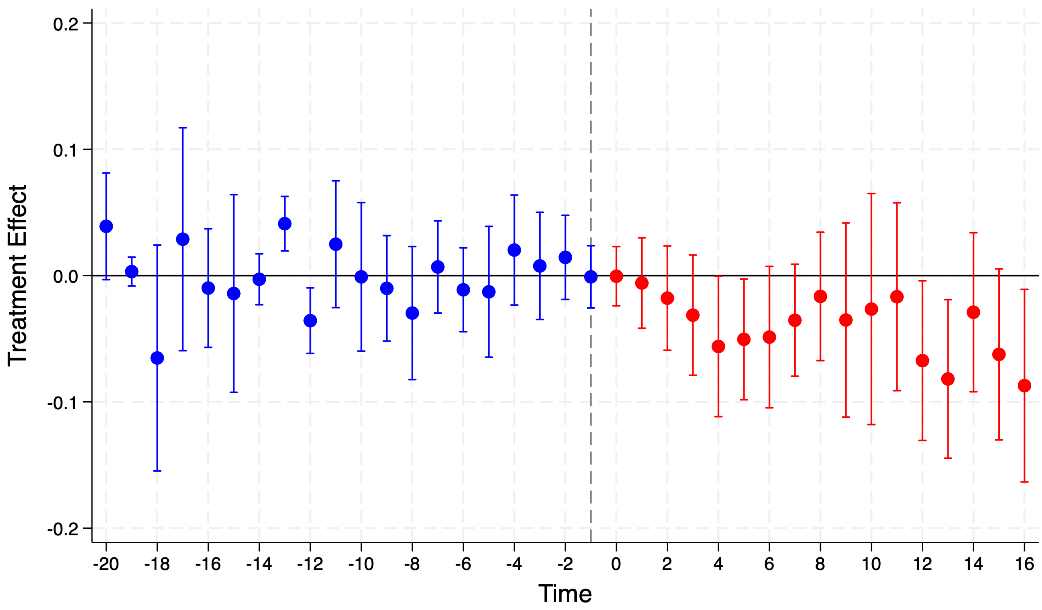
(b) Food-related Occupations



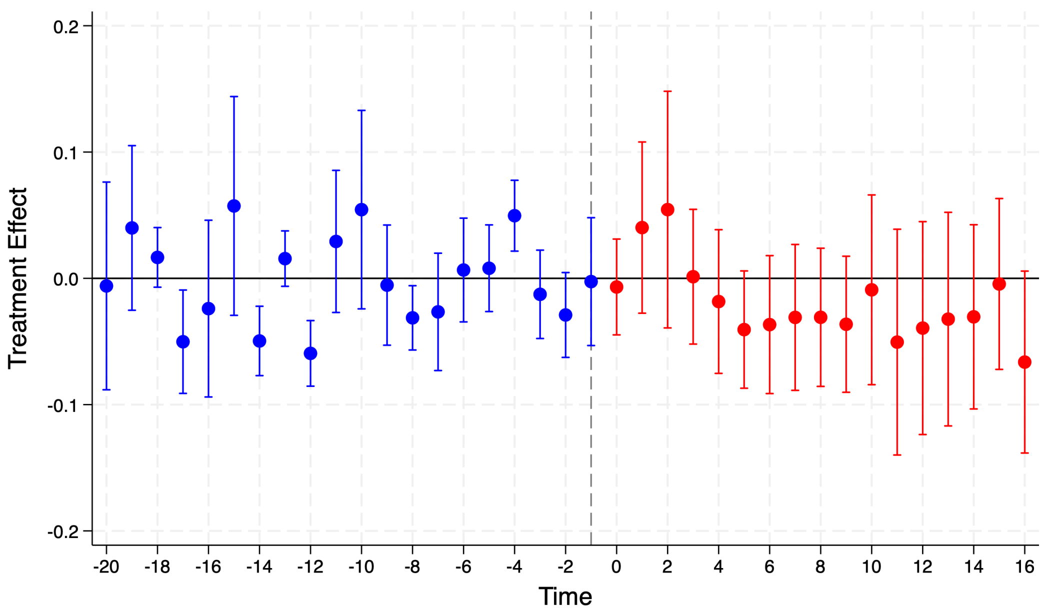
Notes: This figure shows the event-study plots based on the Callaway and Sant'Anna method shown in Panel B of Table 7. 95% confidence intervals are reported based on cluster-robust standard errors.

Figure 5: Additional States Event Study – Callaway and Sant'anna Method

(a) Retail/Cleaning Occupations



(b) Food-related Occupations



Notes: This figure shows the event-study estimates based on the Callaway and Sant'Anna method for the pooled specification with the additional treated states. 95% confidence intervals are reported based on cluster-robust standard errors. Treatment timing is based on the implementation date of the minimum wage per Section 4.4.

