# Essays in Labor and Public Economics

by

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I, Jon Piqueras San Cristobal, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

# Abstract

My thesis combines data and theory to study the functioning of the low-wage labor market and the design of public policies aimed at reducing inequality.

The first chapter studies whether unemployment insurance should vary with labor market conditions, taking into account differences in economic opportunity. In this context, the source of inequality matters, being crucial whether unemployment arises from differences in individual effort or disparities in work opportunities. Utilizing a broad range of data sources and empirical strategies, the study reveals that benefit duration should increase in difficult times, given that the efficiency costs decline while the social benefits increase considerably. An important novel mechanism behind this increase is that the pool of unemployed shifts towards high-effort individuals in bad times, and that society exhibits strong preferences for redistribution towards this type of unemployed individuals.

The second chapter explores whether minimum wage policies induce search effort responses by the unemployed. Using machine learning methods, and combining different sources of publicly available data, it finds that the unemployed increase search effort in response to a higher minimum wage, but they do not find jobs faster. Interpreting the estimates through the lens of a standard search model reveals that equilibrium responses are key for understanding employment effects, and that the policy increases welfare for affected individuals.

The third chapter, co-authored with Emiliano Huet-Vaughn, examines the labor market consequences of a unique natural experiment, consisting of a large increase and an equivalent subsequent decrease to a binding minimum wage. Employing a synthetic control strategy, the paper documents asymmetric responses, where wages in a leading low-wage industry increase as the minimum wage rises, but do not fall when it is lowered. This boost for low-wage workers' earnings is apparently long lasting after the policy is revoked, providing novel evidence of hysteresis in wage setting.

# Impact Statement

Understanding the sources of inequality, as well as the functioning of the labor market, is key for the design of economic policies. Thus, the work contained in this thesis has potential impact both for economic research and for public policy design.

In relation to the impact within academia, this thesis provides several contributions. First, it incorporates preferences for redistribution, conceptually and empirically, into the unemployment insurance framework considering changes in the composition of the unemployed according to labor market conditions. To do so, it implements a novel survey aimed at understanding whether the value of the benefits varies with labor market conditions. This approach could be adapted to understand the value of other economic policies in different contexts, and to explore potential policy differentiation along various dimensions. Second, the thesis proposes a methodology to combine publicly available data in order to understand the equilibrium effects of minimum wages. In particular, the empirical measures developed here such as job search effort could also be used to investigate alternative margins of response to other policies like unemployment insurance. Third, it documents novel facts of wage hysteresis that can be used to inform richer models of nominal rigidity and wage setting.

The contributions of this thesis can also have beneficial use outside of academia. The research contained here is motivated by recent decades' growing inequality and stagnant wages at the bottom of the distribution, alongside large increases in the number of unemployed individuals induced by recent aggregate shocks such as Covid and the Great Recession in many countries. As a result, a renewed interest in policies aimed at reducing inequality has been often highlighted in policy debates. My work, which focuses on two prominent economic policies, unemployment insurance and the minimum wage, can inform the public debate in several ways.
The empirical application of my unemployment insurance framework in the context of Spain indicates that the generosity of unemployment benefits should increase during economic downturns, highlighting the important role of the increasing social value of these transfers. This recommendation is contrary to the current practice in this and most other European countries, and in line with others like the US. Moreover, the evidence on the equilibrium impacts of the minimum wage implies that the public debate should consider supply side responses induced by this type of policies. These mechanisms, which are often well understood for other policies like unemployment insurance, are often missing in the minimum wage debate and so they should be taken into account given their empirical relevance. Finally, the evidence from the minimum wage repeal natural experiment high-

lights that temporary labor market policy can have long-lasting effects on wages. This underscores the ability of policymakers to achieve permanent gains for workers, and the need for considering history dependence in policy setting when designing labor market policies.

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## Chapter 1

# Unemployment Insurance, Inequality of Opportunity, and Labor Market Conditions

## 1.1 Introduction

Unemployment insurance (UI) is a crucial tool for policymakers seeking to support individuals when hit by adverse employment shocks. Its importance is even higher during economic downturns, when large numbers of individuals unexpectedly lose their jobs and rely on this income support to alleviate the hardship of unemployment. Despite this being a widespread issue, there is significant variation across countries in how the generosity of the program varies with economic conditions. While some, like the United States, usually provide more generous transfers during recessions, others, like many countries in Western Europe, keep transfers constant regardless of economic conditions.

Economists have long debated about whether the unemployed should receive more generous transfers when economic conditions are bad. The debate centers around how the efficiency costs and social benefits associated with UI (Baily, 1978; Chetty, 2006) evolve with economic conditions. Existing evidence on social benefits over the cycle, as captured by the consumption insurance value, is scant and remains inconclusive, since empirical studies usually exploit relatively small variations in the unemployment rate which may limit the power to detect fluctuations of these components. More fundamentally, the canonical framework fails to capture that the pool of unemployed workers may be different in good and bad times: while a labor supply narrative may be better suited for the unemployed in good times, involuntary unemployment due to lacking work opportunities becomes increasingly significant as labor market conditions deteriorate.[1] Such difference possibly affects the equity implications of unemployment benefit reforms over the cycle.

This paper proposes, and empirically evaluates, a framework to assess the welfare consequences of changing UI generosity with economic conditions. The framework characterizes optimal unemployment benefit duration depending on three components: the efficiency cost, the value of insurance, and the preferences for redistribution between different types of unemployed. It extends previous work by embedding inequality of opportunity (Roemer, 1998) within the UI framework, accounting for differential selection of individuals into unemployment due to variation in labor market conditions. In the model, the social planner is not solely concerned about consumption inequality due to unemployment; the origin of this inequality also matters, becoming crucial to disentangle whether unemployment emerges from unequal economic opportunities (e.g., job offers) or disparities in indi-

[1] Intuitively, the role of these two components changes substantially over the cycle, as conveyed by Summers (2010): *In an economy that is as demand constrained as ours, whatever small changes in search intensity may be associated with unemployment insurance are not the reason for the persistence of joblessness. With five unemployed Americans seeking work for every job opening available, there can be little doubt that the overwhelming cause of unemployment is not a lack of will among the jobless to find work, but a lack of work opportunities.*

vidual effort (e.g., job search).

The framework delivers a set of high-level "sufficient statistics" that can be estimated empirically, providing a direct link between theory and evidence. I implement this framework in the context of Spain, which is particularly well-suited for this analysis since it features unusually large variation in the unemployment rate. Over the period of study, 2005-2017, the national unemployment rate increased from 7.9% to 26.9%, making this context remarkable relative to international standards (see Figure 1.A1). The empirical analysis combines administrative data on labor supply, survey data on consumption, time use survey data on job search, and a representative online survey I conducted on preferences for redistribution. Along with policy variation and the aforementioned variation in economic conditions, it enables a comprehensive assessment of all the inputs needed to evaluate the optimal variation in UI duration across good and bad times.

The theoretical framework follows Baily (1978), Lentz and Tranaes (2005), Chetty (2008), Schmieder et al. (2012), and Kolsrud et al. (2018), where a population of heterogeneous unemployed individuals look for jobs by making search effort and savings decisions. Individuals are also subject to different economic conditions (good or bad), where search effort is more or less effective in terms of job finding (unequal economic opportunity). The government provides unemployed individuals with unemployment benefits that are paid over a limited number of periods, and this duration can be made dependent on economic conditions.

I extend the standard framework by allowing the unemployed to differ in their willingness to work: the extent to which they search for jobs conditional on the economic environment. The government maximizes the welfare-weighted utility of employed and unemployed individuals. In the spirit of the generalized social marginal welfare weights of Saez and Stantcheva (2016), the welfare weights can depend on the unemployed's willingness to work type. This is motivated by work by Alesina and Angeletos (2005) and Alesina et al. (2018), where the social planner exhibits social preferences that depend on whether inequality emerges because of bad luck (not finding a job when labor demand is dampened) or lack of effort (not finding a job when plenty of opportunities are available).

The optimal cycle-dependent benefit duration follows a Baily-Chetty intuition in this economy: social benefits and costs of unemployment insurance in good times must balance the benefits and costs in bad times. These benefits and costs of UI benefit extensions are fully captured by three sufficient statistics that can be estimated in the data. First, benefit duration depends on the insurance value coming from consumption smoothing. The transfers are more valuable when individuals are less able to self-insure themselves. In this sense, the larger the gap in marginal utilities of consumption between the unemployed in recessions and the unemployed in booms, the larger the gap in UI duration between these two economic conditions should be.

Second, the framework introduces a novel channel determining the social benefits provided by UI transfers over the cycle. In the presence of preferences for redistribution across the unemployed based on their willingness to work, the social planner takes into account that the composition of the unemployed differs in booms and recessions. Although individual willingness to work is not directly observed, aggregate economic conditions seen through the lens of this framework reveal information about the type of the average unemployed. In good times, when job opportunities are available, individuals with a high willingness to work often find a job, and so the pool of unemployed is on average composed of low-willingness to work individuals. However, in bad times, even individuals with high willingness to work are likely to remain unemployed, and thus the pool of unemployed is composed of both low but also high-willingness to work types. This implies that, all else constant, the willingness to work of the average unemployed increases with the unemployment rate.[2] Therefore, a social planner who prefers to insure the consumption drops of individuals whose unemployment status is not caused by limited job search effort can use aggregate conditions to target resources towards them. As a result, the larger the gap in society's relative valuations between the unemployed in recession and the unemployed in boom, the higher the potential UI duration should be in recession relative to boom.

Third, the derived formula highlights that optimal duration also depends on the evolution of the efficiency costs of the policy over the cycle. Higher potential benefit duration reduces incentives to look for jobs, which may result in distortions of labor supply with individuals spending more time unemployed and thus taking longer to find a job. This imposes an additional cost on the government budget, since the longer unemployment duration translates into more resources spent on unemployment benefits and less resources collected through taxes when employed. In this context, the larger the gap between the labor supply distortion caused by extending UI in good times relative to bad times, the larger the gap in generosity should be.

The framework described above guides the empirical analysis, which is structured in three parts.

[2] In other words, aggregate unemployment rate can be seen as a shifter of the signal-to-noise ratio in the spirit of Alesina and Angeletos (2005), where the signal here corresponds to the voluntary part of unemployment and the noise to the involuntary one. The ratio in this context is decreasing with the unemployment rate, so that the moment of the cycle provides additional information to the government about the role of lack of effort vs bad luck in individuals' employment status.

The first part of the analysis explores how the consumption insurance value of the transfers varies with economic conditions. In doing so, I employ a difference-in-differences design similar to Gruber (1997) where I compare the consumption patterns of individuals becoming unemployed with those of individuals who remain working. I exploit rich survey panel data on household consumption, which provide detailed information on different consumption categories. This allows me to study the effect on both food consumption – that has traditionally been the focus of the literature due to data limitations – and on overall consumption including housing, utilities, clothing and transportation among others. I find that, on average, households experience an overall 9.6% (s.e. 1.3%) reduction in consumption upon job loss. Moreover, this drop increases in bad times, with a 1 p.p. increase in unemployment rate resulting in an additional 0.4% (s.e. 0.1) reduction relative to pre-unemployment consumption levels. Similarly to Kroft and Notowidigdo (2016), I find no significant variation in the drop in food consumption over the business cycle. The main categories that drive the consumption drop fluctuations are housing and utilities, and clothing. This implies that extending UI duration in recessions provides larger consumption insurance benefits relative to extensions in boom periods. Moreover, it underscores the importance of leveraging detailed consumption data and helps reconcile previous work documenting a constant drop in consumption over the cycle.

The second part quantifies how the benefits of the transfers due to both preferences for redistribution and changes in the unemployed's composition evolve with economic conditions. I proxy willingness to work by the search effort level that the unemployed exert in boom periods. Individuals who search on a weekly basis are considered to have a high willingness to work, while those who do not are characterized by low willingness. I assess whether society has a preference for redistribution between these types of unemployed individuals. To do so, I explicitly design a survey and administer it to a sample of around 1000 individuals who are representative of the Spanish active population. The survey is designed following recommended practices in the literature (Stantcheva, 2022), and has features such as order randomization and open-ended questions to make sure that results are not driven by order effects or specific characteristics of the implementation. In a first step towards eliciting preferences, respondents are asked to express preference towards resource allocation between two types of unemployed: one who searched for jobs last week and one who did not. Remarkably, around 75% conveys a desire to reward job search behavior. The remaining share of respondents do not consider effort to be a dimension that should drive unequal allocations. In a second step, with the aim of quantifying relative valuations between types, respondents are presented with multiple pairwise choices that differ in the amounts received by each type of unemployed. The design varies experimentally the cost of the transfer between individuals, allowing me to elicit the distribution of relative valuations by society between the two types. This approach yields estimates that imply strong preferences for redistribution, with society valuing a €1 transfer to the high-willingness to work type as much as a €13 transfer to the low-willingness to work type.

After documenting strong preferences for redistribution along the willingness to work dimension, I study changes in the composition of the unemployed over the cycle. By exploiting the Spanish Time Use data, I show that the share of unemployed who search for jobs weekly increases strongly with the unemployment rate. Nevertheless, this increase could reflect both a change in composition of the unemployed and the behavioral change. I follow Mukoyama et al. (2018) to gauge the part driven by composition, and find that there is a large change over the business cycle driven just by the shift in composition: the share of weekly searchers (high willingness to work) goes from 49% in boom to 68% in recession. I provide a sensitivity analysis including a prediction exercise just using the Spanish data, where I first estimate the relationship between effort and a set of demographics, and then study how predicted effort varies given the observed change in demographics of the unemployed over the cycle. The results confirm the main finding of a considerable shift in the pool of unemployed towards high-willingness to work types in bad times. Combining the change in the composition of types over the cycle with the relative valuations between types, I estimate a ratio of welfare weights of 1.31. That is, society values a €1 transfer to the type of individuals unemployed in recession as much as €1.31 to those unemployed during a boom.

The third part employs administrative data on unemployment spells to analyze the evolution of the efficiency cost over the cycle. This cost depends on two components. First, a mechanical one, captured by the share of people who remain unemployed after benefit exhaustion, which I estimate nonparametrically. Second, a behavioral one, captured by the response of unemployment duration to changes in potential benefit duration, which I estimate through a Regression Discontinuity (RD) design. To estimate the latter, I leverage variation in the structure of the Spanish UI system, where the statutory duration of the benefits varies discretely at several work experience thresholds. I find that, on average, for every €1 transferred mechanically to the unemployed, the government needs to levy €1.77 on top of that because of the behavioral response. Consistent with the results of Schmieder et al. (2012), I also find that the efficiency cost declines in recessions, and that this is driven by a higher share of individuals exhausting benefits in recessions as opposed to a larger marginal effect of potential benefit duration on unemployment duration.[3]

Finally, the empirical findings are used to inform the statistics of interest in the derived framework, with the aim of assessing the welfare effects of a hypothetical reform making UI benefit duration dependent on the business cycle. The results indicate that the potential duration of the benefits should increase with the unemployment rate. Specifically, I estimate that a local budget-balanced reform of the UI system that increases the duration of benefits for the unemployed in recession, and reduces the duration for the unemployed in boom, would yield a net welfare gain of €0.95 per euro transferred from individuals unemployed in boom to individuals unemployed in recession. Two thirds of this welfare gain come from an increase of the social benefits provided by UI transfers, while one third relates to the lower efficiency cost of increasing generosity in bad times.

Importantly, these results highlight the quantitative relevance of the change in social benefits over the cycle that has been overlooked in related empirical literature, which mostly focuses on the change in efficiency costs. In contrast, the total welfare gains of extending UI in recessions estimated in this paper are three times as large relative to the gains considering only the efficiency cost. This implies that UI benefit duration should increase more with the unemployment rate than predicted by previous work.[4] Note that this finding is not driven by the specific welfare criterion studied here. Even abstracting from welfare weights and considering only the changes in consumption insurance value over the cycle, which would correspond to the standard utilitarian framework, I estimate that the welfare gains are approximately twice the gains that include only the efficiency cost.

My work relates to several distinct literatures. It relates to work on normative analysis of UI policies that aims to connect theory and data, building on seminal work by Baily (1978), Gruber (1997) and Chetty (2006). While this work focuses on studying the main trade-off of UI — providing insurance while maintaining incentives — in a stylized framework, subsequent studies have considered different features of the generosity of unemployment benefits (Card et al., 2007; Shimer and Werning, 2008; Kolsrud et al., 2018; Lindner and Reizer, 2019; Campos et al., 2022; Ferey, 2022). In terms of changes

[3] This result should be interpreted as a micro effect, since the RD design identifies labor supply responses for individuals with different benefits duration keeping labor market tightness constant. As pointed out by Landais et al. (2018b;a), when labor market tightness is not efficient, one also needs to include a correction term to account for the fact that tightness may also respond to UI increases. Given the evidence in the literature, where it is generally found that the correction term is countercyclical, my finding can be potentially interpreted as a lower bound, being possible that the overall cost of providing UI varies even more with economic conditions than estimated here.

[4] For example, based on the survival rates presented in Kroft and Notowidigdo (2016), back-of-the-envelope calculations assuming a constant marginal response over the cycle suggest that, under the standard framework and considering only the efficiency cost, it is very difficult to rationalize extending benefits from 26 to 99 weeks as implemented in the US. This is a motivation for further understanding of the changes of social benefits, focusing both on the standard insurance framework and incorporating empirically-relevant social preferences.

of generosity over the business cycle, one strand applies the Baily-Chetty insights (Schmieder et al., 2012; Kroft and Notowidigdo, 2016; Landais et al., 2018b;a), while another one relies on model-based calibrations (Mitman and Rabinovich, 2015; McKay and Reis, 2021; Kekre, 2023). My paper contributes by embedding fairness considerations due to inequality of work opportunities into the design of UI policies over the cycle. It draws on insights from the generalized social marginal welfare weights proposed by Saez and Stantcheva (2016), and the equality of opportunity literature (Roemer, 1998; Roemer et al., 2003; Alesina and Angeletos, 2005; Alesina and La Ferrara, 2005; Alesina et al., 2018), accounting for the social preference for reducing the degree of inequality induced by luck, while rewarding individual effort. It is also closely related to the work on "tagging" (Akerlof, 1978), where here economic conditions reveal additional information and are used as an imperfect tag for willingness to work.

The first part of the empirical analysis relates to work on the social benefits of UI transfers.[5] Specifically, the literature has focused on its consumption insurance value, starting with the seminal work by Gruber (1997) and followed by Browning and Crossley (2001); Stephens Jr (2001); Hendren (2017); Kolsrud et al. (2018); Campos and Reggio (2019); Ganong and Noel (2019); Landais and Spinnewijn (2021); Gerard and Naritomi (2021).[6] Evidence on how the value of UI changes with economic conditions is surprisingly scant. A few recent studies document consumption patterns over the period of the Covid pandemic (Chetty et al., 2020; Ganong et al., 2022), while, to the best of my knowledge, only one paper considers changes over the business cycle (Kroft and Notowidigdo, 2016). This study finds that the insurance value does not vary with economic conditions. Unfortunately, given the institutional setting and the available data, their results only consider food consumption, and the variation in unemployment rate effectively exploited is relatively small, raising concerns about potential lack of power to detect fluctuations.[7] I revisit this aspect by providing evidence from a setting where rich consumption data beyond food consumption is available, together with large variation in unemployment rate. I find that the insurance value changes over the cycle, and that the result is driven by non-food consumption

[5]The evidence on the value of transfers has historically lagged behind the evidence on disincentive effects. This historical emphasis in the literature on the costs of government programs, as opposed to their benefits, is not only present in Unemployment Insurance, but extends to other programs including Medicaid, AFDC/TANF and SNAP in the US as documented by Aizer et al. (2022).

[6]More generally, it also relates to studies on consumption responses to income shocks (e.g., Blundell et al., 2008; Low and Pistaferri, 2015; Autor et al., 2019; Gross et al., 2020; Ganong et al., 2020).

[7]The smaller variation in unemployment rate relative to my setting arises from two factors. First, overall fluctuations in the US Great Recession are an order of magnitude smaller than in the case of Spain. Second, benefit generosity in the US tends to increase when the unemployment rate increases. Given that the interest is on understanding consumption insurance over the cycle, while keeping generosity *constant*, the relation between benefits and unemployment needs to be controlled for and so the effective variation used is smaller. The potential duration of benefits in Spain is constant over the cycle, which allows to exploit relatively more variation.

categories, thus being able to reconcile previous findings in the literature.

The implementation of the survey in combination with alternative data sources with the aim of informing policy-relevant parameters relates to a growing body of research spanning different fields including public (Saez and Stantcheva, 2016; Stantcheva, 2021), labor (Jäger et al., 2022), urban (Gaubert et al., 2020), macro (Mui and Schoefer, 2023), and political economy (Kuziemko et al., 2015; Alesina et al., 2023). I contribute by designing and administering a new survey which is explicitly constructed to estimate preferences for redistribution between types of unemployed individuals that differ in their willingness to work. In this sense, I consider not only the insurance component but also the redistribution value of cycle-dependent UI transfers along a specific effort dimension.[8]

Finally, this paper also relates to the literature analyzing labor supply disincentive effects induced by increases in unemployment benefit generosity. While most studies quantify labor supply distortions at a given moment of the cycle (e.g., Katz and Meyer, 1990; Krueger and Meyer, 2002; Card and Levine, 2000; Rothstein, 2011; Landais, 2015; Lalive et al., 2015; Hagedorn et al., 2019; Chodorow-Reich et al., 2019; Boone et al., 2021; Dieterle et al., 2020; Gerard and Gonzaga, 2021; Domenech-Arumi and Vannutelli, 2023; Acosta et al., 2023), few studies analyze the evolution of the disincentive effects over the cycle (Farber et al., 2015; Schmieder et al., 2012; Kroft and Notowidigdo, 2016). My paper provides new insights on how the labor supply responses induced by potential benefit duration extensions evolve at different points of the cycle, in a setting with remarkably large variation in economic conditions.