Tables

Table 1: Additional Treated States

State	**Treatment Month**	**MW Change**	**Percent**	**Type of MW Change**
Delaware	January 2022	\$9.25 - \$10.50	13.5%	Unscheduled - prior increase
Florida	October 2021	\$8.65 - \$10.00	15.6%	Unscheduled - prior increase
Hawaii (*Announcement*)	May 2022	\$10.10 - \$12.00	18.8%	Unscheduled - clean
Hawaii (*Implementation*)	October 2022	—	—	—
Maine	January 2022	\$12.15 - \$12.75	4.9%	Scheduled - inflation indexed
Montana	January 2022	\$8.75 - \$9.20	5.1%	Scheduled - inflation indexed
Nebraska (*Announcement*)	November 2022	\$9.00 - \$10.50	16.7%	Unscheduled - clean
Nebraska (*Implementation*)	January 2023	—	—	—
Ohio	January 2022	\$8.80 - \$9.30	5.7%	Scheduled - inflation indexed
Rhode Island	January 2022	\$11.50 - \$12.25	6.5%	Unscheduled - prior increase
South Dakota	January 2022	\$9.45 - \$9.95	5.3%	Scheduled - inflation indexed
Virginia	May 2021	\$7.25 - \$9.50	31.0%	Implemented

Notes: This table reports the timing and magnitude of the first large minimum wage increase observed between January 2021 and September 2023 for each additional treated state. Treatment timing is defined by the implementation date of the increase. Additional treated states are included only if the increase is preceded by a period with either no change or only very small increases (approximately 1–2 percent) and any larger prior increase occurred more than one year earlier. "Unscheduled – clean" denotes a discrete policy change not mechanically linked to indexation and not preceded by any minimum wage increase. "Unscheduled – prior increase" denotes a discrete policy change that follows small minimum wage increases in preceding years. "Scheduled – inflation indexed" denotes increases generated by state legislated inflation indexation. Virginia is classified as "Implemented" because the actual May 2021 increase occurs after the data begins, while the announcement date coincides with the beginning of the data.

Table 2: Outcome: Log(Job Seekers)

	Retail/Cleaning Occupations	Food-related Occupations
Log (Minimum Wage)	-.144	-.276
	(.075)	(.173)
Observations	1,646	1,646
Number of States	50	50

Notes: Cluster-robust standard errors in parentheses.

Table 3: Difference in Means of Log(Job Seekers) for Hawaii

	Pre-treatment	Post-treatment	Difference	Difference-in-Differences
Retail/Cleaning Occupations				
Treatment Group	10.461	10.671	0.209	**-0.092**
Control Group	11.626	11.928	0.302	
Food-related Occupations				
Treatment Group	10.115	10.445	0.330	**-0.130**
Control Group	10.781	11.241	0.461	

Table 4: Effects of Minimum Wage Increase on Log(Job Seekers) in Hawaii

	Retail/Cleaning Occupations	Food-related Occupations
Baseline		
ATT	-.110	-.131
Cluster-robust SE	(.013)	(.023)
Randomization Inference		
Rank	2	3
Mean Placebo ATT	.005	.007
Placebo SD	(.062)	(.104)
Asymptotic 90% CI	[-.097, .107]	[-.164, .178]
Empirical 90% CI	[-.076, .084]	[-.187, .119]
Observations	693	693
Number of States	21	21

Notes: Baseline estimates report the ATT from specification (2), which includes state and time fixed effects. Cluster-robust standard errors are shown in parentheses beneath the ATT. Randomization inference (RI) constructs an empirical randomization distribution by re-assigning the treatment timing to each of our 20 control states and re-estimating specification (2). The Mean Placebo ATT is the average of these placebo estimates and the Placebo SD is their standard deviation. Rank denotes the position of the actual ATT in the ordered randomization distribution (from most negative to most positive). The Asymptotic 90% CI is centered at the mean of the placebo estimates and is constructed using a normal approximation with critical value 1.64 and the standard deviation of of the randomization distribution. The Empirical 90% CI spans the central 90% of the empirical randomization distribution, from the second-lowest to the second-highest placebo ATT. With 20 placebo estimates, trimming one observation from each tail yields a 90% interval. Both RI confidence intervals reflect uncertainty under the randomization distribution rather than sampling variation.

Table 5: Difference in Means of Log(Job Seekers) for Nebraska

	Pre-treatment	Post-treatment	Difference	Difference-in-Differences
Retail/Cleaning Occupations				
Treatment Group	10.759	10.936	0.177	**-0.084**
Control Group	11.693	11.954	0.260	
Food-related Occupations				
Treatment Group	10.054	10.423	0.369	**-0.084**
Control Group	10.867	11.320	0.453	

Table 6: Effects of Minimum Wage Increase on Log(Job Seekers) in Nebraska

	Retail/Cleaning Occupations	Food-related Occupations
Baseline		
ATT	-.093	-.087
Cluster-robust SE	(.011)	(.018)
Randomization Inference		
Rank	2	4
Mean Placebo ATT	.005	.004
Placebo SD	(.054)	(.085)
Asymptotic 90% CI	[-.084, .094]	[-.135, .143]
Empirical 90% CI	[-.087, .093]	[-.156, .101]
Observations	693	693
Number of States	21	21

Notes: Baseline estimates report the ATT from specification (2), which includes state and time fixed effects. Cluster-robust standard errors are shown in parentheses beneath the ATT. Randomization inference (RI) constructs an empirical randomization distribution by re-assigning the treatment timing to each of our 20 control states and re-estimating specification (2). The Mean Placebo ATT is the average of these placebo estimates and the Placebo SD is their standard deviation. Rank denotes the position of the actual ATT in the ordered randomization distribution (from most negative to most positive). The Asymptotic 90% CI is centered at the mean of the placebo estimates and is constructed using a normal approximation with critical value 1.64 and the standard deviation of of the randomization distribution. The Empirical 90% CI spans the central 90% of the empirical randomization distribution, from the second-lowest to the second-highest placebo ATT. With 20 placebo estimates, trimming one observation from each tail yields a 90% interval. Both RI confidence intervals reflect uncertainty under the randomization distribution rather than sampling variation.

Table 7: Effects of Minimum Wage Increase on Log(Job Seekers) - Pooled Analysis

	Retail/Cleaning Occupations	Food-related Occupations
Panel A — TWFE		
ATT	-.102	-.108
Cluster-robust SE	(.013)	(.026)
Randomization Inference		
Percentile	1	5
Mean Placebo ATT	-.001	.005
Placebo SD	(.042)	(.064)
Asymptotic 90% CI	[-.070, .067]	[-.100, .110]
Empirical 90% CI	[-.073, .068]	[-.114, .094]
Panel B — Callaway and Sant'anna		
ATT	-.062	-.057
Cluster-robust SE	(.012)	(.020)
Randomization Inference		
Percentile	1	12
Mean Placebo ATT	.004	.006
Placebo SD	(.028)	(.049)
Asymptotic 90% CI	[-.042, .051]	[-.074, .085]
Empirical 90% CI	[-.044, .053]	[-.088, .072]
Panel C — Wooldridge		
ATT	-.102	-.108
Cluster-robust SE	(.013)	(.026)
Randomization Inference		
Percentile	1	5
Mean Placebo ATT	-.001	.005
Placebo SD	(.042)	(.064)
Asymptotic 90% CI	[-.070, .067]	[-.100, .110]
Empirical 90% CI	[-.073, .068]	[-.114, .094]
Observations	726	726
Number of States	22	22

Notes: Each panel reports ATT estimates from the indicated specification with state and time fixed effects; cluster-robust standard errors are shown in parentheses. Randomization inference (RI) uses a joint design that accounts for Hawaii and Nebraska adopting at different times. From the 20 control states we form all unordered pairs and, in each iteration, assign one state Hawaii's timing and the other Nebraska's timing and re-estimate the model. This yields 190 joint placebo ATT estimates that define the empirical randomization distribution. The Percentile reports the position of the observed ATT in this distribution (ordered from most negative to most positive). The Mean Placebo ATT is the average placebo estimate and the Placebo SD is their standard deviation. The Asymptotic 90% CI is centered at the placebo mean and uses a normal approximation with critical value 1.64, while the Empirical 90% CI spans the central 90% of the empirical randomization distribution. RI confidence intervals reflect randomization uncertainty rather than sampling variation.