The rest of the paper proceeds as follows. Section 1.2 provides the theoretical framework used to evaluate the marginal welfare gain from benefit duration extensions. Section 1.3 describes the institutional context and the data. Section 1.4 presents the empirical methodology and the results. Section 1.5 describes the welfare implications, and Section 1.6 concludes.

## 1.2 Framework

### 1.2.1 Setup

Here I present a stylized version of the main framework, which will guide the empirical analysis. It builds on Baily (1978), Lentz and Tranaes (2005), Chetty (2008), Schmieder et al. (2012), and Kolsrud et al. (2018), and characterizes the job search problem of the unemployed and the policy choice of the social planner in good and bad times. I study changes in benefit duration over the cycle, while keeping

[8]More broadly, it also relates conceptually to the measurement of the general equity-efficiency trade-off faced by societies when implementing policies, as pointed out by Okun (1975) in his seminal "leaky bucket" thought experiment.

taxes fixed. This allows me to focus on the unemployed, considering transfers between individuals unemployed at different moments of the business cycle, and accounting for composition shifts in the pool of unemployed. A more detailed derivation is provided in Appendix 1.B.1.

**Environment.** There is a continuum of agents with unit mass who become unemployed at $t = 0$ and can be employed or unemployed until last period $T$ when they retire. They make saving decisions $a_{i,t}$ and, while unemployed, they also choose search effort $e_{i,t}$ facing a fixed wage that is high enough to ensure that all job offers will be accepted. Effort increases the probability of finding a job next period, $h^k(e_{i,t})$, in a way that potentially differs depending on the moment of the business cycle $k$.[9] Search is costly as given by $\psi_i(e_{i,t})$, which is assumed to be differentiable, increasing and convex. There are two aggregate economic conditions $k \in \{B, R\}$, Boom and Recession, with the only difference being a different mapping between search effort and job finding such that $h^B(e_{i,t}) > h^R(e_{i,t})$ (i.e. for a given effort level, job finding is higher in Boom than in Recession).[10] Formally, the problem of an unemployed individual in state $k$ at time $t$ is:

$$V^u_{i,t}(a_{i,t}, k_t) = \max_{e_{i,t}, a_{i,t+1}} u(c^u_{i,t}) - \psi_i(e_{i,t}) + \beta E_t[(1 - h^k(e_{i,t}))V^u_{i,t+1}(a_{i,t+1}, k_{t+1}) + h^k(e_{i,t})V^e_{i,t+1}(a_{i,t+1}, k_{t+1})] \quad (1.1)$$

where $a_{i,t+1} = (1 + r)a_{i,t} + b^k_t + y_u - c^u_{i,t}$ and $a_{i,t+1} \geq \bar{a}_i$. When re-employed, the agent solves the following problem:

$$V^e_{i,t}(a_{i,t}, k_t) = \max_{a_{i,t+1}} v(c^e_{i,t}) + \beta E_t V^e_{i,t+1}(a_{i,t+1}, k_{t+1}) \quad (1.2)$$

where $a_{i,t+1} = (1+r)a_{i,t} + w_i - \tau - c^e_{i,t}$ and $a_{i,t+1} \geq \bar{a}_i$. Flow utilities for the unemployed and employed are $u(c^u_{i,t}) - \psi_i(e_{i,t})$ and $v(c^e_{i,t})$, with $u(\cdot)$ and $v(\cdot)$ being increasing and concave.

Unemployed's consumption is denoted by $c^u_{i,t}$, and employed's consumption by $c^e_{i,t}$. Each individual receives a wage $w_i$ and pays a tax $\tau$ when employed, and receives benefits $b^k_t$ (which may depend on economic conditions) and non-labor income $y_u$ when unemployed. Savings cannot be lower than a borrowing constraint $\bar{a}_i$.

**Government policy.** I consider an unemployment benefit policy $P$ characterized by a benefit profile $\{b^k_t\}_{t=1}^T$ for $k \in \{B, R\}$. The government commits to a given benefit policy before the start of the

[9] This formulation can accommodate matching functions where search effort and market tightness are substitutes as in Mukoyama et al. (2018). Intuitively, this implies that when economic conditions are bad, job finding may be low but the marginal product of search is potentially high, which is consistent with the empirical evidence of search effort increasing with unemployment rate, even conditional on observables.

[10] The stylized version of the model considers two aggregate economic conditions for simplicity, but can be easily extended to consider a continuum of them.

model. The starting point is an environment with a fixed benefit level and with fixed potential benefit duration $P^k = P$ which does not depend on the cycle, such that agents receive benefit $b_t^k = b$ from $t = 1$ to $t = P$, and $b_t^k = 0$ afterwards. The choice of benefit generosity by the planner is the benefit duration $P^k$, considering a local reform where the duration can be made dependent on the moment of the cycle $k$. For simplicity, the policy characterization ignores time discounting ($1 + r = \beta = 1$). The government's budget is given by:

$$G(P) = (T - D^B - D^R)\tau - \sum_{t=1}^{T} S_t^B b_t^B - \sum_{t=1}^{T} S_t^R b_t^R \tag{1.3}$$

where $D^k = \sum_{t=1}^{T} S_t^k$ refers to total unemployment duration in aggregate conditions $k$, and $S_t^k$ refers to the survival rate at time t and aggregate conditions $k$. Social welfare for a given unemployment benefit policy is:

$$W(P) = \int \omega(\overline{e}_i) V_{i,0}(P)\, di + \lambda[G(P) - \overline{G}] \tag{1.4}$$

where $V_{i,0}(P)$ refers to indirect lifetime utility, and the social planner places welfare weights $\omega(\overline{e}_i)$, which depend on the willingness-to-work type, capturing specific preferences for redistribution towards individuals with specific attitudes towards job search $\overline{e}_i$ (Saez and Stantcheva, 2016). Specifically, individuals are characterized by a willingness-to-work type, defined as the average effort level exerted when faced with a fixed benefit policy and economic conditions as in Boom period: $\overline{e}_i = E[e_{i,t}|k = B, P]$. This characterization thus assigns a type for each individual regardless of her aggregate state, measuring individual effort when individuals are faced with the same set of economic conditions. Moreover, note that the weights do not respond to changes in the benefit policy since they are based on a fixed type. The Lagrange multiplier on the budget's constraint is denoted by $\lambda$, and $\overline{G}$ is an exogenous revenue constraint.

**Reform of benefit generosity.** The planner considers increasing the duration of benefits for the unemployed in Recession and reducing the duration of benefits for the unemployed in Boom $\left(\frac{db_{P+1}^R}{db_{P+1}^B} < 0\right)$, such that the budget remains balanced. At the optimum, the following condition is obtained:

$$\underbrace{\frac{E_{P+1}^{u,R}[\omega(\overline{e}_i)u'(c_{i,P+1}^u)]}{E_{P+1}^{u,B}[\omega(\overline{e}_i)u'(c_{i,P+1}^u)]}}_{\textbf{Marginal Benefit (MB)}} = \underbrace{\frac{1 + FE^R}{1 + FE^B}}_{\textbf{Marginal Cost (MC)}} \tag{1.5}$$

The expression highlights the key statistics needed to evaluate the welfare implications of changes in unemployment benefit generosity over the cycle. The left hand side refers to the marginal benefit of changing benefit generosity, which is shown to depend on two terms. First, it depends on the ratio of marginal utilities of consumption between the unemployed in Recession and in Boom who are near the benefit exhaustion point. Second, it also depends on the ratio of welfare weights between those same unemployed in Recession and in Boom. Given the definition of the types, note that only the differences in mean effort of the unemployed across the cycle that are driven by composition enter the welfare weights, and so changes in behavior are not considered. The right hand side, the efficiency cost, depends on the relative extra costs — or fiscal externalities — due to labor supply distortions that are induced when increasing benefit generosity in Boom and Recession. In summary, the expression implies that the reform increases welfare if and only if the marginal benefit is greater than the marginal cost. This motivates the empirical part of the paper, which implements the expression in order to assess the welfare consequences of the reform.

### 1.2.2 Implementation: Marginal Benefit

In order to take the above expression to the data, I follow a consumption-based approach (Gruber, 1997; Chetty, 2006; Kolsrud et al., 2018; 2024). The approximated empirical counterpart of the marginal benefit term is:[11]

$$\frac{E^{u,R}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]}{E^{u,B}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]} \approx \underbrace{\frac{\omega^R}{\omega^B}}_{\textbf{WW}} \underbrace{\frac{1+\gamma\frac{c^{e,R}-c^{u,R}}{c^{e,R}}}{1+\gamma\frac{c^{e,B}-c^{u,B}}{c^{e,B}}}}_{\textbf{CS}} \tag{1.6}$$

The equation highlights that the value of the transfers depends on the welfare weights associated with the unemployed individuals at each moment of the business cycle, and on the relative consumption changes experienced upon unemployment.

**Consumption Smoothing (CS).** The transfers provide insurance value due to consumption smoothing gains, which are captured by a risk aversion parameter and the relative consumption changes upon job loss by the unemployed at different moments of the cycle. Intuitively, the larger the consumption drop individuals experience upon unemployment, the higher the value of transferring resources towards that state of the world. Thus, the empirical analysis will focus on assessing whether the

[11]Further details are provided in Appendix 1.B.2.

ability to smooth consumption after job loss differs over the business cycle by estimating how the consumption drops vary with economic conditions.

Note that, as shown in Appendix 1.B.2, differences in consumption (marginal utilities) across the unemployed in different economic conditions may arise via consumption changes upon unemployment as shown in the derived expression, but also due to pre-existing consumption differences while employed. The latter would mean that a benefit reform of this type would imply redistribution across pre-unemployment consumption levels. Given that the empirical exercise in Table 1.A2 shows that the (residualized) level of consumption before unemployment is roughly constant over the business cycle, I focus here on the insurance value for the sake of simplicity in the exposition.[12]

**Welfare Weights (WW).** Even conditional on consumption, the type of individuals who are unemployed in Boom and Recession may be different along dimensions that the social planner cares about (Saez and Stantcheva, 2016), which provides an additional argument for targeting resources towards these individuals. In particular, here I focus on a specific dimension — willingness to work ($\overline{e}_i$) — which is proxied in the empirical analysis by the individual's effort level keeping economic conditions fixed as in a Boom period.

If, for example, the social planner places a larger weight on high-effort types, the first best would be to target benefits based on $\overline{e}_i$, but individual effort is not observed.

However, given the outlined framework, the moment of the cycle when people are unemployed reveals information about their type. Intuitively, when the unemployment rate is low and there are many jobs available, some search effort is enough to find a job and only the low-effort individuals will remain unemployed. In contrast, when the unemployment rate is high and there are fewer jobs available, even individuals who search hard are likely to remain unemployed, and so the pool of unemployed shifts towards individuals who are, on average, of higher-effort types.

Given these preferences, and the selection mechanism described, the average welfare weight for unemployed individuals in Recessions would be larger than the one for the unemployed in Boom, and therefore the planner could do better by conditioning benefits on aggregate conditions (Akerlof, 1978).[13] The empirical part focuses on testing empirically these predictions and quantifying the different components— namely whether society has a preference for redistribution along the willingness to

[12]See Kolsrud et al. (2024) for a similar setting in the context of pension benefit reforms.

[13]This idea applies more generally and could be implemented along other dimensions rather than national aggregate conditions, such as job availability in specific sectors or regions. Moreover, it is not necessary that all the selection along the willingness to work dimension occurs through dynamic selection over the unemployment spell, being also possible that composition shifts arise from differential layoff composition over the cycle.

work dimension and whether the composition of the unemployed changes over the cycle. In addition, it also quantifies the effects under a standard (utilitarian) social planner that does not place different welfare weights across individuals. The results are then used to explore the implications for the welfare impact of a cycle-dependent benefit duration reform.

### 1.2.3 Implementation: Marginal Cost

When changing benefit generosity, affected unemployed individuals tend to take longer to find a job, which imposes an efficiency cost on the government budget. The fiscal externality capturing the extra cost due to labor supply distortions, induced by changing potential benefit duration, is:

$$1 + FE^k \approx 1 + \frac{1}{S^k_{P+1}} \left[ \frac{\partial D^k_b}{\partial b^k_{P+1}} b + \frac{\partial D^k}{\partial b^k_{P+1}} \tau \right] \tag{1.7}$$

where $k$ refers to a specific moment of the cycle, $S^k_{P+1}$ is the share of individuals that remain unemployed in period $P+1$, and $D^k_b$ and $D^k$ refer to unemployment and nonemployment duration respectively[14]. Unemployment duration refers to time spent receiving benefits, while nonemployment duration refers to time between job loss and start of the new job. Intuitively, when the benefit at the exhaustion point is marginally increased, unemployed individuals react by taking longer to find a job. This has a negative effect on the budget because people spend more time receiving benefits and also take longer to start paying taxes. The expression measures the cost in terms of the behavioral response induced by each euro transferred mechanically to the unemployed.

## 1.3 Institutional Background and Data

### 1.3.1 Great Recession and Unemployment Insurance System in Spain

**Great Recession.** The Spanish Great Recession was characterized by a strong boom period over the years up to 2007, followed by a slowdown in 2008 and then a substantial bust in the subsequent years. Internal demand had increased by around 20% in real terms since the early 2000s over the years leading up to the recession (Almunia et al., 2021). The rapid growth was in large part due to an increasingly important construction sector, which accounted for 12.4% of Spain's GDP at the peak in 2007. The growth, incentivized by the relatively cheap credit available in the Spanish economy, fueled a substantial housing boom (Martínez-Toledano, 2020). With a one year delay, after the unraveling of the subprime mortgage market in the US, the reduction in credit supply affected the real economy,

[14]The approximation relies on the effect of extending benefit duration in a given economic condition having small effect of individuals' behavior while unemployed in other economic conditions. See Appendix 1.B.1 for further details.

which led to a collapse in internal demand together with a massive reduction in the importance of this sector, which was 5.4% of GDP by 2014. Overall, this resulted into large effects that extended to other sectors, having a substantial impact in terms of labor market outcomes. The national unemployment rate skyrocketed from 7.9% to 26.9% over a period of 6 years. As mentioned before, this magnitude is remarkable relative to international standards as shown in Figure 1.A1.

**UI System.** The Spanish UI system is a national program financed via payroll taxes. Benefit levels and durations are set by the central government and are the same for all regions. In order to qualify for unemployment insurance, a worker must become unemployed involuntarily and have worked for at least 1 year within the last 6 years after the last time of UI reception. Individuals who quit voluntarily, receive full-time disability benefits or whose age is above 65 are not eligible to get UI. Potential benefit duration — the maximum time an individual can spend receiving UI benefits — is a function of work experience within the last 6 years before becoming unemployed. This duration is invariant over the business cycle. It is similar to systems in other countries in Western Europe such as Germany, and different from countries such as the US where the duration is usually extended when the economy is in a bad state. Specifically, potential benefit duration ranges from 4 to 24 months, increasing by two months every six months worked. In relation to benefit levels, they are determined as a function of pre-unemployment labor income and individual characteristics. The labor income considered is average gross earnings within the last 6 months and the replacement rate is 70%, getting reduced to 60% after 6 months unemployed.[15] From July 2012 onwards, the latter replacement rate was set to 50%. After UI benefit exhaustion and depending on individual characteristics, individuals remaining unemployed may apply for unemployment assistance.

### 1.3.2 Data

**Consumption.** I use household expenditure survey data from the Spanish EPF (*Encuesta de Presupuestos Familares*) over 2006-2015. This yearly survey provides detailed diary-based information on household consumption, labor market status, and demographic characteristics. It is available over a long period of time, so proves useful to study dynamics over the business cycle far from the current period. The waves covered here correspond to an improved version of the survey where the sample size, duration of households' diaries completion and quality have increased substantially with respect

[15] In the stylized model, I focus on the duration of the benefits and make the simplifying assumption that the benefit level is constant up to the exhaustion point. See e.g. Kolsrud et al. (2018) for work considering variation in benefit profiles over the unemployment spell.

to previous waves (INE, 2008). The survey has a panel dimension, with households being followed for two consecutive years, and contains 80 consumption categories which are classified following the COICOP/HBS classification. It accounts for 87% of aggregate consumption, which compares favorably to figures obtained in other countries (Heathcote et al., 2010; Campos and Reggio, 2015). A limitation, shared with other surveys, is that unemployment duration or potential benefit duration are not present in the dataset. Thus, the empirical analysis focuses on consumption patterns of the average unemployed around job loss, and not specifically on those at the moment of benefit exhaustion.

I construct an expenditure measure that includes expenditure on a wide variety of goods and services: Food, Alcohol/tobacco, Clothing, Housing, Inventories, Health, Transport, Telecom, Recreation, Education, Hotels.[16] I exclude rent for homeowners (which is imputed), and deflate this measure using annual CPI. I restrict my sample to household heads aged 25-64 who were either employed in both periods or employed in $t$ and unemployed in $t+1$, and had less than a threefold change in consumption (Gruber, 1997; Kroft and Notowidigdo, 2016; Hendren, 2017).

**Search Effort.** My measure of search effort is constructed from the Spanish Time Use Survey (*Encuesta de Empleo del Tiempo*). Specifically, in order to explore variation over the business cycle, I use the two available waves in years 2002-2003 and 2009-2010. The survey is harmonized at the European level by Eurostat as part of the Harmonized European Time Use Surveys, and is similar to the American Time Use Survey (ATUS). Its objective is quantifying the time spent by individuals on various activities throughout the day. To that end, respondents fill questionnaires and time diaries where they record their daily activities in 10-minute slots over one day, so the survey structure is a cross section.

For the analysis, I focus on the unemployed aged 25-64 and use the information available on job search related activities to construct a measure capturing whether individuals report searching for jobs in a given day. Similarly to the consumption data, the dataset does not contain information on unemployment duration or potential benefit duration, so I focus on the behavior of all unemployed instead of individuals just around the benefit exhaustion point. More details on the construction on the search effort measure are presented in Appendix 1.C.

**Preferences for Redistribution.** I design and administer an online survey to understand preferences for redistribution between the unemployed based on search effort types. To do so, I program a new survey using oTree and partner with the company Respondi/Bilendi to obtain survey respondents

[16] This corresponds to a more comprehensive version of the expenditure measure in Campos and Reggio (2019).

from Spain. This panel company has been previously used in the literature for other studies (e.g., Alesina et al., 2018; 2023). The survey was administered in August 2023, with a sample size of N=1022, and respondents being rewarded with €1.15 for an average completion time of 8 minutes. The sample is representative of the Spanish active population aged 25-64. The data collection was performed to target 24 age-education-sex-employment cells based on the Spanish Labor Force Survey (*Encuesta de Población Activa*). In order to maintain data quality, I further restrict the sample to individuals with a completion time between the 5 and 95 percentiles, and whose answers to the qualitative and quantitative questions explained below are consistent. This results in a final sample of 709 individuals. The survey is designed following recommended practices in the literature (Stantcheva, 2022). In order to address order effects, and understand reasons behind the responses, it has features such as order randomization and open-ended questions to make sure that results are not driven by specific characteristics of the implementation.

The survey first starts with a section collecting information on demographics and labor market status. It then elicits respondents' preferences for redistribution between the unemployed based on effort types, in both qualitative and quantitative ways. To do so, respondents are presented with scenarios where they are asked to allocate government funds between two individuals with different search effort behavior in the previous week, allowing to estimate relative valuations between them. Finally, the survey concludes with open-ended questions to understand the reasons for their choices, and multiple choice questions to know whether they would support a hypothetical UI reform of the type studied in the paper. Appendix 1.E provides more detailed information about the survey structure including the full questionnaire.

**Unemployment Spells.** Information on unemployment spells and unemployment benefit transfers comes from the Social Security administrative records from the Continuous Sample of Employment Histories (*Muestra Continua de Vidas Laborales*). They are effectively matched employer-employee data with daily longitudinal information created by matching income tax data, Social Security records and census data. It covers a 4% non-stratified random sample of individuals that, for any year, have a relationship with the Social Security. My sample comprises the years 2006-2017 to build a dataset which contains the complete history of employment and unemployment spells since entrance in the labor market for the group of individuals who had any relationship with Spain's Social Security during the years of the waves. The variables of interest include information on sex, education, age, experience, job location, earnings, hours, occupation, industry and contract type. Moreover, I observe both unemployment duration (days receiving unemployment benefits) and nonemployment duration (days between the start of UI reception and the beginning of a new employment spell). An important variable in the analysis is potential benefit duration, which is not present in the dataset. For this reason, I compute it applying the UI eligibility rules to the working history of every individual. For the main analysis, given difficulties in the empirical computation of potential benefit duration, I focus on prime-age individuals with PBD between 8 and 22 months.[17] I cap my measure of nonemployment duration at 3 years following common practice in the literature (e.g., Schmieder et al., 2012; Lindner and Reizer, 2019).

**Unemployment Rate and Demographics.** The information on economic conditions is based on the microdata files of the Labor Force Survey (*Encuesta de Población Activa*) provided by the National Statistics Institute (INE). I compute unemployment rate measures at the region-year level over the period 2006-2017 for the analysis of the different components over the business cycle. Given differences in availability of geographical information across datasets, regions correspond to (52) provinces in the unemployment spell analysis, and (17) autonomous communities (CCAA) in the consumption and search effort analyses. I also use this dataset for two additional tasks: (i) obtaining shares of population for each demographic cell to achieve representativeness of the online survey; (ii) performing the prediction exercise based on search effort as described in Section 1.4.2.

## 1.4 Empirical Approach and Results

The model described in Section 1.2 guides the empirical strategy, which aims to recover the required statistics for the assessment of the optimal benefit generosity of the UI system. I start by looking at the evolution of consumption and the welfare weights, and continue analyzing the efficiency cost of the transfers.