Table 8: Results for Additional States (Retail/Cleaning Occupations)

State	**MW Change**	**ATT**	**RI Rank**	**Mean Placebo ATT**	**Asymptotic 90% CI**	**Empirical 90% CI**
Delaware	13.5%	-.048	6	.002	[-.100, .104]	[-.077, .078]
		(.014)		(.062)		
Florida	15.6%	.028	14	-.001	[-.101, .099]	[-.074, .079]
		(.013)		(.061)		
Hawaii	18.8%	-.041	4	.002	[-.088, .092]	[-.095, .095]
		(.012)		(.055)		
Maine	4.9%	-.178	1	.009	[-.093, .111]	[-.070, .085]
		(.014)		(.062)		
Montana	5.1%	-.001	1	.000	[-.102, .102]	[-.070, .085]
		(.014)		(.062)		
Nebraska	16.7%	-.077	2	.004	[-.075, .083]	[-.064, .082]
		(.011)		(.048)		
Ohio	5.7%	.023	11	-.001	[-.103, .101]	[-.080, .075]
		(.014)		(.062)		
Rhode Island	6.5%	-.075	2	.004	[-.098, .106]	[-.075, .098]
		(.014)		(.062)		
South Dakota	5.3%	.024	11	-.001	[-.103, .101]	[-.080, .093]
		(.014)		(.062)		
Virginia	31.0%	-.045	6	.002	[-.121, .125]	[-.133, .108]
		(.016)		(.075)		

Notes: ATT estimates are from specification (2), which includes state and time fixed effects; cluster-robust standard errors are reported in parentheses beneath the ATT. Randomization inference (RI) reassigns the treatment timing to each of the 20 control states and re-estimates the model to form the randomization distribution. The RI Rank reports the position of the observed ATT in this ordered distribution (from most negative to most positive). The Mean Placebo ATT is the average of the placebo estimates; the value in parentheses beneath it is the standard deviation of the randomization distribution. The Asymptotic 90% CI is centered at the placebo mean and uses a normal approximation with critical value 1.64, while the Empirical 90% CI spans the central 90% of the empirical randomization distribution. RI confidence intervals reflect randomization uncertainty rather than sampling variation.

Table 9: Results for Additional States (Food-Related Occupations)

State	MW Change	ATT	RI Rank	Mean Placebo ATT	Asymptotic 90% CI	Empirical 90% CI
Delaware	13.5%	-.107	4	.005	[-.149, .159]	[-.145, .122]
		(.020)		(.094)		
Florida	15.6%	.009	9	.000	[-.172, .172]	[-.171, .133]
		(.023)		(.105)		
Hawaii	18.8%	-.127	3	.006	[-.137, .149]	[-.167, .099]
		(.019)		(.087)		
Maine	4.9%	-.184	1	.009	[-.145, .163]	[-.141, .123]
		(.020)		(.094)		
Montana	5.1%	-.025	8	.001	[-.153, .155]	[-.149, .115]
		(.020)		(.094)		
Nebraska	16.7%	-.072	6	.004	[-.129, .137]	[-.127, .051]
		(.018)		(.081)		
Ohio	5.7%	.061	15	-.003	[-.157, .151]	[-.153, .111]
		(.020)		(.094)		
Rhode Island	6.5%	-.080	6	.004	[-.150, .158]	[-.146, .118]
		(.020)		(.094)		
South Dakota	5.3%	.031	11	-.002	[-.156, .152]	[-.152, .113]
		(.020)		(.094)		
Virginia	31.0%	.054	13	-.003	[-.211, .205]	[-.221, .134]
		(.028)		(.127)		

Notes: ATT estimates are from specification (2), which includes state and time fixed effects; cluster-robust standard errors are reported in parentheses beneath the ATT. Randomization inference (RI) reassigns the treatment timing to each of the 20 control states and re-estimates the model to form the randomization distribution. The RI Rank reports the position of the observed ATT in this ordered distribution (from most negative to most positive). The Mean Placebo ATT is the average of the placebo estimates; the value in parentheses beneath it is the standard deviation of the randomization distribution. The Asymptotic 90% CI is centered at the placebo mean and uses a normal approximation with critical value 1.64, while the Empirical 90% CI spans the central 90% of the empirical randomization distribution. RI confidence intervals reflect randomization uncertainty rather than sampling variation.

Table 10: Effects of Minimum Wage Increase on Log(Job Seekers) for Individual Occupations - Two-way Fixed Effects

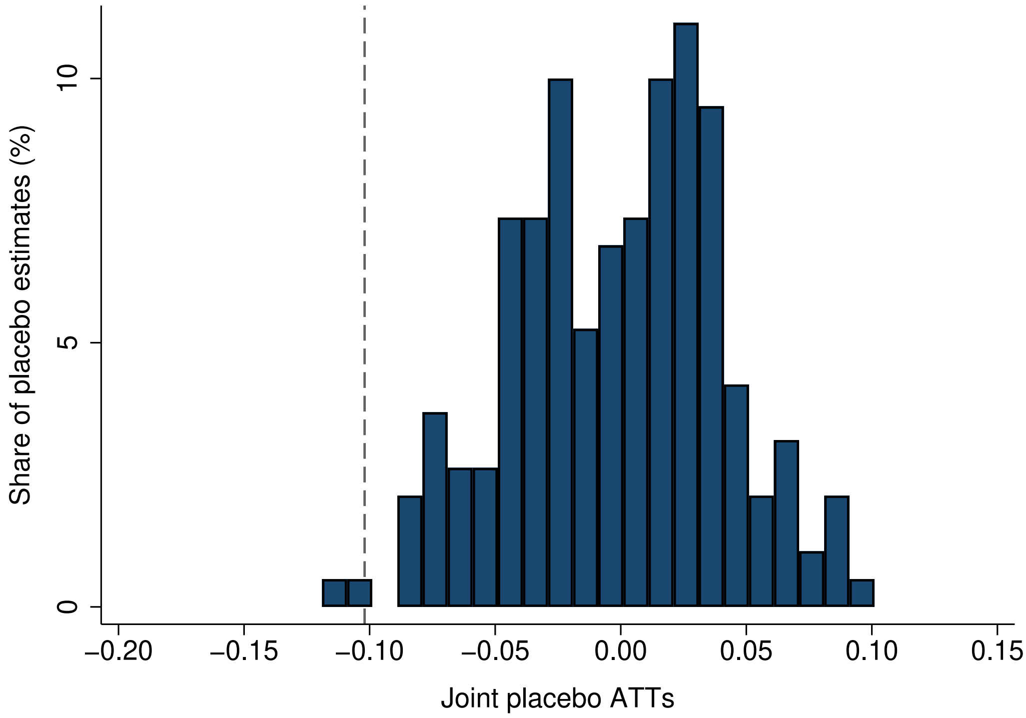
	Food-related Occupations				Retail/Cleaning Occupations				
	Crew Member	Cook	Dishwasher	Host	Cashier	Retail Salespersons	Janitor	Cleaner	Receptionist
ATT	-.20	-.04	.03	-.06	-.08	-.13	-.01	-.14	-.11
	(.06)	(.03)	(.06)	(.07)	(.04)	(.02)	(0.08)	(.06)	(.05)
Mean # of Seekers	32,543	13,864	23,842	34,455	41,572	82,128	21,482	19,494	37,281

Notes: Cluster-robust standard errors are shown in parentheses.

Appendix

Figure A.1: Joint Empirical Randomization Inference Distributions

(a) **Retail/Cleaning Occupations** – Vertical line denotes the estimated ATT (-0.102) from Panel A of Table 7.



(b) **Food-related Occupations** – Vertical line denotes the estimated ATT (-0.108) from Panel A of Table 7.