[17] In the dataset constructed, I require that individuals were previously working in the general regime of the social security. Moreover, I restrict to the first spell of each individual in the sample. Given the statutory rules, an individual that gets unemployed but does not exhaust the PBD and works for a certain amount of time, can choose upon unemployment what the new PBD will be between the remaining months from the old spell and the new ones. This information is missing in the data and thus makes difficult assignment of the running variable so I do not consider those spells. Moreover, I focus on individuals with PBD from 8 to 22 months, discarding the cutoffs at the bottom and at the very top. Given these rules together with short contracts for individuals located around the first cutoffs, apparent bunching appears near these cutoffs. The last cutoff also presents bunching which is mechanical, given that the maximum work experience counted is 6 years, and those individuals tend to have been employed longer than that. In addition, I focus on individuals aged 26-54 given the longer work spells that enable better assignment into treatment, and the lower likelihood to be impacted by retirement and unemployment assistance policies. Finally, the most recent individuals included are the ones becoming unemployed in 2014, so that I have three years after the start of UI reception for all people in the sample. Summary statistics are shown in Table 1.A4.

### 1.4.1 Estimating Consumption Changes

Assessing the insurance value of providing more generous UI transfers in bad times relative to good times requires getting a sense of how individuals change their consumption upon unemployment, and whether this change varies with economic conditions.

To quantify these patterns, I adopt a Difference-in-Differences strategy similar to Gruber (1997), where I compare the consumption behavior of heads of household getting unemployed with those who remain employed over the business cycle. Specifically, I estimate the following model:

$$logC_{irt} = \beta_0 + \beta_1 Unemployed_{it} + \beta_2 Unemployed_{it} \times U.Rate_{rt} + \lambda_i + \lambda_r + \lambda_t + X'\beta + \varepsilon_{irt} \tag{1.8}$$

where $logC_{irt}$ refers to log consumption measured at the household level, $Unemployed_{it}$ is a dummy variable that equals one when the household head is unemployed, $U.Rate_{rt}$ refers to the unemployment rate in the region of the individual, and $\lambda_i$, $\lambda_r$, $\lambda_t$ are individual, region and year fixed effects, respectively. The vector of controls $X$ includes the regional unemployment rate, and information on type of household, intended to control for any mechanical relationship between household composition and economic conditions or job loss.

Table 1.1 shows the main results on consumption smoothing. First, in column (1), we see that, on average, individuals experience a 9.6% (s.e. 1.3%) consumption drop upon unemployment. From column (2), which is the baseline specification, we see that the average consumption drop masks a clear gradient with respect to economic conditions. A 1 p.p. increase in unemployment rate results in an additional -0.4% (s.e. 0.1) change in consumption relative to pre-unemployment level. From column (3), which includes region-times-year fixed effects, we see that the consumption drop covaries similarly with economic conditions. These models include controls for type of household. Columns (4)-(6) include additional covariates that intend to capture changes in household composition. Results are very similar to the ones without these covariates.

Figure 1.1 depicts the main result, focusing on the baseline specification and grouping individuals by their region's unemployment rate. The groups are selected so that the average unemployment rate in each group roughly approximates the national average in Boom, normal times and Recession. It shows that the within individual change in consumption between employment and unemployment gets significantly and monotonically larger when the unemployment rate increases. This is in contrast to the findings of Kroft and Notowidigdo (2016), where the authors do not find that the individual drop gets larger when economic conditions deteriorate. The difference in findings may be explained by limited power to detect an effect in their study given the relatively small sample size and/or variation in unemployment rate, and the fact that the consumption measure only captures food components. In my setting, there is relatively larger variation in economic conditions and a more comprehensive consumption measure that goes beyond good consumption, which allows me to revisit this issue and get further insights.

To do so, I decompose the change in the consumption drop over the cycle by consumption category in order to understand what components of the consumption basket drive the cyclicality of the overall drop. Specifically, I estimate models of the form:

$$ShareC^{j}_{irt,0} = \beta_0 + \beta_1 Unemployed_i + \beta_2 Unemployed_i \times U.Rate_{rt} + \lambda_i + \lambda_r + \lambda_t + X'\beta + \varepsilon_{irt} \quad (1.9)$$

where $ShareC^{j}_{irt,0}$ corresponds to the share of each consumption category $j$ relative to total consumption of the individual in the first year observed in the sample. Then, for each category, I compute the difference between the consumption change in Recession and the change in Boom.[18] I focus on the preferred specification (column (2) in Table 1.1), now analyzing consumption shares. Note that considering shares relative to the previous year provides a clear interpretation since adding up all the estimates by category yields the total change in the consumption drop documented before (Table 1.1). The results are depicted in Figure 1.2, where I find that the drop in food consumption upon unemployment gets somehow larger in bad times but the magnitude is not statistically significant. Nonetheless, the larger drop in overall consumption in Recession arises mainly from larger drops in a few consumption categories: housing, utilities, and clothing and shoes.[19] Therefore, my findings can help reconcile the empirical result in Kroft and Notowidigdo (2016). Although their theoretical model predicted a larger consumption drop in bad times, they find that it does not significantly vary with economic conditions when measured only by the food component. Given my findings, it thus seems that focusing only on a component such as food consumption that is relatively inelastic may limit the understanding of the welfare costs of job loss. Although it is sufficient in theory to just use food consumption — provided that one uses the appropriate curvature of utility over food (Chetty, 2006) —, in practice obtaining estimates that are precise enough to detect cyclical variations in this component may be difficult. Therefore, this result highlights the importance of empirically assessing consumption behavior using a more comprehensive measure.

Overall, the results here have shown that the unemployed in bad times suffer considerably larger

[18] This statistic ($\frac{\Delta^{ue,R}}{c} - \frac{\Delta^{ue,B}}{c}$) is computed as $\beta_2 \times (U.Rate^R - U.Rate^B)$, where economic conditions are defined as Recession corresponding to 26% and Boom to 8% unemployment rate, respectively.

[19] Note that the design compares the consumption behavior of the unemployed relative to employed individuals in the same region and time period, so that the results are not explained by the aggregate housing boom and bust documented in Section 1.3.1 which are common across groups.

consumption drops upon unemployment than the unemployed in good times. Moreover, this additional drop comes from countercyclical drops in non-food categories such as housing and utilities, and clothing.

**Additional Evidence and Robustness.** Further analysis presented in Table 1.A1 explores mechanisms and sensitivity of the main results. While the findings above reveal that overall consumption declines in bad times, here I start by exploring whether this is just driven by longer unemployment duration in recessions. Results are robust to controlling by either regional unemployment duration or regional unemployment duration distribution (share of unemployed longer than 6 months). This suggests that the larger drop found in bad times is not fully explained by longer unemployment spells, with larger drops at a given duration potentially being the main driver.[20]

Additionally, I explore whether the 2012 reform which slightly decreased the replacement rate after 6 months (see Section 1.3.1) can mechanically explain the countercyclical drop in consumption. The findings indicate that this policy change does not drive the main results.

In relation to differences in consumption across the unemployed in different economic conditions, they may also come from pre-unemployment differences in consumption as mentioned before. Table 1.A2 examines whether the pre-unemployment gap in consumption for individuals who become unemployed changes over the business cycle. Following the main specification, conditional on region and year fixed effects, the results indicate that the consumption gap does not change with economic conditions. This implies that the benefit reform does not induce redistribution in terms of consumption levels, which is the motivation for focusing on the insurance aspect in the main analysis.

### 1.4.2 Estimating Welfare Weights

A key statistic to evaluate how the benefits provided by UI transfers vary with economic conditions is the ratio of welfare weights. Specifically, these weights capture society's relative valuations between the type of unemployed individuals receiving the transfers at different moments of the cycle, who may differ in their general attitudes towards job search.

To estimate this parameter, I proceed in three steps. First, I study preferences for redistribution between individuals of different willingness-to-work types. Second, I analyze whether the composition of the unemployed changes along the willingness-to-work dimension over the cycle. Third, I combine

[20] Kolsrud et al. (2018) show that the consumption drop gets larger over the unemployment spell in the Swedish context. Unfortunately, a measure of unemployment duration is not available at the individual level in the Spanish consumption data, so I construct it at the region-year level from the LFS, potentially introducing some measurement error that may attenuate the results.

both results in order to estimate the ratio of welfare weights between the unemployed in Recession and the unemployed in Boom.

### Preferences for Redistribution over Willingness-to-Work Types

I build on the idea of the generalized social marginal welfare weights of Saez and Stantcheva (2016) in order to understand whether society has a preference for redistribution over the willingness to work dimension. To do so, I design and administer an online survey to estimate the statistics of interest highlighted by the framework. Here I describe the key questions, and refer the reader to Appendix 1.E for more detailed information.

After responding a set of demographic questions, individuals are shown the first question that is intended to elicit preferences for redistribution in a qualitative way. The question reads as follows:[21]

> *Imagine there are 2 unemployed individuals, A and B, who receive €950 while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed:*
>
> *A looked for jobs last week*
>
> *B did not look for jobs last week*
>
> *The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Please choose what allocation option you would prefer:*
>
> – *More money to A than to B*
> – *More money to B than to A*
> – *Same amount to A and B*

The answers to this question are shown in Figure 1.3a, which shows that respondents have strong preferences for redistribution over effort types. Around 75% of respondents prefer to allocate more resources to the individual that was looking for work in the previous week. Almost all of the other respondents would allocate the same resources to both individuals — meaning that they would not redistribute based on this dimension—, with virtually no respondents preferring to give more to the individual who did not search.

Although a stark qualitative picture emerges from the answers to this question, this is not enough to estimate the statistic we are after. Therefore, in order to make progress on that front, after the qualitative question, respondents are presented with 10 quantitative questions designed to obtain the relative valuations between types. They are of the following form:

[21] Note that the hypothetical benefit amount and potential duration correspond to the national average at the time of the survey as reported by the Spanish Employment Agency (SEPE).

*Imagine there are 2 unemployed individuals, A and B, who receive €950 while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed:*

*A looked for jobs last week*

*B did not look for jobs last week*

*The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Now imagine that an unequal allocation may be costly. That is, it can allocate €500 each, or more to one than the other, but the latter option may result in the total amount of resources that can be distributed being reduced. Please choose what allocation option you would prefer:*

- *500 for A and 500 for B*
- *550 for A and 400 for B*

For each of these questions, there are always two allocation options: one offering the same amount for both individuals, and the other one where the amounts differ between them. Under the equal allocation option, both individuals receive the same amount of €500 (i.e. $r_0^A = r_0^B = 500$). Under the alternative option, one individual gets €550 ($r_1^A = 550$) and the other individual an amount $r_1^B$ obtained from the set $\{50, 100, 150, 200, 250, 300, 350, 400, 450\}$.[22] The identity of individuals A and B, the order of questions, as well as the order in which the equal allocation option appears within the question are all randomized. This intends to alleviate concerns about order effects.[23] In practice, the experimental variation created in the cost of the transfer, or Marginal Rate of Transformation (MRT), allows to identify the relative valuation over individuals, or Marginal Rate of Substitution (MRS), and induces individuals to accept the unequal allocation whenever:

$$\frac{\omega(\overline{e}_A)}{\omega(\overline{e}_B)} = MRS \geq MRT = \frac{r_0^B - r_1^B}{r_1^A - r_0^A} \tag{1.10}$$

Figure 1.3b depicts the share of individuals that choose an unequal allocation for different values of the cost of the transfer. First, we see that when the cost of the transfer is low, a large share of respondents choose the unequal allocation that gives more to individual A. Specifically, when there is no cost of transferring resources between individuals (MRT=1), the share is around 75%, which equals

[22] In practice, all individuals are presented with the same two options of 550 to A (B) and 450 to B (A). This allows me to assess data quality by comparing answers, for the same individual, to a question with no cost of transfer and asked in both qualitative and quantitative way. The other 8 questions are randomly drawn from the remaining set of transfer amounts.

[23] Note that the fact that each individual is presented only with 10 of these questions, and that there is order randomization, only has the objective of reducing fatigue and increasing data quality. This design does not seek to create a treatment and a control group to estimate any causal effect since the analysis pools all individuals together. The aim is just to avoid making respondents answer about all potential options with different transfer costs while being able to learn about the distribution of preferences in the population.

the figure obtained in the qualitative question.[24] Moreover, we see that the share declines as the cost increases, implying lower willingness to redistribute as more resources are lost to implement a given transfer. Overall, respondents exhibit a strong preference for redistribution from the individual that did not search last week and towards the one that searched. However, when asked to redistribute in the opposite direction, towards the individual that did not search, almost no one is in favor of that allocation. Leveraging the responses of individuals, I estimate the relative valuation (or average marginal rate of substitution) between the type of individual A and B as follows:

$$\frac{\omega(\bar{e}_A)}{\omega(\bar{e}_B)} = \sum_j (s_j - s_{j+1}) MRT_j \tag{1.11}$$

where $s_j$ corresponds to the share of individuals accepting an unequal allocation when presented with an allocation option of cost $MRT_j$. Thus, I effectively compute a weighted average of the (binned) MRS distribution by summing over bins corresponding to all MRT values. Given the relatively high share still present at the largest cost considered in the design (MRT=9), my preferred specification assumes a linear extrapolation with a downward slope as estimated over the range before that point. Following this approach I obtain that $\frac{\omega(\bar{e}_A)}{\omega(\bar{e}_B)} = 12.8$.[25] Consequently, respondents have a strong preference for individuals who exert effort, with a valuation of around 13 times relative to individuals not searching for jobs.

**Additional Evidence and Robustness.** It is plausible that respondents support redistribution based on effort, not due to a direct concern for effort itself, but rather because of some other underlying factor that correlates with this dimension. In order to understand what respondents have in mind when answering the questions, I ask them open-ended questions about the reasons for their allocations. A wordcloud with their answers is depicted in Figure 1.3c, where the most common expressions include "search for job", "effort" and "reward", which seem to be directly related to the dimension of interest.[26]

[24] As mentioned before in Section 1.3.2, the final sample excludes individuals that provide contradictory responses to the same question, asked in both a qualitative and a quantitative way.

[25] Without extrapolating, assuming that all individuals at the upper tail of the distribution have exactly MRS=9, I obtain a lower bound of $\frac{\omega(\bar{e}_A)}{\omega(\bar{e}_B)} = 4$. As I show later, the main statistic of interest, which is the ratio of welfare weights between the unemployed in boom and recession, is not very sensitive to this issue.

[26] Nevertheless, although respondents care about the effort dimension per se, there seems to be an understanding of confounders and heterogeneity in the population. For example, the analysis of the open-ended answers reveals that some individuals have in mind the case of single mothers with kids. They suggest that, despite this group potentially exerting less effort compared to others, they prefer not to support redistribution based on the effort dimension in this context. For this reason, the question used to elicit preferences intentionally does not fix other characteristics of the hypothetical individuals beyond the benefit policy, given that the unemployed's composition may change over the cycle along various dimensions in addition to effort. As a result, while understanding how preferences for redistribution vary depending on the source of effort differences is beyond the scope of this paper, the exercise below, which computes welfare weights over the cycle,

Additionally, in Figure 1.A2, I explore whether the preferences for redistribution are driven by specific groups that may be more likely to benefit from the policy. Remarkably, I find that preferences are homogeneous and are not driven by specific groups. Unemployed individuals present slightly reduced support for redistribution based on willingness to work relative to the employed, but still more than 50% wishes to reward effort.

I also consider whether previous experiences of respondents, during their impressionable years (18-25) (Krosnick and Alwin, 1989), affect their stated preferences. In this sense, I do not find that long-term unemployment of the parents or household's economic difficulties during this important period impact preferences along the willingness to work dimension.

While the baseline implementation elicits preferences for redistribution between types while fixing the economic environment, it could be the case that the welfare weight function also depends on economic conditions itself, on top of the unemployed's composition changes. To explore the importance of this issue, I examine whether preferences are heterogeneous by the unemployment rate of the respondents' region. As shown in Figure 1.A2, preferences are stable across economic conditions.

Finally, I explore qualitatively whether there is hypothetical support for a UI reform of the type studied in this paper. Suggestive evidence from Figure 1.3d indicates that respondents claim they would tend to approve the proposed differentiation of the policy over the cycle. See Appendix 1.D for further analysis on this aspect.

**Unemployed's Composition**

The second piece of information required is whether the composition of the unemployed shifts towards high willingness to work types when economic conditions worsen. Intuitively, a reduction in labor demand where the offer arrival rate is low potentially makes search less effective and thus creates a selection mechanism where, all else equal, high effort types are more likely to remain unemployed relative to a situation when the unemployment rate is low. Here I provide empirical evidence that demonstrates the relevance of this mechanism.[27]

To do so, I focus on the following measure: the share of unemployed that search for jobs in a given

accounts for changes in the composition of the unemployed along dimensions that correlate with effort to the extent that respondents incorporate that heterogeneity/correlation. In relation to this, it is also important to note that the question fixes the potential unemployment benefit policy, and the transfers are lump-sum. This aims to obtain responses that are orthogonal to differences in consumption and to capture preferences based on effort.

[27] As mentioned before, since the statistic of interest is average willingness to work over the cycle, it is not necessary that the main mechanism driving composition changes is a decrease in job finding in bad times. This could also be explained by a different composition of layoffs.

week.[28] Specifically, I am interested in how this measure changes with economic conditions, but only to the extent that is driven by a composition shift. As described before, given that the unemployed may change their search effort behavior in response to changes in the offer arrival rate over the cycle, my measure of effort type fixes effort at the level that would be observed were the unemployed faced with an offer arrival rate as in Boom (i.e. unemployment rate of 8%).

I leverage the two available waves of Spanish Time Use data (2002-2003 and 2009-2010).[29] Exploiting the variation in unemployment rate across regions over the two time periods, I obtain the results shown in Figure 1.4.[30] We observe that the share of individuals that search in a given week increases strongly with the unemployment rate. Specifically, it goes from around 50% when the unemployment rate is 8% to more than 80% when the unemployment rate exceeds 20%.

Importantly, not all the fluctuations in search effort are potentially due to a composition effect.[31] As mentioned before, individuals may also change their search behavior while unemployed in response to changes in the availability of jobs. Evidence from Mukoyama et al. (2018) with panel data from the US indicates that around 50% of the effort increase observed over the cycle is due to a composition effect, with both observable and unobservable dimensions playing an important role.[32] Applying this magnitude to my context in order to only consider changes due to composition, I obtain that the share of unemployed individuals searching in a given week increases with the unemployment rate, going from 49% in Boom to 68% in Recession.

For the estimation of the welfare weights below, I perform a sensitivity analysis where I consider different magnitudes for the change due to composition effects.[33] In addition, I also perform a complementary prediction exercise only using Spanish data, where I predict search effort based on the set of demographics present in both the Time Use data and the Labor Force Survey. Specifically, I

[28]This measure captures an intensive margin of job search. Given that according to the International Labour Organization (ILO) definition of unemployment, a requirement to be considered unemployed is to have searched for jobs in the last month, this explores the degree of search intensity within that period. It is also a relevant measure considered in other surveys like the Survey of Consumer Expectations (SCE) carried out by the Federal Reserve Bank of New York (Faberman et al., 2022).

[29]For more details on data construction, see Appendix 1.C.

[30]Given data availability the analysis uses only two years, and residualizes the variables on region FEs to not consider variation from structural differences across regions that are constant over time. Note that the relationship documented is not only present in Spain, and does not depend on the specific search effort measure or the specification used. See Table 1.A3 for evidence of overall countercyclicality of usage of different search methods over 15 years across 34 countries using data from Eurostat.

[31]For related literature that investigates the drivers of the cyclicality in the job finding rate instead of search effort, see e.g. Kroft et al. (2016) and Mueller and Spinnewijn (2023).

[32]The authors use market tightness as the measure of economic conditions and minutes of search as the measure of search effort. They exploit the panel dimension of the Current Population Survey (CPS) to account for both observables and unobservables. Findings from their working paper using unemployment rate instead present similar results.

[33]See Table 1.3 for estimates of the Marginal Benefit term under different implementation assumptions regarding composition changes.

estimate the relationship between search effort and the demographic characteristics based on the Time Use data and then impute search effort in the Labor Force Survey using the same demographics.[34] Note that the set of demographics in both datasets is limited and I do not have a panel dimension in the Labor Force Survey to account for unobservables, so this provides a lower bound on the magnitude of composition shift over the cycle that I can detect relative to the richer data setup in Mukoyama et al. (2018). Nonetheless, Figure 1.A4 reassuringly shows that predicted search effort changes over the business cycle even when only considering this limited set of demographics present in the Spanish data.

**Combining Information: Unemployed in Recession vs Boom**

The previous sections showed that society exhibits strong preferences for transferring resources from low to high willingness-to-work individuals. Moreover, the composition of the unemployed shifts towards high willingness-to-work types in bad times. Here I combine both pieces of information to obtain an estimate of how individuals value the unemployed in Recession relative to Boom.

As we found before, we have $\frac{\omega(\bar{e}_A)}{\omega(\bar{e}_B)} = 12.8$. Then, from the exercise employing Time Use data we have that the shares of each effort type in Boom and Recession are:
$s_A^{Recess.} = 0.49$; $s_B^{Recess.} = 0.51$; $s_A^{Boom} = 0.68$; $s_B^{Boom} = 0.32$. After that, I compute $\omega^R = (s_A^{Recess.})\omega(\bar{e}_A) + (s_B^{Recess.})\omega(\bar{e}_B)$ and $\omega^B = (s_A^{Boom})\omega(\bar{e}_A) + (s_B^{Boom})\omega(\bar{e}_B)$. As a result, the ratio of welfare weights is $\frac{\omega^R}{\omega^B} = 1.31$, which indicates that society values a €1 transfer to the type of individuals unemployed in Recession as much as €1.31 to those unemployed during a Boom.

This implies strong preferences for redistribution along the specific effort dimension, a result that is robust to different implementation assumptions. On the one hand, under the strong assumption that the distribution of MRS does not have values larger than the ones randomized, we get a ratio of 1.22. On the other hand, using the baseline estimate, even if the part of the search effort increase in Recession that is due to composition is 25% — half of the one found in the US —, the magnitude is 1.15. Alternatively, using the variation in effort due to composition effects obtained from the prediction exercise that only uses the sparse set of demographics in the Spanish Time use data combined with LFS, I obtain a similar magnitude: 1.13. As explained before, this likely understates the composition shift given the inability to account for unobservables and the sparsity of the predictors set present in

[34] For this exercise, given the availability of only two waves of the Time Use Survey, I focus on the years up to 2010 which is the last period for which search effort is actually observed and so we can compare measured search effort with predicted (by composition) search effort. The demographics present in both datasets that are used in the prediction model are interactions between age categories and sex; between education categories and sex; and between married status and sex.

the data.

In summary, this section has demonstrated that there is an important shift in the composition of the unemployed over the cycle, and that society values substantially more the type of individuals that receive transfers in bad times.

### 1.4.3 Estimating Efficiency Cost

When increasing potential benefit duration (PBD), individuals may change their search behavior and thus impose some extra cost on the government budget. Equation (1.7) highlights the main statistics needed to quantify this component, namely the marginal effect of potential benefit duration on unemployment $\frac{dD_b^k}{db_{P+1}^k}$ and nonemployment duration $\frac{dD^k}{db_{P+1}^k}$, and the exhaustion rate $S_{P+1}^k$.

In order to gain a better understanding of the effect of PBD on unemployment and nonemployment duration, I start by providing graphical evidence of the behavior exhibited by unemployed individuals when presented with additional benefit generosity. As outlined before, the institutional setup delivers a treatment assignment mechanism typical of a Regression Discontinuity design, and thus my empirical strategy exploits the discontinuities in potential benefit duration present in the UI system. Figure 1.5 illustrates the structure of the UI system for the main sample, where it is shown that PBD discretely increases by 2 months at specific cutoffs of recent work experience. To estimate the statistics of interest, the empirical approach pools all cutoffs together. First, I provide evidence for the validity of the design in two ways: testing whether there is any discontinuity in the density of the running variable following McCrary (2008), and assessing whether selection on observables could explain the effects on the outcomes of interest (Card et al., 2015). I do not find discontinuities in the density or in the covariates indexes of the outcomes as depicted in Figure 1.A5. Second, I proceed to analyze the impact of higher benefit generosity on the main labor market outcomes as shown in Figure 1.6. Panel (a) shows the discontinuity in PBD that arises when crossing the cutoff. Panels (b) and (c) depict the conditional expectation functions of unemployment and nonemployment duration, which also present discrete jumps exactly at the cutoff, consistent with PBD having a positive effect on both outcomes. To quantify these effects, I estimate models of the following form:

$$Y_{irt} = \alpha + \beta \mathbb{1}[Experience_i > 0] + \phi \mathbb{1}[Experience_i > 0] \times U.Rate_{rt} + X'\theta + \lambda_r + \lambda_t + \varepsilon_{irt} \tag{1.12}$$

where $Y_{irt}$ refers to potential benefit duration, unemployment duration and nonemployment duration, $\mathbb{1}[Experience_i > 0]$ is a dummy for being on the right side of the normalized cutoff, $U.Rate_{rt}$

refers to unemployment rate at the region level from the quarter when the individual became unemployed, $X$ is a set of controls including a (linear) polynomial in normalized work experience within the last 6 years and the regional unemployment rate, and $\lambda_r$ and $\lambda_t$ are region and time fixed effects, respectively.

The results are shown in Table 1.2. In column (1), we see that PBD increases at the cutoff by 60.33 (s.e. 5.44) days, reflecting the discontinuous structure of the UI schedule previously described. In column (3), we see that unemployment duration increases by 25.05 (s.e. 7.72) days in response to the higher PBD enjoyed when crossing the cutoff. When looking at the effect on nonemployment duration, column (5), we see that there is an increase of 43.30 (s.e. 15.56) days. In terms of marginal effects, they correspond to an effect of PBD on unemployment duration of 0.42, and an effect of PBD on nonemployment duration of 0.72. These estimates are at the upper tail of the distribution in the literature as surveyed by Schmieder and Von Wachter (2016). For the effect on nonemployment duration, which is the one that has received more attention in the literature, I find an elasticity of 0.92. This is closer to the largest estimate after excluding outliers reported in the survey, with a value of 1. All of these together imply that providing the unemployed with more generous UI benefits induces large behavioral responses.
Columns (2), (4), and (6) investigate whether these behavioral responses vary when the unemployment rate increases. I do not find that these marginal effects are different from each other when the increase in generosity is implemented in Boom relative to Recession.

Now I turn to assessing how the exhaustion rate — the value of the survival function at the benefit exhaustion point — varies with the unemployment rate. I estimate it nonparametrically in the following way:

$$S^k_{P+1} = \frac{1}{N}\sum_{i=1}^{N} \mathbb{1}[D^k_i > P^k_i | k] \tag{1.13}$$

which captures the share of individuals with a nonemployment duration $D^k$ longer than their potential benefit duration $P^k$, where $k$ refers to different values of the regional unemployment rate. Figure 1.7 depicts the results, where it is shown that the share of individuals that are not able to find a job before exhausting unemployment insurance benefits increases with the unemployment rate, going from 0.19 to 0.34 when the unemployment rate changes from 8% to 26%, respectively.

## 1.5 Welfare Implications

In relation to the value provided by the unemployment insurance transfers, the empirical analysis above has shown that there are larger social benefits of increasing the potential duration of benefits in bad times relative to good times. Table 1.3 presents the estimates for the marginal benefit term, combining the findings on the value of insurance and the welfare weights. For the benchmark specification, with $\frac{\omega^R}{\omega^B} = 1.31$ and a risk aversion parameter $\gamma = 4$ (Landais and Spinnewijn, 2021), I find that the social benefits of transferring resources towards the unemployed in recession is 64% larger than for the unemployed in boom. Moreover, a main takeaway that arises from the sensitivity analysis in Table 1.3 is that, regardless of the implementation assumptions, the value provided by UI transfers is larger in recessions. This is true when focusing only on the insurance value, but the magnitude is considerably larger when also accounting for the preferences for redistribution together with the shift in the composition of the unemployed along the effort dimension.

In relation to the cost, the estimates above imply that benefit extensions are less costly when economic conditions are bad. Using those estimates to calibrate the fiscal externality formula in equation (1.7), I obtain the results presented in Table 1.4. At the mean unemployment rate (15%), for every €1 transferred mechanically to the unemployed, the government needs to levy €1.77 on top of that because of the behavioral response. This efficiency cost is measured as euros of behavioral cost per euro of total mechanical transfer. Moreover, the fiscal externality is larger in good times, ranging from €1.35 in recession to €2.42 in boom. Overall, these results are consistent with the ones obtained by Schmieder et al. (2012), where they find that marginal effects remain constant over the cycle, with the only relevant component that changes with economic conditions being the exhaustion rate. As pointed out by Landais et al. (2018a;b), this type of analysis does not consider general equilibrium mechanisms and one should include a correction term to the optimal formula in order to account for them. The evidence from this literature points towards a correction term that is countercyclical. Therefore, in that case the cyclicality of the marginal cost becomes more pronounced, and so my estimates can be potentially thought as lower bound estimates of the countercyclicality of this component.[35]

[35] As mentioned before, when estimating the effect of PBD on nonemployment duration, the design keeps labor market tightness constant so that the estimate refers to a micro effect. As pointed out by Landais et al. (2018a;b) in the case of benefit levels, one must consider not only the microelasticity but also the macroelasticity. If tightness is not efficient, in addition to the term in my formula which captures the effect of UI on welfare holding tightness constant, one should also add a correction term which is equal to the effect of UI on tightness times the effect of tightness on welfare. On the one hand, available evidence in the literature suggests that the macroelasticity is larger than the microelasticity meaning that UI increases tightness. On the other hand, the effect of tightness on welfare is found to be countercyclical. Hence, given that the correction term would be countercyclical, the potentially countercyclical behavior of my partial equlibrium estimate can be thought as a lower bound for the one including general equilibrium mechanisms.

Now I finally combine both, social benefits and costs, and proceed to quantify the net welfare impact of changing potential benefit duration according to the state of the economy. To do so, I consider the effect of the budget-balanced reform described above such that $b^R_{P+1} > b^B_{P+1}$ as shown in Appendix 1.B.3. Normalizing by unemployed's marginal utility in boom, I find that that the net welfare impact of the reform is €0.95. That is, society would gain an extra €0.95 per euro transferred from individuals unemployed in boom to individuals unemployed in recession. Moreover, I decompose the welfare gain between the different components as shown in Figure 1.8. I find that they are all quantitatively important. Specifically, the welfare weights component accounts for around 37% of the total gain, while the consumption smoothing accounts for 30% and the efficiency cost channel for 33%. This implies that there are large welfare gains from having a system where the generosity of the transfers varies with economic conditions. Importantly, this analysis highlights that the welfare gain not only increases in Recession because of an efficiency cost decline as emphasized by the bulk of the empirical literature, but also because the social benefits of the transfers increase considerably in bad times. This increase in social benefits is found when focusing only on the insurance channel, but this becomes substantially more pronounced when accounting for preferences for redistribution and changes in the composition of the unemployed. Overall, this pushes for potential benefit duration being countercyclical, with a larger variation over the cycle than implied by previous work which documented that the social benefits do not vary with economic conditions.

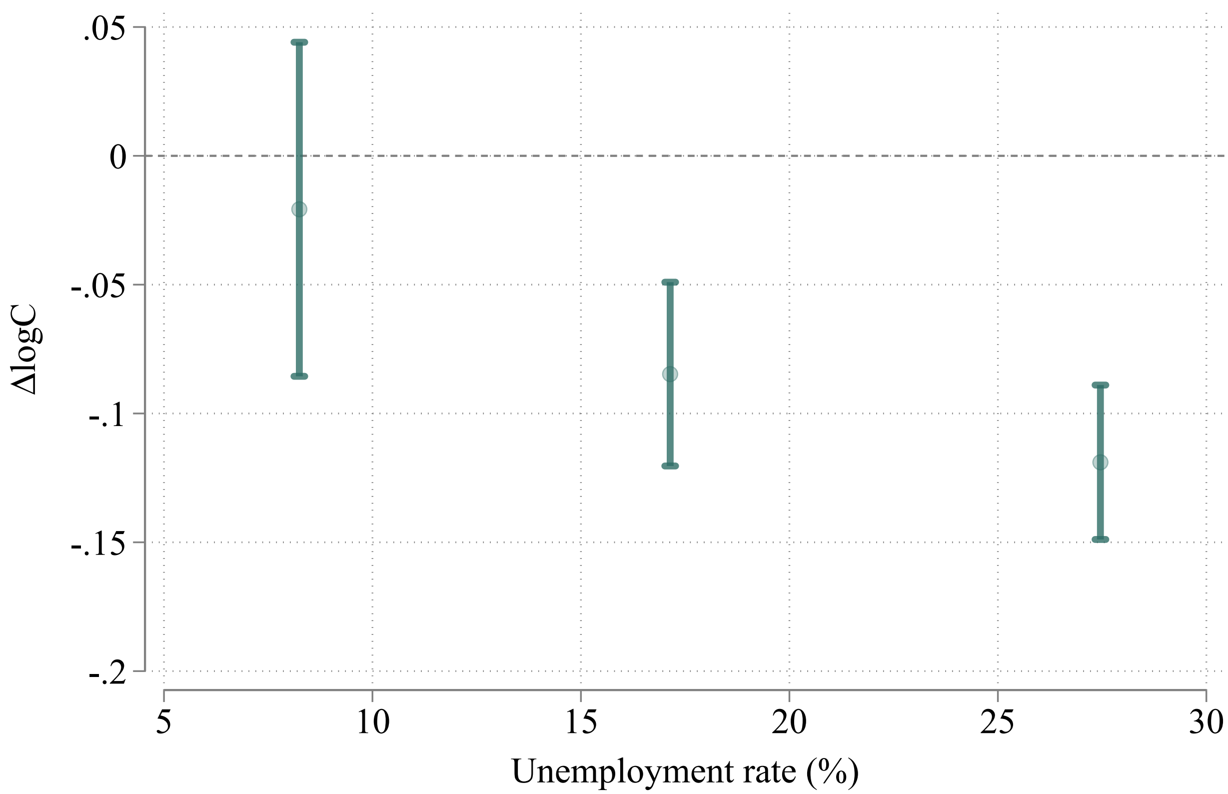
## 1.6 Conclusion

This paper provides a comprehensive assessment on whether the duration of the benefits should be higher when economic conditions are bad. I propose a simple and robust framework where the gains from allowing the UI system to vary according to the state of the economy depend on the value of insurance, preferences for redistribution over effort types, and efficiency cost. I exploit rich consumption data, administrative data on labor supply, time use data on job search and an own survey to document four facts. First, individuals reduce their consumption more when hit by unemployment in Recession. Second, the pool of unemployed shifts towards high willingness to work individuals in bad times. Third, society has a strong preference for transferring resources towards high willingness to work individuals. Fourth, the efficiency cost is large, and decreases in bad times.

Overall, I find that there are large welfare gains derived from having UI generosity varying with the unemployment rate. These gains are larger than previously found in the literature, highlighting the importance of accounting for the increasing social benefits of the transfers in Recession. Moreover, these additional gains are not only due to the standard consumption insurance channel, but also come from the important role of society's preferences for redistribution along the willingness to work dimension together with shifts in the composition of the unemployed.

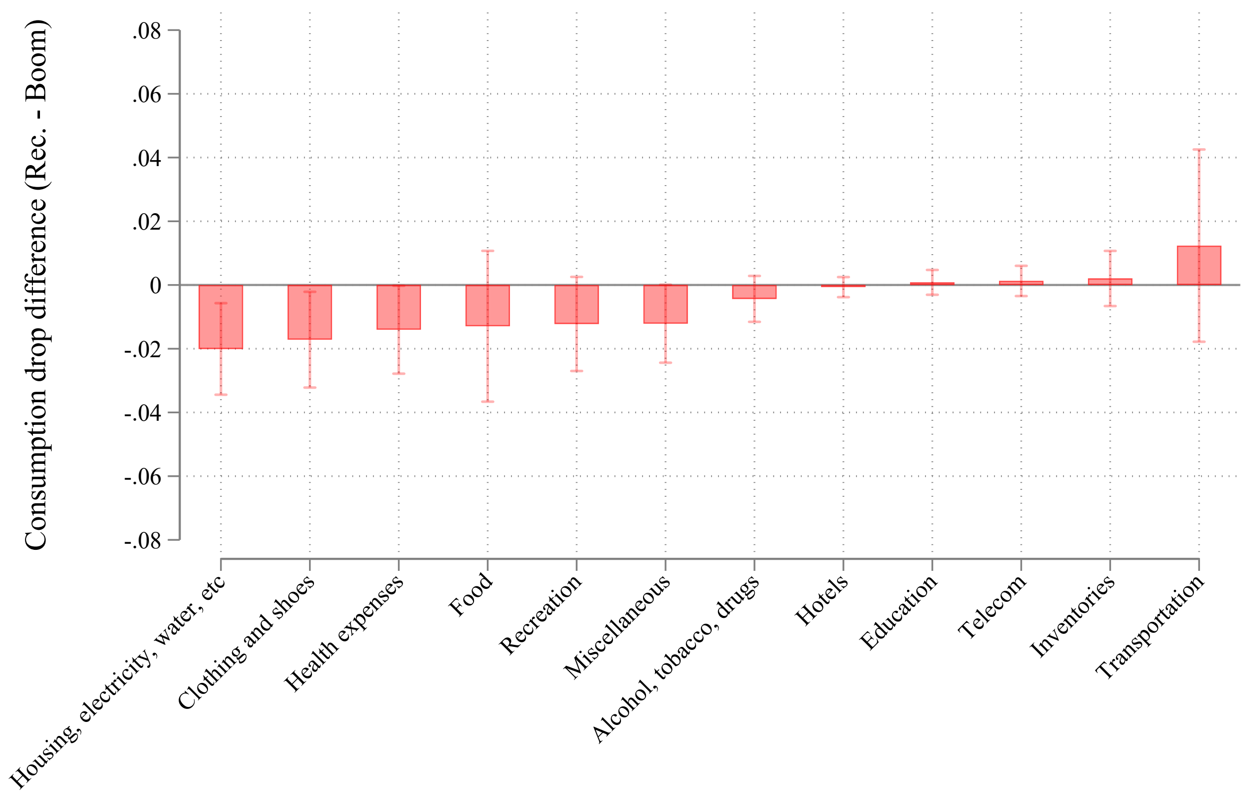
# Figures and Tables

**Figure 1.1: Consumption Smoothing Gains - Insurance**




*Note:* The graph shows how the within-individual consumption drop upon job loss varies with the unemployment rate. Specifically, the graph plots the $\alpha_j$ coefficients obtained by estimating the following regression equation:
$logC_{irt} = \sum_j \alpha_j \cdot \mathbb{1}[unemployed_{it}] \cdot \mathbb{1}[URate_{rt} = j] + \lambda_i + \lambda_r + \lambda_t + X'\beta + \varepsilon_{irt}$, where $logC_{irt}$ refers to log consumption measured at the household level, $\mathbb{1}[unemployed_{it}]$ is a dummy variable that equals one when the household head is unemployed, $\mathbb{1}[URate_{rt} = j]$ is a dummy variable that equals one when the regional unemployment rate of the household falls within unemployment rate group $j$, and $\lambda_i$, $\lambda_r$, $\lambda_t$ are individual, region and year fixed effects, respectively. The vector of controls $X$ includes dummies for the regional unemployment rate groups, and information on type of household and other household composition variables, intended to control for any mechanical relationship between household composition and economic conditions or job loss. The three unemployment rate groups approximate the national average in Boom, normal times and Recession. 95% confidence intervals from standard errors clustered at the region-year level.

**Figure 1.2: Consumption Smoothing Gains - Insurance: Evolution of Consumption Drop Over the Cycle by Category**

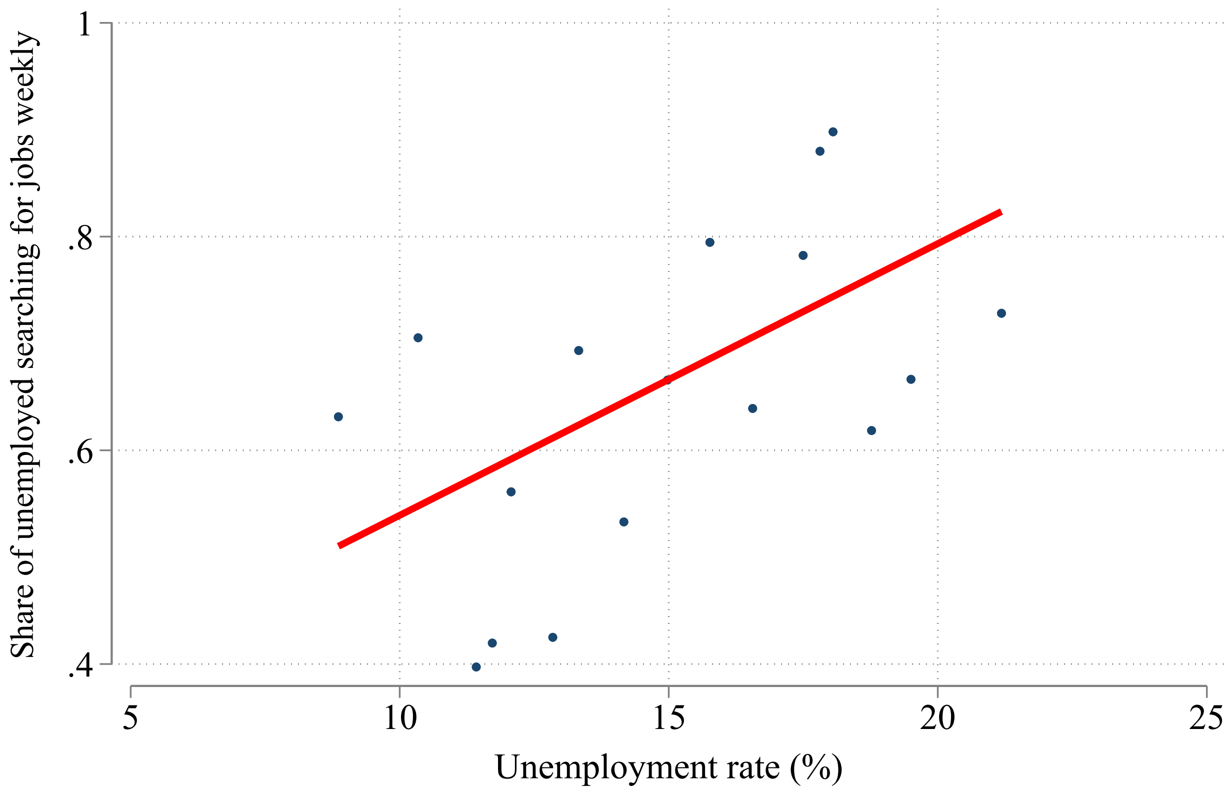



*Note:* The graph shows how the magnitude of the category-specific change in consumption upon unemployment varies with economic conditions. Specifically, I first estimate the following model $ShareC^j_{irt,0} = \beta_0 + \beta_1 Unemployed_i + \beta_2 Unemployed_i \times U.Rate_{rt} + \lambda_i + \lambda_r + \lambda_t + X'\beta + \varepsilon_{irt}$, where $ShareC^j_{irt,0}$ corresponds to the share of each consumption category $j$ relative to total consumption of the individual in the first year observed in the sample, $Unemployed_{it}$ is a dummy variable that equals one when the household head is unemployed, $U.Rate_{rt}$ refers to the unemployment rate in the region of the individual, and $\lambda_i$, $\lambda_r$, $\lambda_t$ are individual, region and year fixed effects, respectively. The vector of controls $X$ includes the regional unemployment rate, and information on type of household and other household composition variables, intended to control for any mechanical relationship between household composition and economic conditions or job loss. Then, for each category, I compute the difference between the change in Recession and the change in Boom ($\frac{\Delta^{ue,R}}{c} - \frac{\Delta^{ue,B}}{c}$). Adding up all the estimates by category yields the total change in the consumption drop shown in Table 1.1. 95% confidence intervals from standard errors clustered at the region-year level.