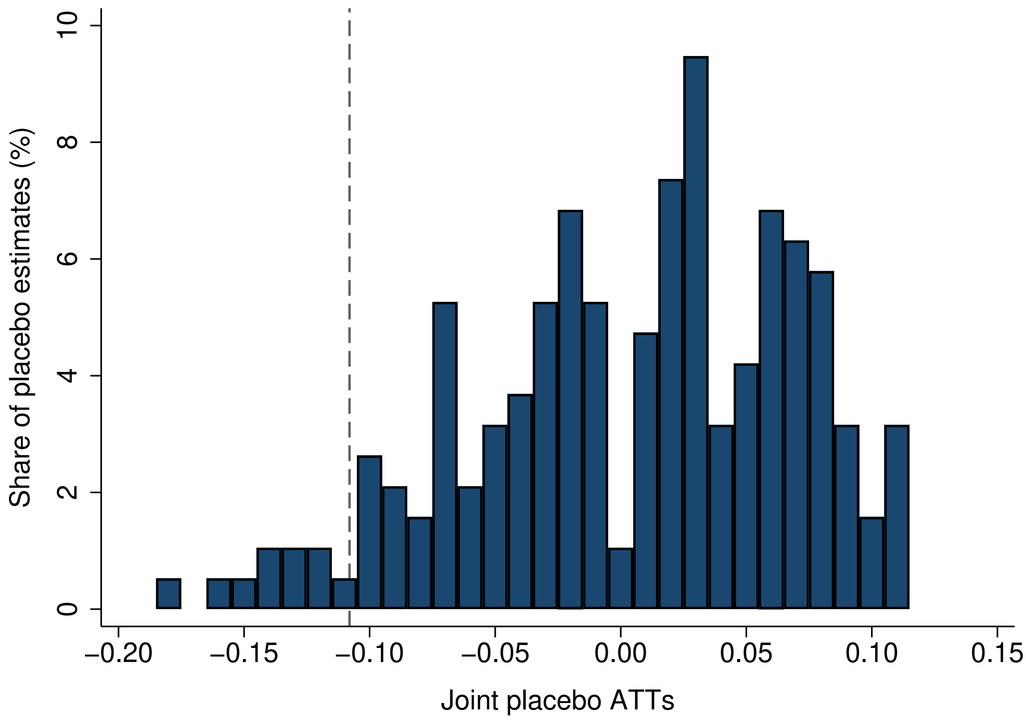


Table A.1: Descriptive Statistics

Variable	Mean	SD	Median
***Panel A.* Hawaii**			
Log (Job Seekers) - Retail/Cleaning Occupations	10.56	0.16	10.53
Log (Job Seekers) - Food-related Occupations	10.275	0.21	10.25
Median Age (years)	44.37	.99	44
Female Share (%)	49.99	.68	50
White Share (%)	24.62	1.92	24.88
Urban Share (%)	23.65	2.72	23.48
Mean Number of Children per Household	.35	.03	.35
N		33	
***Panel B.* Nebraska**			
Log (Job Seekers) - Retail/Cleaning Occupations	10.81	0.14	10.82
Log (Job Seekers) - Food-related Occupations	10.17	0.23	10.12
Median Age (years)	38.23	1.91	38
Female Share (%)	50.06	1.14	49.81
White Share (%)	89.67	1.58	89.71
Urban Share (%)	21.46	1.93	21.4
Mean Number of Children per Household	.31	.04	.31
N		33	
***Panel C.* Control States**			
Log (Job Seekers) - Retail/Cleaning Occupations	11.77	1.01	11.87
Log (Job Seekers) - Food-related Occupations	11	0.99	11
Median Age (years)	40.84	3.58	41
Female Share (%)	51.18	1.29	51.22
White Share (%)	82.18	10.74	85.46
Urban Share (%)	16.49	8.80	16.8
Mean Number of Children per Household	.30	.05	.29
N		660	

Notes: This table reports the mean, standard deviation, and median for our primary outcome variable, Log(Job Seekers), during the period of analysis, January 2021 through September 2023. Log(Job Seekers) refers to the logarithm of the number of unique monthly platform users actively searching for job postings in the relevant occupation groups. We also report state-level demographic characteristics drawn from the Current Population Survey (CPS), including median age, the share of females, the share of whites, the share of households residing in metropolitan areas, and the mean number of children per household. Treated states are broadly similar to the control states along these dimensions, with the notable exception of racial composition: while the control states are on average 82% white, the corresponding share in Hawaii is approximately 25%. We re-estimate specification (2) with these demographic controls for both Hawaii and Nebraska. In each case, the addition of demographic controls has a negligible impact on the parameter estimates.

Table A.2: Occupations with Lowest Mean Hourly Wages

Occupation Code Name	**Occupation title**	**Mean Hourly Wage**
772	Shampooers	14.07
687	Cooks, Fast Food	14.31
699	Fast Food and Counter Workers	14.48
753	Amusement and Recreation Attendants	14.54
751	Ushers, Lobby Attendants, and Ticket Takers	14.67
798	Cashiers	14.77
709	Hosts and Hostesses, Restaurant, Lounge, and Coffee Shop	14.78
752	Miscellaneous Entertainment Attendants and Related Workers	14.89
676	Lifeguards, Ski Patrol, and Other Recreational Protective Service Workers	15.07
708	Dishwashers	15.22
1219	Laundry and Dry-Cleaning Workers	15.33
704	Other Food Preparation and Serving Related Workers	15.37
746	Gambling and Sports Book Writers and Runners	15.4
783	Childcare Workers	15.42
743	Entertainment Attendants and Related Workers	15.5
1220	Pressers, Textile, Garment, and Related Materials	15.55
878	Hotel, Motel, and Resort Desk Clerks	15.66
1363	Parking Attendants	15.72
705	Dining Room and Cafeteria Attendants and Bartender Helpers	15.74
694	Food Preparation Workers	15.85
695	Food and Beverage Serving Workers	15.89
612	Home Health and Personal Care Aides	16.05
742	Animal Caretakers	16.12
702	Food Servers, Nonrestaurant	16.27
1228	Sewers, Hand	16.28
691	Cooks, Short Order	16.31
678	School Bus Monitors	16.33
685	Cooks and Food Preparation Workers	16.35
738	Animal Care and Service Workers	16.49
686	Cooks	16.52
1328	Ambulance Drivers and Attendants, Except Emergency Medical Technicians	16.55
680	Food Preparation and Serving Related Occupations	16.58
796	Retail Sales Workers	16.59
1366	Automotive and Watercraft Service Attendants	16.6
755	Locker Room, Coatroom, and Dressing Room Attendants	16.63
721	Maids and Housekeeping Cleaners	16.66