**Figure 1.3: Online Survey: Redistribution Over Search Effort Types**

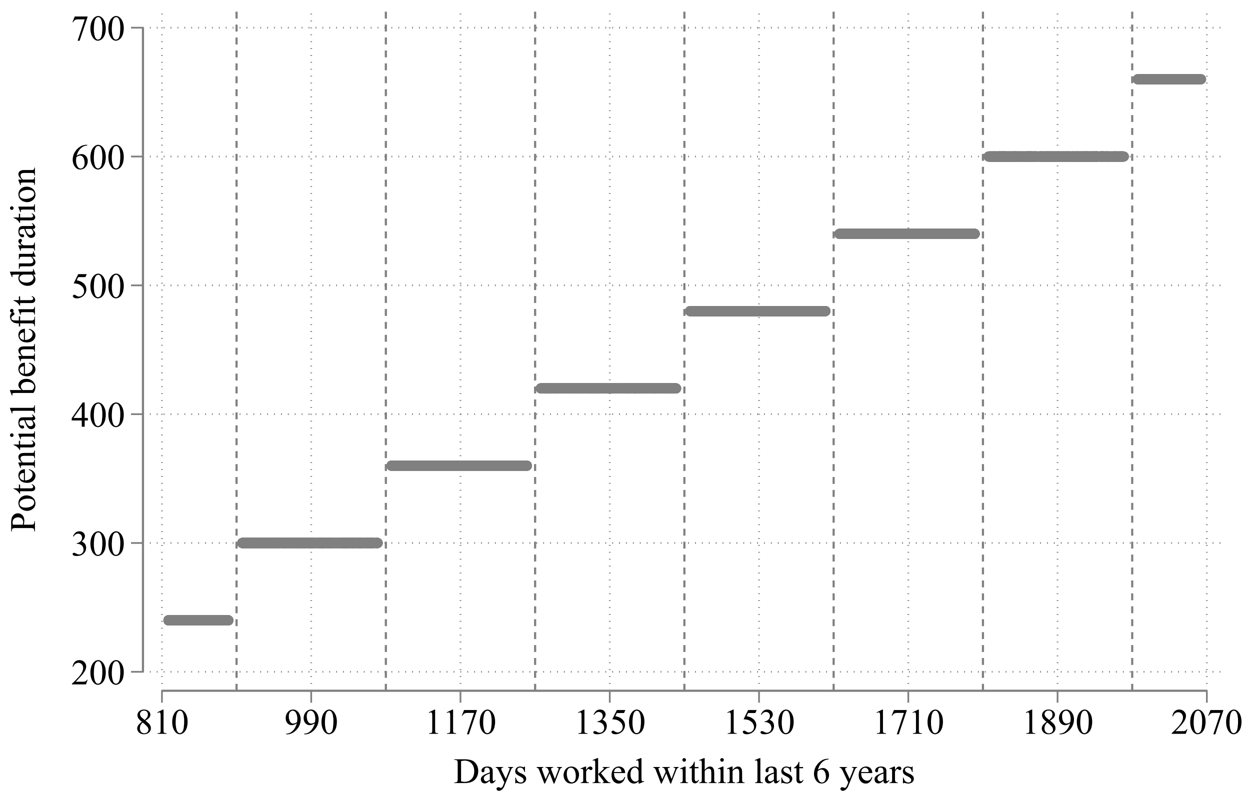

*Note:* The graphs present results from the online survey I designed. Panel (a) depicts the share of respondents choosing a given option when presented with the question on qualitative preferences for redistribution: *Imagine there are 2 unemployed individuals, A and B, who receive €950 while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed: A looked for jobs last week; B did not look for jobs last week. The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Please choose what allocation option you would prefer: More money to A than to B; More money to B than to A; Same amount to A and B.* Panel (b) depicts the share of respondents accepting an unequal allocation when presented with the question on quantitative preferences for redistribution: *Imagine there are 2 unemployed individuals, A and B, who receive €950 while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed: A looked for jobs last week; B did not look for jobs last week. The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Now imagine that an unequal allocation may be costly. That is, it can allocate €500 each, or more to one than the other, but the latter option may result in the total amount of resources that can be distributed being reduced. Please choose what allocation option you would prefer: 500 for A and 500 for B; 550 for A and 400 for B.* The offered amounts are experimentally varied as explained in Section 1.4.2, so that the implied marginal rate of transformation (MRT) or cost of the transfer varies depending on the amounts. Panel (c) depicts a wordcloud with the most common words used by respondents to describe their reasons for choosing the option in the question for Panel (a). Panel (d) shows the share of individuals stating that they would support a cycle-dependent benefit duration reform. The exact question is: *There exists the idea that benefits received by each unemployed could be more generous (higher benefit level or duration) at times when economic conditions are bad (high unemployment rate), relative to times when economic conditions are good (low unemployment rate). Some reasons for that are that when economic conditions are bad, the unemployed suffer larger consumption reductions upon job loss, and the type of unemployed at that moment are, on average, individuals who put more effort into job finding but who face more difficulties given the lack of job offers. Would you agree with having unemployment benefits changing with economic conditions in this way?*.

**Figure 1.4: Unemployed's Search Effort Over the Business Cycle**




*Note:* The graph shows how search effort by the unemployed varies with economic conditions. It is built using the 2002-2003 and 2009-2010 waves of the Spanish Time Use data, and unemployment rate from the LFS. First, data on the probability of daily search is collapsed at the region level. Second, it is converted to weekly terms following Faberman et al. (2022). Third, it is residualized on region FEs and weighted by population. More details on the methodology are presented in Appendix 1.C.

**Figure 1.5: UI Schedule**

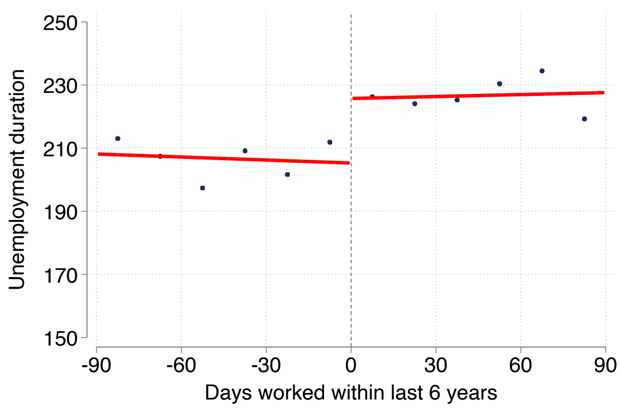



*Note:* The graph shows the Spanish Unemployment Insurance schedule. Specifically, it depicts the statutory relationship between recent work experience and potential benefit duration, measured in days.

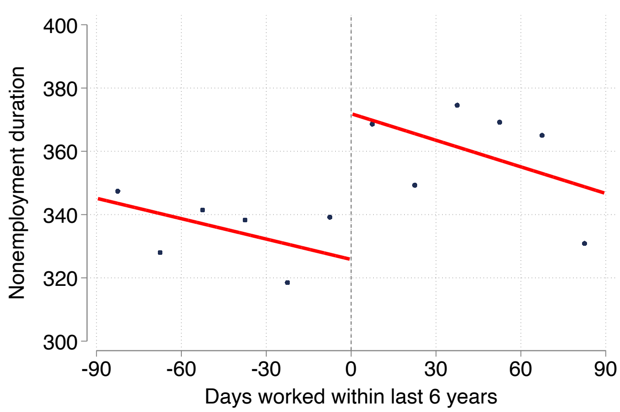
## Figure 1.6: Effect of UI Generosity on Labor Market Outcomes

### (a) Potential Benefit Duration


### (b) Unemployment Duration

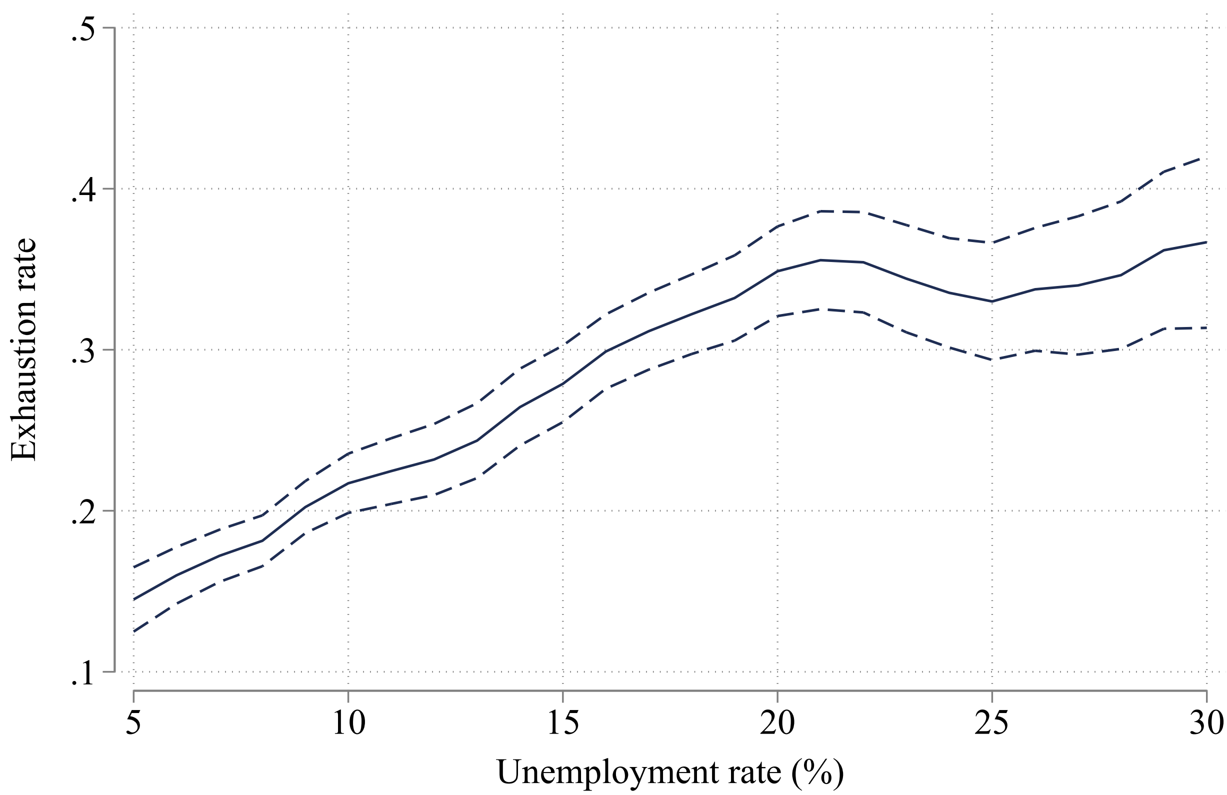



### (c) Nonemployment Duration



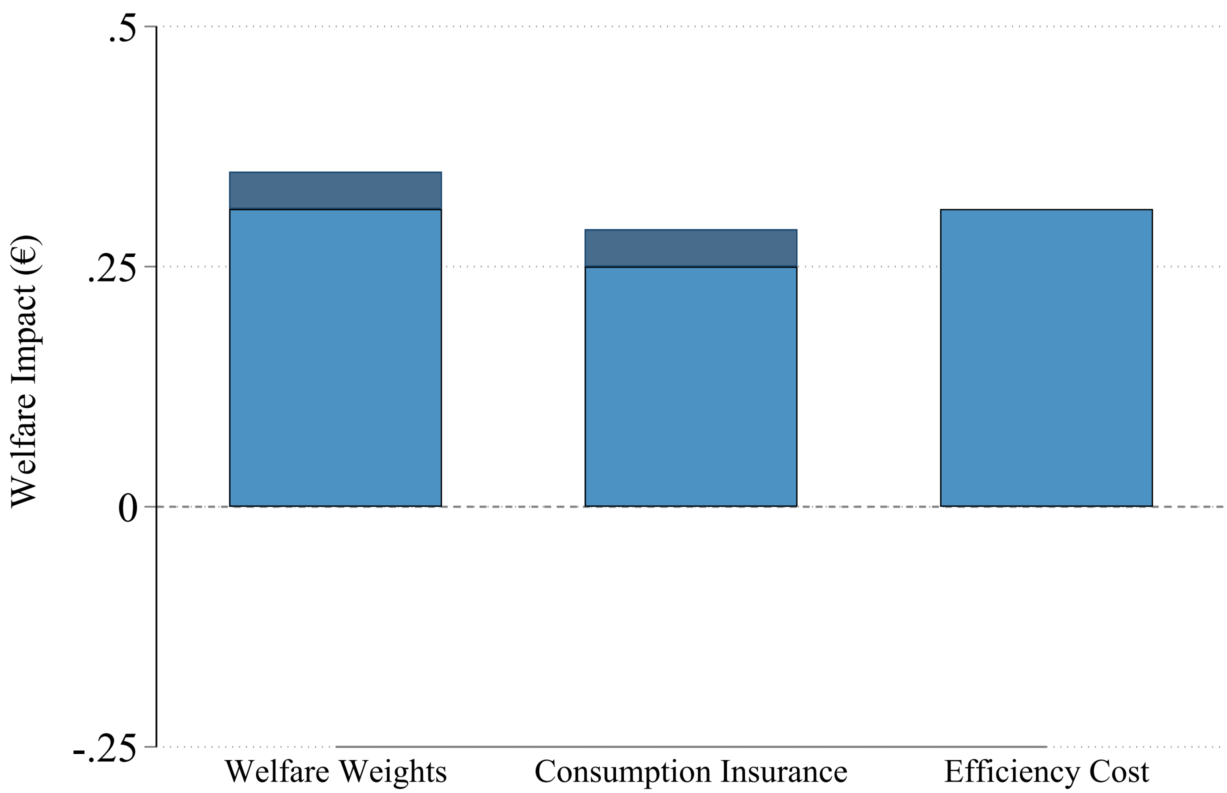

*Note:* Panels (a), (b), and (c) are binned scatter plots of PBD, unemployment and nonemployment duration, respectively, versus work experience within last 6 years, measured in days. They are depicted in days relative to the closest cutoff, effectively pooling together all cutoffs shown in Figure 1.5. Nonemployment duration is capped at 3 years.

**Figure 1.7: Exhaustion Rate**




*Note:* The graph shows how the exhaustion rate — the share of individuals remaining nonemployed beyond the maximum entitled time with paid transfers — evolves with economic conditions. It is estimated nonparametrically as described in Section 1.4.3.

**Figure 1.8: Welfare Impact**




*Note:* The graph shows the contribution of the different components — welfare weights, consumption insurance and efficiency cost — to the net welfare gain of implementing a budget-balanced cycle-dependent benefit duration reform. The contribution of each component refers to the change in the welfare gain when that component changes from no variation over the cycle to the variation estimated in the main text. It uses the estimates in the main text of the implementation equations (1.6) and (1.7), and follows the derivation in Appendix 1.B.3. The darker areas correspond to an interaction effect between the ratio of welfare weights and the consumption insurance component.

**Table 1.1: Consumption Baseline Results**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A: Estimates** | | | | | | |
| Unemployed | -0.096 | -0.006 | 0.003 | -0.095 | -0.011 | -0.001 |
| | (0.013) | (0.032) | (0.035) | (0.013) | (0.031) | (0.034) |
| Unemployed $\times$ Unemp. rate (%) | | -0.0041 | -0.0045 | | -0.0040 | -0.0044 |
| | | (0.0013) | (0.0015) | | (0.0012) | (0.0014) |
| Region FE | Yes | Yes | No | No | Yes | No |
| Year FE | Yes | Yes | No | Yes | Yes | No |
| Region $\times$ Year FEs | No | No | Yes | No | No | Yes |
| Controls | No | No | No | Yes | Yes | Yes |
| N | 63304 | 63304 | 63304 | 63304 | 63304 | 63304 |
| **Panel B: Post-estimation** | | | | | | |
| $\frac{\Delta^{ue,B}}{c}$ (Boom: Unemp. rate 8%) | | -0.039 | -0.033 | | -0.042 | -0.036 |
| | | (0.022) | (0.024) | | (0.023) | (0.025) |
| $\frac{\Delta^{ue,R}}{c}$ (Recession: Unemp. rate 26%) | | -0.112 | -0.113 | | -0.113 | -0.115 |
| | | (0.011) | (0.012) | | (0.013) | (0.014) |
| **Panel C: Statistics** | | | | | | |
| $\frac{\Delta^{ue,R}}{c} - \frac{\Delta^{ue,B}}{c}$ (Difference: Recession - Boom) | | -0.074 | -0.080 | | -0.071 | -0.079 |
| | | (0.023) | (0.026) | | (0.021) | (0.025) |

*Note*: The table shows how the consumption change upon unemployment evolves over the business cycle. Results are obtained from a regression with specification as in equation (1.8). Columns (1)-(3) include controls for regional unemployment rate and type of household. Columns (4)-(6) include controls for regional unemployment rate, type of household and the following set of covariates: age, sex, education, civil status, (log) number of individuals in household, number of household members working, number of household members unemployed, number of kids, rural status and house ownership. Standard errors are clustered at the region-year level and reported in parentheses.

**Table 1.2: Marginal Cost Baseline Results**

| | Pot. benefit duration | | Unemp. duration | | Nonemp. duration | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\mathbb{1}[Exp. > 0]$ | 60.33 | 55.50 | 25.05 | 29.80 | 43.30 | 69.34 |
| | (5.44) | (7.06) | (7.72) | (10.45) | (15.56) | (22.57) |
| $\mathbb{1}[Exp. > 0]$ x Unemp. rate (%) | | 0.33 | | -0.32 | | -1.76 |
| | | (0.34) | | (0.49) | | (1.16) |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 6451 | 6451 | 6451 | 6451 | 6451 | 6451 |

*Note*: The table shows the marginal effect on potential benefit duration, unemployment duration and nonemployment duration. It also explores whether these relationships vary with regional unemployment rate. They are estimated following equation (1.12). Standard errors are clustered at the region level and reported in parentheses.

**Table 1.3: Marginal Benefit Calibration**

| | $\gamma = 1$ | $\gamma = 2$ | $\gamma = 3$ | $\gamma = 4$ | $\gamma = 5$ |
|---|---|---|---|---|---|
| $\frac{\omega^R}{\omega^B}$=1 | 1.07 | 1.14 | 1.20 | 1.25 | 1.31 |
| $\frac{\omega^R}{\omega^B}$=1.13 | 1.21 | 1.28 | 1.35 | 1.42 | 1.48 |
| $\frac{\omega^R}{\omega^B}$=1.15 | 1.23 | 1.31 | 1.38 | 1.44 | 1.50 |
| $\frac{\omega^R}{\omega^B}$=1.31 | 1.40 | 1.49 | 1.57 | 1.64 | 1.71 |
| $\frac{\omega^R}{\omega^B}$=1.5 | 1.61 | 1.70 | 1.79 | 1.88 | 1.96 |

*Note*: The table combines results on consumption smoothing gains and welfare weights estimated in previous sections to produce estimates of the marginal benefit term, $\frac{E^{u,R}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]}{E^{u,B}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]}$, under different implementation assumptions following the consumption-based approach in equation (1.6). The rows show results under different ratios of welfare weights, where I use the benchmark estimate for relative valuation across types obtained in Section 1.4.2, and vary the magnitude of search effort change over the business cycle that is due to a composition effect. Row (1) refers to the case where there is no composition change. Column (2) refers to the case where only composition changes due to observables as measured by the set of demographics in the Spanish Time Use data and LFS, as obtained from the prediction exercise in Appendix 1.C. Column (3) refers to half of the composition change found by (Mukoyama et al., 2018) in the US using CPS data, while column (4) refers to the same composition change found by them. Column (5) refers to the case where all the change in effort is assumed to be due to composition effects. Columns (1)-(5) explore sensitivity of the results with respect to the parameter of relative risk aversion, $\gamma$.

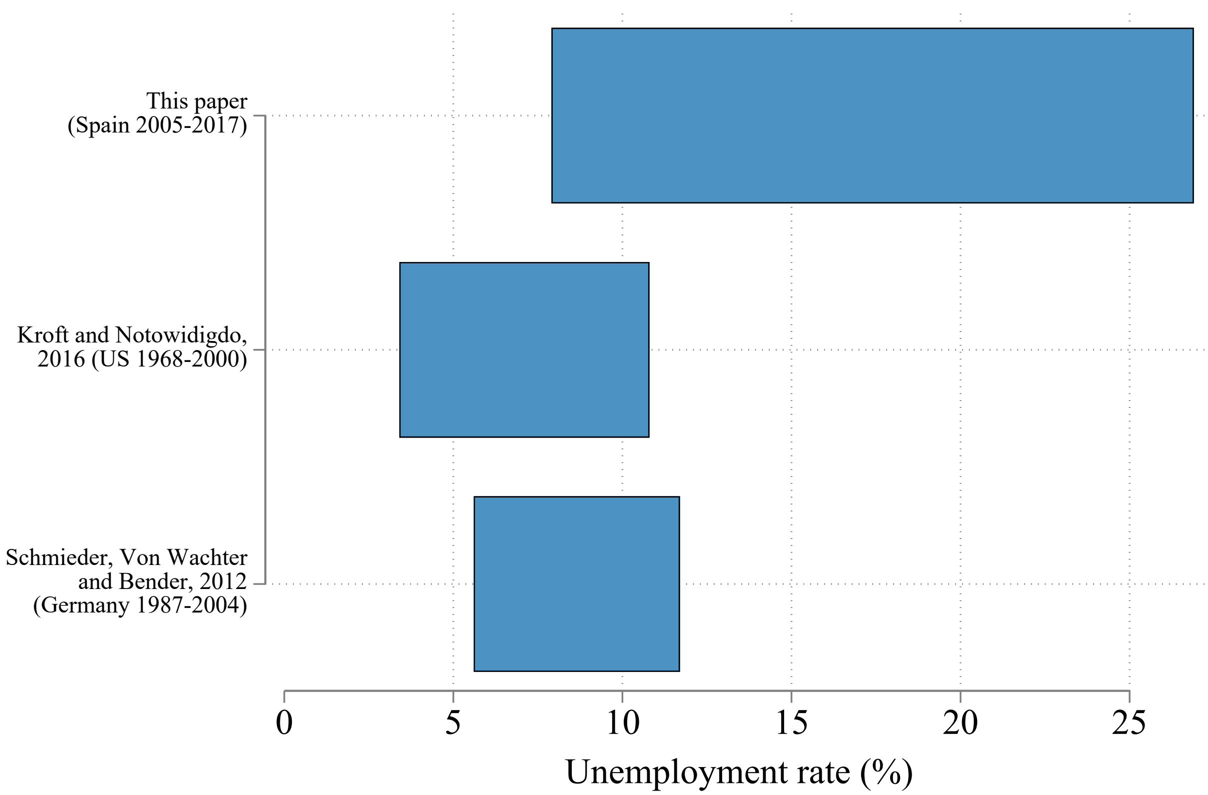
**Table 1.4: Marginal Cost Calibration**

| | $\frac{dD_b}{db_{P+1}}$ | $\frac{dD}{db_{P+1}}$ | $b$ | $\tau$ | $S_{P+1}$ | $FE$ |
|---|---|---|---|---|---|---|
| Boom (8% unemp. rate) | 0.015 | 0.025 | 850 | 53 | 0.19 | 2.42 |
| Mean (15% unemp. rate) | 0.015 | 0.025 | 850 | 53 | 0.26 | 1.77 |
| Recession (26% unemp. rate) | 0.015 | 0.025 | 850 | 53 | 0.34 | 1.35 |

*Note*: The table quantifies how the efficiency cost varies with economic conditions. It shows the estimates for the different components in equation (1.7), and how they change when the economy goes from boom to recession, with the magnitudes reflecting the national unemployment rate at different points of the business cycle. Effects of a €1 equivalent increase in potential benefit duration on unemployment duration $\frac{dD_b}{db_{P+1}}$ and nonemployment duration $\frac{dD}{db_{P+1}}$ are measured in days. Average monthly benefit level is $b = 850$, and the payroll tax is $\tau = 53$. The tax is obtained as the amount consistent with the stated benefit level and the ratio of UI recipients to employed individuals over the period 2007-2017, following Landais (2015). Number of UI recipients is obtained from OECD, and number of employed from EPA (INE).

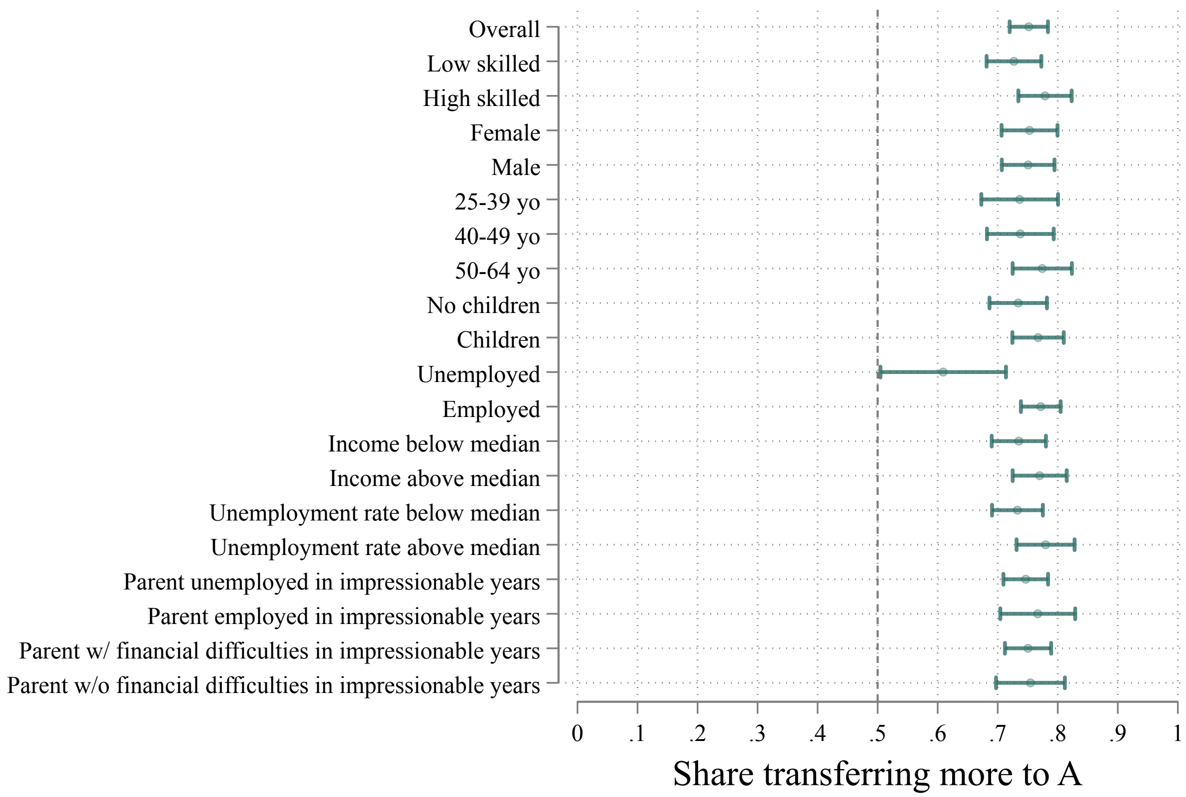
## 1.A Additional Figures and Tables

**Figure 1.A1: Spanish Unemployment Fluctuations in the Context of Recent Literature**



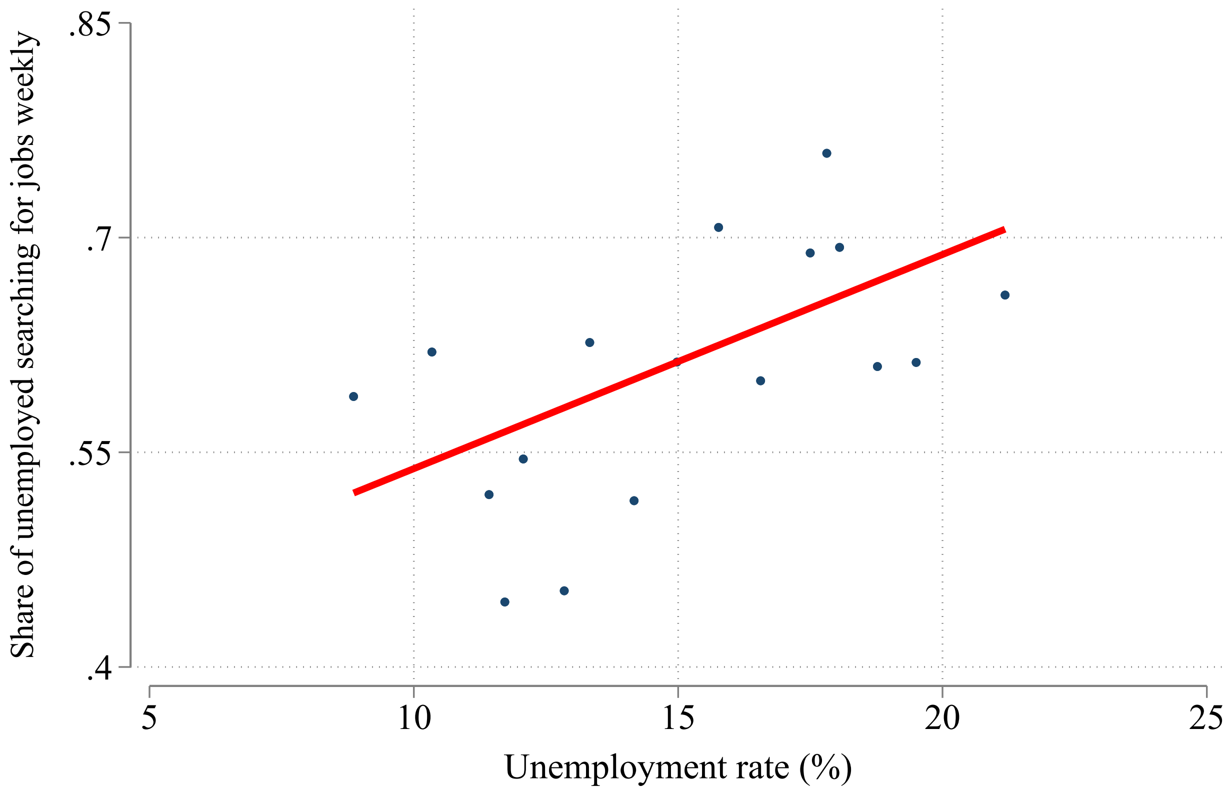

*Note:* This graph presents the magnitude of the Spanish unemployment fluctuations in relation to two prominent papers in the literature. Those papers use data from US (Kroft and Notowidigdo, 2016) and Germany (Schmieder et al., 2012). Blue bars correspond to the range of national unemployment rate in their country and period of study.

**Figure 1.A2: Preferences for Redistribution: Heterogeneity**



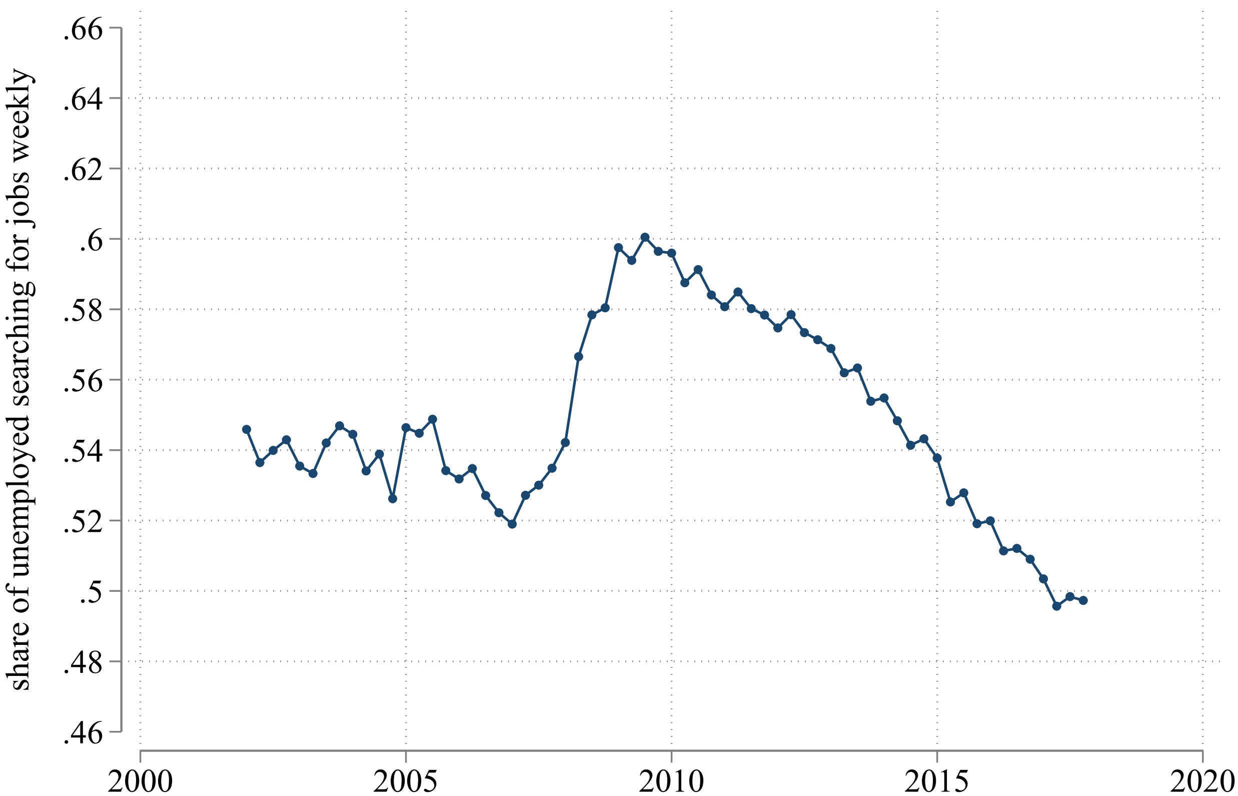

*Note:* The graph explores heterogeneity in preferences for redistribution between the different types of unemployed. It focuses on the qualitative question: *Imagine there are 2 unemployed individuals, A and B, who receive €950 while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed: A looked for jobs last week; B did not look for jobs last week. The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Please choose what allocation option you would prefer: More money to A than to B; More money to B than to A; Same amount to A and B.*, as described in Section 1.4.2. I summarize the answers in a dummy variable that equals one for individuals choosing to allocate more resources to the individual who searched for jobs last week, and zero otherwise. The plot depicts the average value for that variable for the different demographic characteristics elicited in the Preliminary Questions and Demographic blocks in the online survey, as shown in Appendix 1.E. Impressionable years refer to 18-25 (Krosnick and Alwin, 1989). Point estimates are shown along 95% confidence intervals.

**Figure 1.A3: Unemployed's Search Effort Over the Business Cycle**




*Note:* The graph shows how search effort by the unemployed varies with economic conditions. It is built using the 2002-2003 and 2009-2010 waves of the Spanish Time Use data, and unemployment rate from the LFS. First, data on the probability of daily search is collapsed at the region level. Second, it is converted to weekly terms assuming search is independent across days. Third, it is residualized on region FEs, and weighted by population. More details on the methodology are presented in Appendix 1.C.

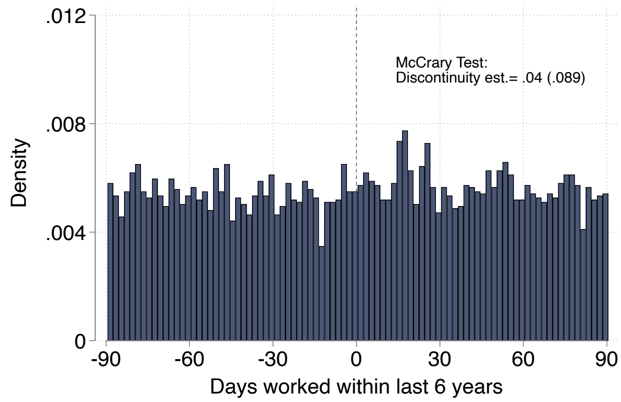
**Figure 1.A4: Unemployed's Search Effort Over the Business Cycle: Composition Effect Based on Observables**




*Note:* The graph shows the component of search effort that varies over the business cycle due to composition shift in observable characteristics. It is built first estimating a model that relates the probability of daily search with a set of demographic characteristics in the Time Use data. These variables are: interactions between age categories and sex; interactions between education categories and sex; interaction between married status and sex; and unemployment rate, day of the week when diary was filled and region FEs. Then, I predict daily search based on that model using the same set of demographic characteristics into the Labor Force Survey. Finally, I convert to weekly terms following Faberman et al. (2022). The depicted change in search effort is therefore only due to a composition effect based on observables, and not driven by changes in search effort behavior within individual in response to economic conditions.

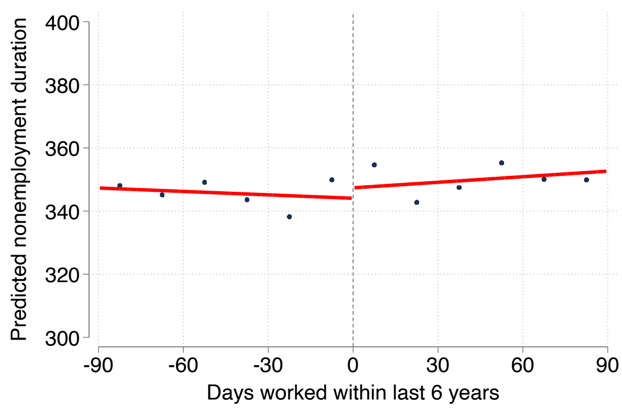
## Figure 1.A5: Validity of the Design

### (a) Density




### (b) Unemployment Duration


### (c) Nonemployment Duration

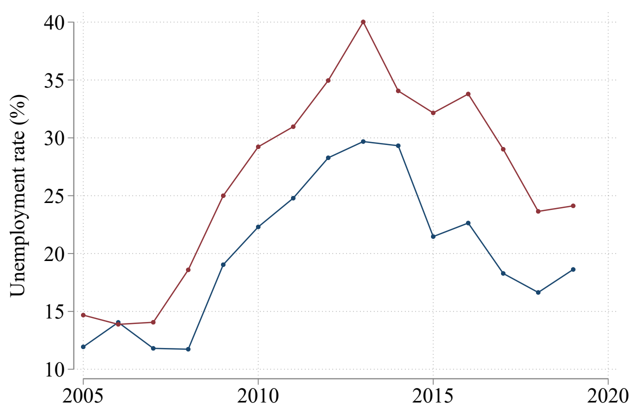



*Note:* Panel (a) follows McCrary (2008) to assess whether the density of the running variable presents any discontinuity at the normalized cutoff. Panels (b) and (c) plot the evolution of predicted unemployment and nonemployment duration around the normalized cutoff. This covariate index is constructed using a linear model of the covariates in the benchmark specification in the spirit of Card et al. (2015).

## Figure 1.A6: Support for Increase in UI Generosity - CIS Survey

### (a) Support by Demographic Groups


### (b) Unemployment Rate by Shock Size




### (c) Support by Shock Size


*Note:* The plots are obtained using data from the Spanish CIS survey on public finance issues over the years 2005-2019. Panel (a) shows, on the left axis, the evolution of the share of people supporting increases in UI generosity over the business cycle for different groups of individuals. I recode the answers to the question into a dummy variable that equals one if the respondent would like a more generous UI system, and zero otherwise. National unemployment rate is shown on the right axis. Panel (b) shows the unemployment rate for two groups of regions according to the intensity of the Great Recession shock. Specifically, I compute for each region the change in unemployment rate between 2007 and 2013, and create two groups containing regions below and above the median change in the sample. Panel (c) shows the evolution of the demand for UI generosity increase split by the same groups as in Panel (b).

**Table 1.A1: Consumption Robustness Results**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Unemployed | -0.006 | 0.749 | 0.004 | -0.004 | -0.011 | 0.845 | -0.008 | -0.008 |
| | (0.031) | (0.801) | (0.059) | (0.030) | (0.031) | (0.783) | (0.058) | (0.030) |
| Unemployed $\times$ Unemp. rate (%) | -0.0041 | -0.0038 | -0.0039 | -0.0043 | -0.0040 | -0.0038 | -0.0039 | -0.0043 |
| | (0.0013) | (0.0016) | (0.0015) | (0.0013) | (0.0012) | (0.0015) | (0.0014) | (0.0012) |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Unemployment duration | No | Yes | No | No | No | Yes | No | No |
| Share unemp. 6-12 months | No | No | Yes | No | No | No | Yes | No |
| $\mathbb{1}[\text{year} \geq 2012]$ | No | No | No | Yes | No | No | No | Yes |
| Controls | No | No | No | No | Yes | Yes | Yes | Yes |
| N | 63304 | 63304 | 63304 | 63304 | 63304 | 63304 | 63304 | 63304 |

*Note*: The table explores sensitivity for the evolution of consumption changes upon unemployment over the business cycle. Columns (1) and (5) are the same ones as columns (2) and (5) in Table 1.1, which report results with and without controls for the benchmark specification, respectively. Column (2) interacts unemployed's consumption with a first and second order polynomial in unemployment duration, for the unemployed with duration up to one year. Column (3) interacts unemployed's consumption with the share of unemployed up to one year that have a duration between 6-12 months. Column (4) interacts unemployed's consumption with a dummy that equals one for the period from 2012 onwards, which corresponds to the period where the benefit level after 6 months was slightly decreased. Columns (6), (7) and (8) repeat the same specifications before and also include controls for type of household and the following set of covariates: age, sex, education, civil status, (log) number of individuals in household, number of household members working, number of household members unemployed, number of kids, rural status and house ownership. Standard errors are clustered at the region-year level and reported in parentheses.

**Table 1.A2: First-Year Consumption**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Unemployed (t+1) | -0.253 | -0.252 | -0.255 | -0.163 | -0.154 | -0.157 |
| | (0.0186) | (0.0455) | (0.0459) | (0.0171) | (0.0431) | (0.0437) |
| Unemployed (t+1) × Unemp. rate (%) | | -0.00005 | 0.00020 | | -0.00045 | -0.00025 |
| | | (0.00239) | (0.00241) | | (0.00226) | (0.00229) |
| Region FE | Yes | Yes | No | No | Yes | No |
| Year FE | Yes | Yes | No | Yes | Yes | No |
| Region × Year FEs | No | No | Yes | No | No | Yes |
| Controls | No | No | No | Yes | Yes | Yes |
| N | 31652 | 31652 | 31652 | 31652 | 31652 | 31652 |

*Note*: The table shows consumption how consumption evolves over the business cycle during the first year individuals are observed in the sample. Specifically, it compares first-year (t) consumption patterns (while employed) for individuals that get unemployed in the following period (t+1) relative to those that remain employed. Results are obtained from a regression similar to specification as in equation (1.8). However, since here I only look at individuals' first year in the sample, these specifications do not include individual FEs. Columns (1)-(3) include controls for type of household. Columns (4)-(6) include controls for type of household and the following set of covariates: age, sex, education, civil status, (log) number of individuals in household, number of household members working, number of household members unemployed, number of kids, rural status and house ownership. Standard errors are clustered at the region-year level and reported in parentheses.