Table A.2: Bottom Occupations by Mean Hourly Wage (*Continued*)

Occupation Code Name	Occupation title	Mean Hourly Wage
756	Entertainment Attendants and Related Workers, All Other	16.67
624	Physical Therapist Aides	16.74
952	Graders and Sorters, Agricultural Products	16.77
1222	Sewing Machine Operators	16.83
1337	Taxi Drivers	16.88
611	Home Health and Personal Care Aides	
761	Funeral Attendants	16.92
1217	Textile, Apparel, and Furnishings Workers	16.93
1388	Cleaners of Vehicles and Equipment	16.95
711	Miscellaneous Food Preparation and Serving Related Workers	17.03
712	Food Preparation and Serving Related Workers, All Other	17.03
1391	Packers and Packagers, Hand	17.05
457	Floral Designers	17.07
1167	Bakers	17.09
747	Gambling Service Workers, All Other	17.14
719	Building Cleaning Workers	17.23
688	Cooks, Institution and Cafeteria	17.27
882	Library Assistants, Clerical	17.29
799	Gambling Change Persons and Booth Cashiers	17.32
690	Cooks, Restaurant	17.34
718	Building Cleaning and Pest Control Workers	17.36
776	Baggage Porters and Bellhops	17.36
1365	Transportation Service Attendants	17.37
956	Farmworkers and Laborers, Crop, Nursery, and Greenhouse	17.37
720	Janitors and Cleaners, Except Maids and Housekeeping Cleaners	17.43
786	Recreation Workers	17.44
771	Manicurists and Pedicurists	17.54
701	Waiters and Waitresses	17.56
1226	Shoe Machine Operators and Tenders	17.58
892	Receptionists and Information Clerks	17.59
830	Telemarketers	17.64
804	Retail Salespersons	17.64
853	Gambling Cage Workers	17.67
1372	Passenger Attendants	17.68
1170	Meat, Poultry, and Fish Cutters and Trimmers	17.71
1224	Shoe and Leather Workers	17.72
961	Forest and Conservation Workers	17.72
1336	Shuttle Drivers and Chauffeurs	17.75

Table A.2: Bottom Occupations by Mean Hourly Wage (*Continued*)

Occupation Code Name	**Occupation title**	**Mean Hourly Wage**
954	Miscellaneous Agricultural Workers	17.75
1225	Shoe and Leather Workers and Repairers	17.80
1232	Textile Cutting Machine Setters, Operators, and Tenders	17.81
957	Farmworkers, Farm, Ranch, and Aquacultural Animals	17.82
696	Bartenders	17.83
1234	Textile Winding, Twisting, and Drawing Out Machine Setters, Operators, and Tenders	17.84
790	Personal Care and Service Workers, All Other	17.88
789	Miscellaneous Personal Care and Service Workers	17.88
1109	Tire Repairers and Changers	17.92
634	Veterinary Assistants and Laboratory Animal Caretakers	17.94
1230	Textile Machine Setters, Operators, and Tenders	17.96
947	Agricultural Workers	17.97

Notes: This table shows all occupations with mean hourly wage below $18 in the United States.

Table A.3: Comparison of BLS Categories and Occupations Used in this Study

BLS occupational category	**Mean hourly wage**	**Our searched job titles**
Cooks and Food Preparation Workers	$16.35	cook, crew member
Food and Beverage Serving Workers	$15.89	crew member
Other Food Preparation and Serving Related Workers	$15.37	host, dishwasher
Retail Sales Workers	$16.59	retail salespersons
Cashiers	$14.78	cashiers
Janitors and Cleaners, Except Maids and Housekeeping Cleaners	$17.43	janitors, cleaners
Receptionists and Information Clerks	$17.59	receptionist

Notes: This table shows the mean hourly wage for each of the occupations associated with the job titles chosen for our study.

Table A.4: Minimum Wage for U.S. States

State Name	MW 2018	MW 2019	MW 2020	MW 2021	MW 2022	MW 2023	Group
Alabama	7.25	7.25	7.25	7.25	7.25	7.25	Control
Alaska	9.84	9.89	10.19	10.34	10.34	10.85	Excluded
Arizona	10.50	11	12	12.15	12.80	13.85	Excluded
Arkansas	8.50	9.25	10	11	11	11	Excluded
California	11	12	13	14	15	15.50	Excluded
Colorado	10.20	11.10	12	12.32	12.56	13.65	Excluded
Connecticut	10.10	10.10	11	12	13	14	Excluded
Delaware	8.25	8.75	9.25	9.25	10.50	11.75	Additional
Florida	8.25	8.46	8.56	8.65	10	11	Additional
Georgia	7.25	7.25	7.25	7.25	7.25	7.25	Control
Hawaii	10.10	10.10	10.10	10.10	10.10	12	Treatment
Idaho	7.25	7.25	7.25	7.25	7.25	7.25	Control
Illinois	8.25	8.25	9.25	11	12	13	Excluded
Indiana	7.25	7.25	7.25	7.25	7.25	7.25	Control
Iowa	7.25	7.25	7.25	7.25	7.25	7.25	Control
Kansas	7.25	7.25	7.25	7.25	7.25	7.25	Control
Kentucky	7.25	7.25	7.25	7.25	7.25	7.25	Control
Louisiana	7.25	7.25	7.25	7.25	7.25	7.25	Control
Maine	10	11	12	12.15	12.75	13.80	Additional
Maryland	9.25	10.10	11	11.75	12.50	13.25	Excluded
Massachusetts	11	12	12.75	13.50	14.25	15	Excluded
Michigan	9.25	9.45	9.65	9.65	9.87	10.10	Excluded
Minnesota	9.65	9.86	10	10.08	10.33	10.59	Excluded
Mississippi	7.25	7.25	7.25	7.25	7.25	7.25	Control
Missouri	7.85	8.60	9.45	10.30	11.15	12	Excluded
Montana	8.30	8.50	8.65	8.75	9.20	9.95	Additional
Nebraska	9	9	9	9	9	10.50	Treatment
Nevada	8.25	8.25	8.25	9	9.75	10.50	Excluded
New Hampshire	7.25	7.25	7.25	7.25	7.25	7.25	Control
New Jersey	8.60	8.85	11	12	13	14.13	Excluded
New Mexico	7.50	7.50	9	10.50	11.50	12	Excluded
New York	10.40	11.10	11.80	12.50	13.20	14.20	Excluded
North Carolina	7.25	7.25	7.25	7.25	7.25	7.25	Control
North Dakota	7.25	7.25	7.25	7.25	7.25	7.25	Control
Ohio	8.30	8.55	8.70	8.80	9.30	10.10	Additional
Oklahoma	7.25	7.25	7.25	7.25	7.25	7.25	Control
Oregon	10.25	10.75	11.25	12	12.75	13.50	Excluded
Pennsylvania	7.25	7.25	7.25	7.25	7.25	7.25	Control
Rhode Island	10.10	10.50	10.50	11.50	12.25	13	Additional
South Carolina	7.25	7.25	7.25	7.25	7.25	7.25	Control
South Dakota	8.85	9.10	9.30	9.45	9.95	10.80	Additional
Tennessee	7.25	7.25	7.25	7.25	7.25	7.25	Control
Texas	7.25	7.25	7.25	7.25	7.25	7.25	Control
Utah	7.25	7.25	7.25	7.25	7.25	7.25	Control
Vermont	10.50	10.78	10.96	11.75	12.55	13.18	Excluded

Table A.4: Minimum Wage for USA states (*Continued*)

State Name	**MW 2018**	**MW 2019**	**MW 2020**	**MW 2021**	**MW 2022**	**MW 2023**	**Group**
Virginia	7.25	7.25	7.25	7.25	11	12	Additional
Washington	11.50	12	13.50	13.96	14.49	15.74	Excluded
West Virginia*	8.75	8.75	8.75	8.75	8.75	8.75	Excluded
Wisconsin	7.25	7.25	7.25	7.25	7.25	7.25	Control
Wyoming	7.25	7.25	7.25	7.25	7.25	7.25	Control

Notes: Minimum wages listed above are as of January 1st for the given year. All states with stable minimum wage requirements from 2018 to 2023 are included in the control group with the exception of West Virginia, which changed its minimum wage soon before 2018. All results shown in this paper are qualitatively identical when we include West Virginia in the control group. Nebraska and Hawaii had unscheduled changes in their minimum wages in 2022 or 2023 and thus are included in the treatment group. Delaware, Florida, Maine, Montana, Ohio, Rhode Island, and South Dakota are included in the "additional" treatment group because each has a discrete, relatively large minimum wage increase in the sample period that is preceded by no change or only very small increases (approximately 1–2 percent), with any larger prior increase occurring more than one year earlier. Virginia's minimum wage increase, by contrast, was unscheduled and announced immediately prior to the start of the analysis period. Although the lack of pre-announcement data prevents Virginia from being included in our primary analysis, the implementation of Virginia's minimum wage increase occurs within the sample window and is observed with pre-implementation data. For the additional treated states, treatment timing is defined by the implementation date of the minimum wage increase. We therefore also include Virginia in the additional treatment group. All other states are excluded from the main analysis.