**Table 1.A3: Unemployed's Search Effort Over the Business Cycle - Search Methods Across Countries**

| | All | | Ask friends | | Study job ads | | Apply to employers | | Contact public service | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Unemp. rate (%) | 0.423 | 0.308 | 1.016 | 0.833 | 0.704 | 0.495 | 1.063 | 0.793 | 0.949 | 0.795 |
| | (0.0770) | (0.0761) | (0.227) | (0.235) | (0.344) | (0.473) | (0.246) | (0.245) | (0.599) | (0.547) |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Mean | 32.9 | 32.9 | 65 | 65 | 58.7 | 58.7 | 54.4 | 54.4 | 51 | 51 |
| Countries | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 |
| N | 4868 | 4868 | 497 | 497 | 496 | 496 | 497 | 497 | 497 | 497 |

| | Answer job ads | | Contact priv. agency | | Other | | Look for land | | Look for licenses | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) |
| Unemp. rate (%) | 0.737 | 0.425 | 0.441 | 0.473 | -0.615 | -0.919 | -0.027 | 0.005 | -0.042 | -0.007 |
| | (0.143) | (0.174) | (0.196) | (0.316) | (0.707) | (0.519) | (0.021) | (0.054) | (0.027) | (0.042) |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Mean | 41.5 | 41.5 | 20.5 | 20.5 | 14 | 14 | 1.8 | 1.8 | 1.7 | 1.7 |
| Countries | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 |
| N | 496 | 496 | 496 | 496 | 438 | 438 | 492 | 492 | 492 | 492 |

*Note*: The table presents the relationship between the unemployment rate (%) and the percentage of unemployed that declared using each of the different search methods over the last 4 weeks. The data spans the years 2005-2019 and 34 countries. The data are obtained from Eurostat, which collects it from national Labor Force Surveys. The annual unemployment rate and population are also from Eurostat. Columns (1)-(2) pool all search methods. Columns (3)-(20) represent different search methods used to seek work ordered in the table according to usage, which include: contacting private employment agencies; applying to employers directly; asking friends, relatives, and trade unions; publishing or answering job advertisements; studying job advertisements; taking tests, interviews, or examinations; looking for land, premises, or equipment; looking for licenses, permits, or financial resources; and other methods. The countries included in the sample are Austria, Belgium, Bulgaria, Switzerland, Cyprus, Czechia, Germany, Denmark, Estonia, Spain, Finland, France, Croatia, Hungary, Ireland, Iceland, Italy, Lithuania, Luxembourg, Latvia, Montenegro, North Macedonia, Malta, Netherlands, Norway, Poland, Portugal, Romania, Serbia, Sweden, Slovenia, Slovakia, Turkey and United Kingdom. For each method, regression results are presented from specifications with country FE and with/without year FE, along with the mean value of the dependent variable, the number of countries, and the number of country-year-method observations. Regressions are weighted by country population and standard errors, in parentheses, are clustered at the country level.

**Table 1.A4: Marginal Cost Sample Summary Statistics**

| | Mean | Std. Dev. |
|---|---|---|
| Potential benefit duration | 445.61 | 129.74 |
| Nonemployment duration | 347.88 | 358.12 |
| Days worked last 6 years | 1424.46 | 380.78 |
| Unemployment rate | 14.83 | 7.54 |
| Previous wage | 1538.86 | 723.44 |
| Lifetime experience | 2703.63 | 1504.30 |
| Age | 33.12 | 5.87 |
| Full-time | 0.97 | 0.12 |
| Rural | 0.40 | 0.49 |
| Education: | | |
| Primary | 0.57 | 0.50 |
| High School | 0.27 | 0.44 |
| University | 0.17 | 0.37 |
| Occupation: | | |
| Low | 0.19 | 0.39 |
| Lower medium | 0.55 | 0.50 |
| Upper medium | 0.15 | 0.35 |
| High | 0.11 | 0.32 |
| N | 6451 | |

*Note:* The table shows the mean and standard deviation of the variables from the final sample used to estimate the statistics in the efficiency cost term. Nonemployment duration refers to the number of days between start of UI reception and beginning of a new employment spell and is capped at 3 years. Previous wage corresponds to the average nominal wage over the last 6 months before becoming unemployed and is expressed in euros. Lifetime experience is measured in days and age in years. Full-time is a continuous variable regarding the number of hours worked where the value of one corresponds to full-time job (e.g., 0.5 is part-time). Rural is a dummy variable which equals one if the previous job was located in a municipality with less than 40000 inhabitants.

## 1.B Derivations

### 1.B.1 Model

I follow closely Chetty (2006) and Kolsrud et al. (2018), but consider differentiation of the unemployment benefit policy according to economic conditions and a social planner with fairness considerations based on willingness to work. Let $\xi_{i,t}$ refer to a vector of state variables containing all relevant information up to time $t$, which determines individual's employment status, aggregate economic conditions $k$ and behavior at time $t$. $F_{i,t}(\xi_{i,t})$ denotes the unconditional distribution of $\xi_{i,t}$ given information available at time $t$. I assume $F_{i,t}$ is a cdf function and $\Omega$ denotes the maximal support of $F_{i,t}$ for $\forall i, t$. Aggregate economic conditions are denoted by $k \in \{B, R\}$.

Individuals choose each period how much to consume from income and assets, and how much effort to exert if unemployed. An employed individual earns $w_i - \tau$, and when unemployed receives $b_t^k + y_u$. The law of motion of assets when employed is $a_{i,t+1} = (1+r)a_{i,t} + w_i - \tau - c_{i,t}^e$; and when unemployed is $a_{i,t+1} = (1+r)a_{i,t} + b_t^k + y_u - c_{i,t}^u$, with $a_{i,t+1} \geq \bar{a}_i$. The Lagrange multipliers on these constraints are $\mu_{i,t}^{e,k}(\xi_{i,t})$, $\mu_{i,t}^{u,k}(\xi_{i,t})$ and $\mu_{i,t}^{a}(\xi_{i,t})$, respectively.

Let $\theta_{i,t}^{s,k}(\xi_{i,t})$ denote an individual's employment status $s$ at time $t$ in aggregate economic conditions $k$. When $\theta^{e,k} = 1$, the individual is employed in aggregate economic conditions $k$. When $\theta^{u,k} = 1$, the individual is unemployed in aggregate economic conditions $k$. They equal zero otherwise. Each individual is in one of four mutually exclusive labor market status, denoted by the combination of employment status and aggregate conditions. In each period $t$, an unemployed individual chooses a level of search effort $e_{i,t}$ as well. This search effort level determines the probability to leave unemployment for employment, with the mapping potentially depending on economic conditions.

Each individual $i$ chooses a program $(e_i, c_i^u, c_i^e)$ where

$$e_i = \{e_{i,t}(\xi_{i,t})\}_{t\in\{1,2..T\},\xi_{i,t}\in\Omega,\theta^{u,k}(\xi_{i,t})=1},$$

$$c_i^u = \{c_{i,t}^u(\xi_{i,t})\}_{t\in\{1,2..T\},\xi_{i,t}\in\Omega,\theta^{u,k}(\xi_{i,t})=1},$$

$$c_i^e = \{c_{i,t}^e(\xi_{i,t})\}_{t\in\{1,2..T\},\xi_{i,t}\in\Omega,\theta^{e,k}(\xi_{i,t})=1},$$

to solve

$$
\begin{aligned}
V_{i,0}(P) = \max \Sigma_{t=1}^{T}\beta^{t-1} & \int \{\Sigma_k[u_i\left(c^u_{i,t}(\xi_{i,t}), e_{i,t}(\xi_{i,t})\right)\theta^{u,k}_{i,t}\left(\xi_{i,t}\right) + v_i\left(c^e_{i,t}\right)\theta^{e,k}_{i,t}\left(\xi_{i,t}\right)]\}dF_{i,t}\left(\xi_{i,t}\right) \\
& + \Sigma_{t=1}^{T}\beta^{t-1}\int\{\Sigma_k\mu^{u,k}_{i,t}\left(\xi_{i,t}\right)[(1+r)a_{i,t-1}(\tilde{\xi}_{i,t-1}) + b^k_t - c^u_{i,t}\left(\xi_{i,t}\right) - a_{i,t}]\theta^{u,k}_{i,t}\left(\xi_{i,t}\right)\}dF_{i,t}\left(\xi_{i,t}\right) \\
& + \Sigma_{t=1}^{T}\beta^{t-1}\int\{\Sigma_k\mu^{e,k}_{i,t}\left(\xi_{i,t}\right)[(1+r)a_{i,t-1}(\tilde{\xi}_{i,t-1}) + w_i - \tau - c^e_{i,t}\left(\xi_{i,t}\right) - a_{i,t}]\theta^{e,k}_{i,t}\left(\xi_{i,t}\right)\}dF_{i,t}\left(\xi_{i,t}\right) \\
& + \Sigma_{t=1}^{T}\beta^{t-1}\int\mu^{a}_{i,t}\left(\xi_{i,t}\right)[\overline{a}_i - a_{i,t+1}]dF_{i,t}\left(\xi_{i,t}\right),
\end{aligned}
$$

where the short-hand notation $\tilde{\xi}_{i,t-1}$ denotes the vector of state variables at time $t-1$ that preceded the vector of state variables $\xi_{i,t}$ at time $t$. While in the empirical implementation in the main text I consider separability of utility between consumption and effort, here I allow for non-separability in the characterization. In the framework, the individual starts unemployed and remains active until $T$, with employment being an absorbing state. The individual's exit rate out of unemployment in aggregate economic conditions $k$ at time $t$ depends on her search effort at time $t$. The (unconditional) probability to be unemployed at time $t+1$ in aggregate economic conditions $k$ is

$$
\Pr(\theta^{u,k}_{i,t+1} = 1) = \int[1 - h^k(e_{i,t}(\xi_{i,t}))]\theta^{u,k}_{i,t}(\xi_{i,t})dF_{i,t}(\xi_{i,t}).
$$

The present value of the government's budget is

$$
G(P) = \Sigma_{t=1}^{T}\frac{1}{(1+r)^{t-1}}\int\int\{\Sigma_k[-b^k_t\theta^{u,k}_{i,t}(\xi_{i,t}) + \tau\theta^{e,k}_{i,t}(\xi_{i,t})]\}dF_{i,t}(\xi_{i,t})di.
$$

The government solves

$$
\max\int\omega(\overline{e}_i)V_{i,0}(P)di + \lambda[G(P) - \overline{G}],
$$

where $\omega(.)$ is a welfare weight, $\lambda$ is the Lagrange multiplier on the government's budget constraint and $\overline{G}$ is an exogenous revenue constraint. The welfare weight is assumed to depend on willingness to work $\overline{e}_i$, which is defined as the effort level exerted by individuals when faced with aggregate economic conditions as in Boom period and a fixed benefit policy:

$$
\overline{e}_i = E[e_{i,t}|\theta^{u,B}_{i,t} = 1, P],
$$

The characterization is based on local policy changes and thus only allows for local tests and recommendations. For the ability to translate globally, the program would need to be strictly concave in $P$. To provide tractable expressions of local welfare implications I assume that the social welfare function is differentiable.

**Unemployment Policy** Following Chetty (2006), I assume that lifetime utility is smooth, increasing and strictly quasi-concave in $(c_i^u, c_i^e, e_i)$ and that the value function $V_{i,0}(P)$ is differentiable such that the envelope theorem applies.

The welfare impact of a change in the unemployment benefit $b_{P+1}^k$ equals

$$\frac{\partial W(P)}{\partial b_{P+1}^k} = \int \omega(\bar{e}_i)\frac{\partial V_{i,0}(P)}{\partial b_{P+1}^k}di + \lambda\frac{\partial G(P)}{\partial b_{P+1}^k},$$

where, using $S_t^{r,k} = S_t^k/[1+r]^{(t-1)}$,

$$\begin{aligned}\frac{\partial G(P)}{\partial b_{P+1}^k} &= -S_{P+1}^{r,k} - \Sigma_{t'=1}^T(b_{t'}^k+\tau)\frac{\partial S_{t'}^{r,k}}{\partial b_{P+1}^k} - \Sigma_{t'=1}^T(b_{t'}^{k'}+\tau)\frac{\partial S_{t'}^{r,k'}}{\partial b_{P+1}^k}\\ &\approx -S_{P+1}^{r,k} - \Sigma_{t'=1}^T(b_{t'}^k+\tau)\frac{\partial S_{t'}^{r,k}}{\partial b_{P+1}^k}\\ &= -S_{P+1}^k - \frac{\partial D_b^k}{\partial b_{P+1}^k}b - \frac{\partial D^k}{\partial b_{P+1}^k}\tau\end{aligned}$$

where the second line approximation relies on the effect of extending benefit duration in a given economic condition having small effect of individuals' behavior while unemployed in other economic conditions, and the last line follows from the simplification $r = 0$ and a constant benefit policy paying $b$ up to period $P$ and zero afterwards as in the main text, and

$$\begin{aligned}\int \omega(\bar{e}_i)\frac{\partial V_{i,0}(P)}{\partial b_{P+1}^k}di &= \beta^P \int\int \omega(\bar{e}_i)\frac{\partial u_i^u(c_{i,P+1}^u(\xi_{i,P+1}), e_{i,P+1}(\xi_{i,P+1}))}{\partial c_{i,P+1}^u}\theta_{i,P+1}^{u,k}(\xi_{i,P+1})dF_{i,P+1}(\xi_{i,P+1})di\\ &= S_{P+1}^k E\left[\omega(\bar{e}_i)\frac{\partial u_i^u(c_{i,t}^u(\xi_{i,t}), e_{i,t}(\xi_{i,t}))}{\partial c_{i,t}^u}\middle| t = P+1, \theta_{i,t}^{u,k} = 1\right].\end{aligned}$$

Where the last line follows from the simplification $\beta = 1 + r = 1$. The expectation operator thus averages over all potential states in which the individual is unemployed at time $P + 1$ in aggregate conditions $k$. The weight of individual $i$'s marginal utility in calculating the average marginal utility among the unemployed at time t is scaled by $S_{i,t}^k/S_t^k$.

Combining the two expressions, I find

$$\frac{\partial W(P)}{\partial b_{P+1}^k} = 0 \Leftrightarrow E_{P+1}^{u,k}\left[\omega(\bar{e}_i)\frac{\partial u_i^u(c_{i,t}^u(\xi_{i,t}), e_{i,t}(\xi_{i,t}))}{\partial c_{i,t}^u}\right] = \lambda\left[1 + \frac{1}{S_{P+1}^k}\left[\frac{\partial D_b^k}{\partial b_{P+1}^k}b + \frac{\partial D^k}{\partial b_{P+1}^k}\tau\right]\right].$$

Thus, extending the unemployment benefit in both Boom and Recession, and combining expressions, I obtain:

$$\frac{E_{P+1}^{u,R}\left[\omega(\bar{e}_i)\frac{\partial u_i^u(c_{i,t}^u(\xi_{i,t}), e_{i,t}(\xi_{i,t}))}{\partial c_{i,t}^u}\right]}{E_{P+1}^{u,B}\left[\omega(\bar{e}_i)\frac{\partial u_i^u(c_{i,t}^u(\xi_{i,t}), e_{i,t}(\xi_{i,t}))}{\partial c_{i,t}^u}\right]} = \frac{1 + \frac{1}{S_{P+1}^R}\left[\frac{\partial D_b^R}{\partial b_{P+1}^R}b + \frac{\partial D^R}{\partial b_{P+1}^R}\tau\right]}{1 + \frac{1}{S_{P+1}^B}\left[\frac{\partial D_b^B}{\partial b_{P+1}^B}b + \frac{\partial D^B}{\partial b_{P+1}^B}\tau\right]}.$$

which corresponds to equation (1.5) in the main text.

### 1.B.2 Implementation Assumptions

- *Assumption 1* - Relevant heterogeneity in consumption only across Boom and Recession, not within unemployed in the same aggregate economic condition: $c_{i,t}^{u,k} = c_t^{u,k}$[36]
- *Assumption 2* - Homogeneous preferences: $u_i(c) = v_i(c) = u(c)$
- *Assumption 3* - Homogeneous relative risk aversion: $\gamma^k \approx \gamma$ (where $\gamma^k = \frac{-u''(c^{u,k})c^{u,k}}{u'(c^{u,k})}$)
- *Assumption 4* - Taylor approximation for $u'(c^{u,k})$ around $c^{e,k}$:

$$\begin{aligned} u'(c^{u,k}) &\approx u'(c^{e,k}) + u''(c^{e,k})(c^{u,k} - c^{e,k}) \\ &= u'(c^{e,k}) \left[1 + \gamma \frac{c^{e,k} - c^{u,k}}{c^{e,k}}\right] \end{aligned}$$

Thus, we arrive at the expression in the main text:

$$\begin{aligned} \frac{E_{P+1}^{u,R}[\omega(\bar{e}_i)u'(c_{i,P+1}^u)]}{E_{P+1}^{u,B}[\omega(\bar{e}_i)u'(c_{i,P+1}^u)]} &\approx \frac{\omega^R}{\omega^B}\frac{u'(c^{u,R})}{u'(c^{u,B})} \\ &\approx \frac{\omega^R}{\omega^B}\frac{u'(c^{e,R})}{u'(c^{e,B})}\frac{1+\gamma\frac{c^{e,R}-c^{u,R}}{c^{e,R}}}{1+\gamma\frac{c^{e,B}-c^{u,B}}{c^{e,B}}} \\ &\approx \frac{\omega^R}{\omega^B}\left[1+\gamma\frac{c^{e,B}-c^{e,R}}{c^{e,B}}\right]\frac{1+\gamma\frac{c^{e,R}-c^{u,R}}{c^{e,R}}}{1+\gamma\frac{c^{e,B}-c^{u,B}}{c^{e,B}}} \\ &= \frac{\omega^R}{\omega^B}\frac{1+\gamma\frac{c^{e,R}-c^{u,R}}{c^{e,R}}}{1+\gamma\frac{c^{e,B}-c^{u,B}}{c^{e,B}}} \end{aligned}$$

where the second line follows from another Taylor approximation of $u'(c^{e,R})$ around $u'(c^{e,B})$, and the last line from the empirical finding of $c^{e,R} \approx c^{e,B}$ shown in Table 1.A2.

### 1.B.3 Welfare Impact

Consider a reform where $\frac{db_{P+1}^R}{db_{P+1}^B} < 0$ such that the budget remains balanced:
$S_{P+1}^R b_{P+1}^R = S_{P+1}^B b_{P+1}^B \Rightarrow S_{P+1}^R(1+FE^R)db_{P+1}^R = -S_{P+1}^B(1+FE^B)db_{P+1}^B$

$$\begin{aligned} dW =& S_{P+1}^R E_{P+1}^{u,R}[\omega(\bar{e}_i)u'(c_{i,P+1}^u)]db_{P+1}^R + S_{P+1}^B E_{P+1}^{u,B}[\omega(\bar{e}_i)u'(c_{i,P+1}^u)]db_{P+1}^B - \\ &- \lambda\left[S_{P+1}^R(1+FE^R)db_{P+1}^R + S_{P+1}^B(1+FE^B)db_{P+1}^B\right] \\ =& S_{P+1}^R E_{P+1}^{u,R}[\omega(\bar{e}_i)u'(c_{i,P+1}^u)] - S_{P+1}^B E_{P+1}^{u,B}[\omega(\bar{e}_i)u'(c_{i,P+1}^u)]db_{P+1}^B\frac{S_{P+1}^R(1+FE^R)}{S_{P+1}^B(1+FE^B)}db_{P+1}^R \end{aligned}$$

[36] When there is heterogeneity within the same economics conditions, one needs to account for the covariance between marginal utility and welfare weights (and risk aversion parameter if not assumed to be constant) (Andrews and Miller, 2013; Kolsrud et al., 2024).

where the second equality follows from the budget neutrality of the reform.

$$
\begin{aligned}
\frac{dW/S^R_{P+1}db^R_{P+1}}{E^{u,B}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]} &= \underbrace{\frac{E^{u,R}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]}{E^{u,B}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]}}_{=1.64} - \underbrace{\frac{1+FE^R}{1+FE^B}}_{=0.69} \\
&= \mathbf{0.95}
\end{aligned}
$$

where welfare is measured in monetary terms relative to $E^{u,B}_{P+1}[\omega(\overline{e}_i)u'(c^u_{i,P+1})]$, which denotes the average welfare-weighted marginal utility for the individuals unemployed in Boom (Boom and Recession refer to 8% and 26% unemployment rate respectively).

## 1.C Time Use Data

A key input of the framework in the main text is the share of unemployed individuals that searched for jobs over the last week, and how it varies with economic conditions. Here I provide a more detailed explanation of how I estimate these statistics.

The data used are the two available waves (2002-2003 and 2009-2010) of the Spanish Time Use survey, which is similar to the American Time Use Survey (ATUS) in the US. Since this survey oversamples individuals during Fridays, Saturdays and Sundays, I estimate averages at the daily level for the variable of interest — share of individuals with positive search time — and then aggregate by adding up all the days of the week. I compute these averages at the region-year level (CCAA), which is the lowest geographical level available. I also collect data on unemployment rate at this same level from the Statistical National Institute (INE).

While time use surveys provide very detailed information on the activities of individuals throughout the day, they have the caveat that individuals only fill the dairy once, and so we cannot see how the same individual behaves over the whole week. Given that my statistic of interest is the share of individuals who searched last week, I estimate this from the daily probability of search. To do so, I follow Faberman et al. (2022) who compare their estimates from the Survey of Consumer Expectations (SCE) with the ones obtained with ATUS, and provide a framework to map daily probability of job search into weekly share of searchers. The advantage of SCE is that individuals are asked specifically about whether they searched for jobs last week, and not just one day as in ATUS. For this exercise, they leverage the fact that in the UK time use survey, individuals fill diaries for two consecutive days, which provides information about what share of individuals search frequently. They find that 35 % of the unemployed who search one day, repeat again the next day. They assume that (i) there are two types of searchers: steady or intermittent job seekers; (ii) individuals observed searching two days in a row are steady job seekers who search every day; (iii) individuals who do not search two days in a row are intermittent job seekers who search only once per week and could therefore be randomly observed looking for work on any day of the week. Based on this statistic, they assume that in the US 35% of unemployed search every day of the week and the rest only once per week, resulting in the following probability of weekly search:

$$\pi_{\text{week}_j} = \mu_j^{uk} \pi_j^{day} + 7(1 - \mu_j^{uk}) \pi_j^{day}$$

which provides a good approximation for their estimate with SCE data. Thus, I apply this to the Spanish data and obtain that the weekly share of searchers ranges between 50% in Boom and 95% in Recession. The analysis residualizes the variables on region FEs to not compare regions that are structurally different and focus on the variation over the business cycle. Figure 1.4 depicts the results.

Moreover, I also provide results estimating the share of weekly searchers using a different approach to prove the robustness of the findings. To do so I instead assume that search is independent across days, and just compute the probability of not searching in a week as $\pi_{\text{week}_j} = 1 - (1 - \pi_j^{day})^7$. This is a similar approach to Braun (2022), who computes the share at the monthly level instead. Results are shown in Figure 1.A3. Reassuringly, even in this case under a strong independence assumption, the share ranges between 52% and 78%, which implies a relatively lower increase but still a substantial shift which is in line with the baseline finding.

## 1.D Public Opinion Polls

As described in the main text, the empirical implementation of the framework suggests that the UI system should be more generous when economic conditions worsen. However, this induces a violation of horizontal equity which may raise implementation concerns. Here I explore public opinions about this type of reform, presenting two pieces of suggestive evidence on the political economy of a cycle-dependent benefit reform. First, in the online survey I designed, respondents are also asked whether they would support a UI reform of the type described in this paper. The objective of this question is to assess whether they agree with the interpretation of their answers through the lens of my framework. Figure 1.3d shows that a majority of people claim they would be likely to support a UI reform of the type described above.

Second, I draw on annual public opinion surveys over the years 2005-2019. This information on public attitudes towards certain public policies comes from the individual-level files of the Public Opinion and Fiscal Policy Survey (*Encuesta de Opinión Pública y Política Fiscal*) carried out by the Spanish CIS. This is a representative sample, eliciting opinions on several public policy issues over time. My analysis focuses on a question repeated every year that asks whether the unemployment benefits provided should be same/more/less generous than in the given year. I combine the waves for the years 2005-2019[37]. Here I focus on a question asked every year about whether they think that the government should devote more/less/same resources to UI than are provided at each point in time. I classify individuals as demanding more generous UI when they state that more resources should be devoted. The evolution of the answers to this question over the cycle are shown in Figure 1.A6. Panel (a) shows that people demand more generous UI when economics conditions are bad, with a remarkable increase in this demand that parallels the increase in the unemployment rate. Although I am not able to isolate mechanisms in this analysis, a potential explanation is that the probability of receiving benefits goes up in recession (probability of paying taxes goes down), so people just respond in that way that because they will be more likely to benefit directly from the reform, and not because they consider that there is a higher value of transferring resources towards the type of individuals unemployed in recessions. However, the same plot reveals that the increase in UI demand is common across groups (employed, unemployed, retired)[38], and thus does not depend on the group-specific

[37]The annual samples consist of around 2500 individuals, so the total sample contains information on approximately 38000 individuals.

[38]The increase in support for a more generous UI system when economic conditions worsen could be just driven by a composition effect: if the unemployed support more UI increases than the employed on average, the fact that more people become unemployed in Recession could explain the increase in the support for UI. It is not the case, which can also be shown

probability of receiving benefits which varies across them. To gain further insights, in Panels (b) and (c) I divide regions in two groups by the size of the region unemployment shock experienced in order to see whether regions that are relatively hit harder by the Great Recession increase their support for higher UI generosity more. I find again that the increase is common across groups and not related to the change in the unemployment risk. Although more suggestive, this complementary evidence is consistent with people taking into account other dimensions beyond their own unemployment risk, and supporting more generous UI when economic conditions deteriorate.

formally using a shift-share decomposition of the change in UI support from trough to peak (2005-2014): $UI_t - UI_{t-1} = \sum s_{i,t-1}(UI_{i,t} - UI_{i,t-1}) + (s_{i,t} - s_{i,t-1})UI_{i,t}$, where the within-group component accounts for 95% of the increase.

## 1.E Online Survey

### Instructions (Information Sheet)

**You are invited to participate in this survey, which investigates the preferences of Spanish people for public policies. It takes approximately 8 minutes. When you complete it, you will receive panel points worth €1.15 as a reward.**
We are a non-partisan group of researchers affiliated to University College London. Our goal is to understand preferences about various social policies. **It is very important that you respond honestly. No matter what your political views are, this is an important question and by completing this survey, you are contributing to our knowledge as a society.**
Anytime you don't know an answer, just give your best guess.

All the information collected about you is treated confidentially. The information is only used for scientific purposes and the survey is registered at University College London. This is intended to be used for academic publication, a chapter of a PhD thesis and potentially for subsequent research. You have been chosen to participate because you are aged 25-64 and are representative of the Spanish population. Participation is voluntary and you can withdraw at any point if you wish. **The data are anonymized and you will not be individually identified from responses.** There are no direct risks or benefits for you from taking part in the survey, beyond the monetary reward. This project has received approval by the UCL Research Ethics Committee (#21191/001). In case you have doubts or complaints, you can contact the corresponding member of the Research Team (jon.piqueras.17@ucl.ac.uk) or the Ethics Committee (ethics@ucl.ac.uk). The title of the study is "Redistribution and Social Insurance Design". This project is funded by the Institute for Fiscal Studies.

Local Data Protection Privacy Notice:

The controller for this project will be University College London (UCL). The UCL Data Protection Officer provides oversight of UCL activities involving the processing of personal data. This "local" privacy notice sets out the information that applies to this particular study. Further information on how UCL uses participant information can be found in our "general" privacy notice (here). The categories of personal data used will be as follows: year of birth, gender, province of residence, employment status, income, socio-economic status, education, postcode. The lawful basis that will be used to process your personal data are: "Public task" for personal data and "Research purposes" for special category data. Your data will be anonymized at any point and therefore it cannot be withdrawn. If you are concerned about how your personal data is being processed, or if you would like to contact us about your rights, please contact UCL in the first instance at data-protection@ucl.ac.uk.

**Thank you for reading this information sheet and for considering to take part in this research study. If you agree with these terms, please continue reading the consent form below before you start.**

### Consent Form

**I confirm that I understand that by clicking "Next" below I am consenting to ALL these elements of the study. I understand that by not giving consent to these elements I may be deemed ineligible for the study.**

- I confirm that I have read and understood the Information Sheet for the above study. I have had an opportunity to consider the information and what will be expected of me.
- I consent to participate in the study. I understand that my personal information will be used for the purposes explained to me. I understand that according to data protection legislation, "public task" will be the lawful basis for processing.
- I understand that my data gathered in this study will be stored anonymously and securely. It will not be possible to identify me in any publications.
- I understand that my participation is voluntary and that I am free to withdraw at any time without giving a reason.
- I understand the direct/indirect benefits and risks of participating.
- I understand that I will be compensated if I complete the survey.
- I agree that my anonymized research data may be used by others for future research.
- I hereby confirm that I understand the inclusion criteria as detailed in the Information Sheet.
- I am aware of who I should contact if I wish to lodge a complaint.
- I voluntarily agree to take part in this study.

Next

# Preliminary Questions

**What is your year of birth?**

--------

**What is your sex?**

- ◯ Male
- ◯ Female

**What is your current employment status?**

- ◯ Full-time employment
- ◯ Part-time employment
- ◯ Unemployed looking for jobs
- ◯ Other (for example, student or retired)
- ◯ Disabled/unable to work

**What is your highest educational level?**

- ◯ Primary education
- ◯ Secondary education
- ◯ Baccalaureate/professional training
- ◯ Bachelors degree
- ◯ Masters degree
- ◯ PhD

Next

# Demographics

**What is your province of residence?**

--------

**What is the duration of your usual workday? (for example, 100% corresponds to full time and 50% to part time)**

- 0%
- 10%
- 20%
- 30%
- 40%
- 50%
- 60%
- 70%
- 80%
- 90%
- 100%

**What is your gross <u>monthly</u> labor income if employed, or benefits if unemployed? (that is, before taxes, without decimals, in €)**

**What is your current type of job?**

- Permanent
- Temporary
- Self employed
- Not working

**How many children do you have (younger than 26 years old)?**

--------

**How much do you have in savings (savings in current account + value of other assets you own - debt, in €)?**

--------

**While you were 18-25 years old, was any of your parents more than one year unemployed?**

- Yes
- No

**While you were 18-25 years old, did your parents have difficulties to make ends meet?**

- Yes
- No

Next

20%

# Opinions on Public Policy

*Imagine there are 2 unemployed individuals, A and B, who receive €950 while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed:*

- *Individual A looked for jobs last week*
- *Individuo B did not look for jobs last week*

**The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Please choose what allocation option you would prefer:**

- ◯ More money to A than to B
- ◯ More money to B than to A
- ◯ Same amount to A and B

Next

40%

# Opinions on Public Policy

*Imagine there are 2 unemployed individuals, A and B, who receive 950€ while unemployed and are entitled to receive benefits for up to 14 months if remaining unemployed:*

- *Individual A looked for jobs last week*
- *Individual B did not look for jobs last week*

*The government wants to transfer €1000 to individuals A and B in a one-time payment, and is thinking of how to allocate them. Now imagine that an unequal allocation may be costly. That is, it can allocate €500 each, or more to one than the other, but the latter option may result in the total amount of resources that can be distributed being reduced.*

*Please choose, for each pair of options below, what allocation option you would prefer:*

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

○ 550 for A, 450 for B
○ 500 for A, 500 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

○ 350 for A, 550 for B
○ 500 for A, 500 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

○ 150 for A, 550 for B
○ 500 for A, 500 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

○ 500 for A, 500 for B
○ 450 for A, 550 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

○ 500 for A, 500 for B
○ 250 for A, 550 for B

Next

60%

# Opinions on Public Policy

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

- ○ 50 for A, 550 for B
- ○ 500 for A, 500 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

- ○ 500 for A, 500 for B
- ○ 550 for A, 300 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

- ○ 550 for A, 350 for B
- ○ 500 for A, 500 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

- ○ 500 for A, 500 for B
- ○ 300 for A, 550 for B

**Individual A looked for jobs last week**
**Individual B did not look for jobs last week**

**What amounts (€) would you prefer for individuals A and B?**

- ○ 500 for A, 500 for B
- ○ 550 for A, 400 for B

Next

80%

# General Opinions (last questions)

**When you were asked before about your preferred allocation of the €1000, you answered you would prefer to give "More money to A than to B".**
**(Note: Remember that A looked for jobs last week, and B did not looked for jobs last week).**

**Please, explain why you chose that option:**

---

**There exists the idea that the benefits received by each unemployed could be more generous (higher benefit level or duration) at times when economic conditions are bad (high unemployment rate), relative to times where economic conditions are good (low unemployment rate).**
**Some reasons for that are that when the economic conditions are bad, the unemployed suffer larger consumption reductions upon job loss and the type of unemployed at that moment are, on average, people who put more effort into job finding but who face more difficulties given the lack of job offers.**

**Would you agree with having unemployment benefits changing with economic conditions in this way?**

- Yes
- No

---

**We want to improve the survey and your opinion matters. Please, write below if you have any comment about the questions, the survey structure or whether the questions are clear/intuitive (in case you do not have comments, write NO).**

---

Next

100%

## Many thanks for answering the questions!

---

## Please click on "End survey" to end your participation.

End survey

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# Chapter 2

# Search Effort and the Minimum Wage

## 2.1 Introduction

After extensive economics research, and given the patterns of growing inequality and stagnant wages at the bottom of the distribution observed in recent decades, the minimum wage continues to be a deeply debated policy tool. A prominent view among its opponents in the minimum wage debate is that employment is determined by the demand side of the market, and an increase in wages will come at the cost of fewer jobs. Nevertheless, this narrative contrasts with the view often exhibited when analyzing other economic policies like unemployment insurance, where it is postulated that the supply side influences employment (Manning, 2021).

Bringing this alternative scenario to the minimum wage setting, in a context of imperfect labor markets with frictions, the impact of the policy is theoretically ambiguous. When firms post vacancies and unemployed individuals search for jobs, an increase in the minimum wage may lead to firms reducing vacancies, but this can be compensated by increased search effort by the unemployed, which can lead to employment remaining constant in equilibrium.

Despite the theoretical relevance of this search effort mechanism (e.g., Pissarides, 2000; Acemoglu, 2001), there is very limited empirical evidence on how effort reacts to the minimum wage. In order to estimate the effort response to the minimum wage by the unemployed, one needs at least three features in a context with minimum wage variation: (i) large sample size on unemployed individuals; (ii) measure of search intensity; and (iii) measure of exposure to the minimum wage. The lack of these three features in the same dataset in a context with substantial minimum wage variation may be behind the lack of evidence.

This paper overcomes the empirical challenges in the following way. First, I use data from the CPS Basic to get a large sample of unemployed individuals in the United States. Second, in order to obtain a measure of search intensity, I combine data from the CPS Basic and from Time Use data (ATUS). In particular, I exploit the fact that a set of questions on search methods used by the unemployed are present in both datasets. I apply machine learning methods to train a model in the ATUS data that predicts search intensity using information on search methods and demographics. After that, I use the estimated model to predict search intensity back in the CPS in order to make use of the large sample size. Third, given the importance of assessing the impact of the minimum wage on potentially affected individuals, I obtain a measure of exposure to the minimum wage for unemployed individuals. For this exercise, I apply machine learning methods following Cengiz et al. (2022) in a sample of employed individuals where wage information is available (CPS MORG) in order to predict minimum wage status based on demographic characteristics. I then use this trained model to predict exposure based on demographics among my unemployed individuals in the CPS Basic.

Having constructed these measures, I then estimate the impact of the minimum wage on the search effort of unemployed individuals most affected by it, using 49 state-level minimum wage events in a stacked event-study specification over the years 1999-2019 in the United States. I find that an increase of the minimum wage by 12% leads to an increase of search effort by 6.1%. This finding is robust to several concerns, including confounding state-level shocks, and provides the first evidence on an important mechanism often highlighted in search models, which is crucial to explain the employment effect documented by the literature.

At the same time, I also find that individuals who increase effort are not more likely to find a job quicker, which is consistent with the zero employment effect in the literature.

In order to interpret the estimates, I take a standard random search model (DMP) and introduce unemployed's search effort. The employment response in this framework can be decomposed between a partial equilibrium response of effort when market tightness is held constant, and a market-level adjustment term where effort is held fixed but tightness is allowed to adjust. In this context, the effort response found predicts that employment should have increased by 4 p.p. in response to the minimum wage if tightness did not adjust. However, the fact that, in equilibrium, vacancies may go down and effort goes up, makes that the return to every unit of effort in terms of job finding decreases, and so equilibrium employment remains unchanged.

Moreover, the proposed setup allows me to study the welfare impact of the minimum wage in a very transparent way. The key idea is that the search effort response by unemployed individuals reveals information about their welfare. Specifically, the observed increase in effort allows me to obtain an upper bound for the negative welfare impact due to higher search cost. I then compare this cost with the welfare benefit due to increased expected wages, which results in an unambiguously positive impact on welfare.

My paper contributes to several strands of the literature. First, it relates to the literature on job search and the minimum wage. The existing literature in this area is mainly theoretical and emphasizes the importance of the search effort channel (e.g., Burdett and Mortensen, 1998; Pissarides, 2000; Acemoglu, 2001; Flinn, 2006; Ahn et al., 2011). Yet, the empirical evidence lags behind. To the best of my knowledge, there are only two papers studying this question, which document small and mixed responses. Laws (2018) finds no clear evidence that the policy affects search effort, with some suggestive evidence that the minimum wage may decrease the unemployed's search effort. Evidence from Adams et al. (2022) reveals a very short-lived positive effect that appears only during the month when the minimum wage is increased, which immediately vanishes after that. In contrast, I find a significant positive long-term impact on job search effort of the low-wage unemployed, providing evidence on the empirical relevance of this core mechanism in search models.

Second, it relates to work on job search and unemployment, and in particular to the one employing Time Use data (e.g., Krueger and Mueller, 2010; 2012; DellaVigna et al., 2017; Mukoyama et al., 2018; Faberman and Kudlyak, 2019; Marinescu and Skandalis, 2021; DellaVigna et al., 2022; Faberman et al., 2022; Adams et al., 2022). Two papers in this area are particularly related to my work. First, my paper relates to Mukoyama et al. (2018) who combine information in CPS and ATUS data to build a measure of search effort, and investigate how it varies over the business cycle. Second, my work relates to Adams et al. (2022) who investigate the impact of the minimum wage on search effort. This latter paper uses ATUS data and an event-study approach and find that effort only responds positively to the minimum wage in the very short-run, one month, among individuals that report non-zero search. Nevertheless, this effect appears to be temporary and is only found in a special population selected based on contemporaneous search behavior. On the one side, their sample size is small, so it is not clear whether they are able to detect a long-term effect even if there was one. On the other side, in most specifications they focus on all workers/unemployed, so given that the minimum wage workers are a small share of the labor force it is not clear that one can detect an effect by looking at all the individuals[1]. I contribute by combining the richness of Time Use data with the large sample size of the CPS Basic using machine learning methods, and using this setup to learn about the causal impact of the minimum wage.

Third, it relates to work on the employment effects of the minimum wage (e.g., Card and Krueger, 1994; Giuliano, 2013; Dube et al., 2016; Cengiz et al., 2019; 2022; Godøy et al., 2021; Dustmann et al., 2022) . While the equilibrium employment effects of the policy have received much attention, I provide empirical evidence on a leading mechanism behind the observed equilibrium outcome. This allows to obtain additional information on a key input of search-and-matching models that I use to investigate the welfare consequences of the policy.

The rest of the paper proceeds as follows. Section 2.2 describes the data. Section 2.3 explains the empirical methodology. Section 2.4 presents the results. Section 2.5 interprets the estimates through

[1]They also investigate effects for some demographic groups more likely to be affected by the minimum wage but, unfortunately, the reported estimates lack precision presumably due to the even smaller sample size in these specifications.

the lens of a standard search model and assesses the welfare impact of the policy, and Section 2.6 concludes.

## 2.2 Data

**Current Population Survey - Basic.** I use the CPS Basic files over the years 1999-2019. This is a monthly survey containing around 60,000 households in the United States. The demographics of interest used are age, race, Hispanic, gender, education, veteran status, marital status and rural residency. If unemployed, the dataset also includes information on whether individuals used a variety of methods to search for jobs (listed in Table 2.1). Moreover, the survey structure also has a short individual panel dimension, with individuals being first in the sample during four consecutive months, followed by eight months out, and then four more months in the sample again. I exploit this to construct a 2-month job finding rate, defined as the probability that an individual found a job within the next two months.

**Current Population Survey - Merged Outgoing Rotation Group.** I also use the CPS-MORG files, which corresponds to a subset of the sample present in the CPS Basic. Specifically, these observations correspond to individuals who are in their fourth or eight month in the sample, when usual hourly wages, weekly earnings and weekly hours worked are asked. I exclude observations with imputed values for hourly wages, weekly earnings or hours worked. As is standard in the literature, I use reported hourly wage for hourly workers and define hourly wage as usual weekly earnings divided by usual weekly hours for other workers.

**American Time Use Survey.** I leverage information from ATUS over the years 2003-2019. Respondents fill a daily diary where they register the amount of time devoted to different detailed activities, including job search. Moreover, individuals in this sample are also asked the same demographic and job search methods questions that are asked in the CPS Basic.

**Minimum Wages.** I use data on state-level minimum wages over 1979-2019 from Vaghul and Zipperer (2016) and updated by Cengiz et al. (2022). The empirical analysis leverages 49 minimum wage changes after applying several restrictions as described in Section 2.3.3.

## 2.3 Empirical Methodology

In order to assess the impact of the minimum wage on unemployed's search effort, I proceed in three steps. First, I construct a variable capturing exposure to the minimum wage in order to know what individuals are potentially affected by the minimum wage. Second, I build a search effort measure that can be used in a large sample size like the CPS Basic. Third, I present the regression model used to estimate the causal impact of the minimum wage.

### 2.3.1 Exposure to the Minimum Wage

The first step needed to investigate how the minimum wage affects search behavior by the unemployed is to know who the unemployed that would potentially earn the minimum wage upon employment are. This is important since the minimum wage workers are a small share of the labor force, and thus one needs to zoom in on the set of workers that will potentially respond to the policy in case there is a response. Otherwise one can find that effort does not react in the whole population even when there is a significant effect among the treated population. Nevertheless, wages for the unemployed are not observed, which complicates classification of the unemployed into minimum wage individuals. In order to address this challenge, I follow the prediction approach proposed by Cengiz et al. (2022)[2], which is described as follows.

**Prediction Algorithm.** The algorithm employed to classify unemployed individuals into being affected by the minimum wage is based on gradient-boosted decision trees (Friedman, 2001). To operationalize this approach, I work with datasets from CPS-ORG and CPS-Basic. Using the wage information in the CPS-ORG, I define being exposed to the minimum wage as earning less than 125% of the minimum wage. Focusing on the periods before state-level changes occur, I divide the sample into a training and a test sample. Then, I apply the prediction algorithm where a set of demographics (age, education, gender, rural residency, marital status, race, Hispanic and veteran status) are used to predict whether individuals are exposed to the minimum wage. The tuning parameters are set using 5-fold cross validation. Once the model is trained in the training dataset of the CPS-ORG, I use it to predict exposure to the minimum wage on the sample of unemployed individuals in the CPS Basic, using the same set of demographics as predictors.

[2]This methodology builds on the original classification approach proposed by Card and Krueger (1995).

**Assessing Model Performance.** A standard way to assess performance in the machine learning literature is to analyze precision-recall curves. For a given subsample of individuals, precision refers to the number of true minimum workers contained in that subsample as a proportion of the number of people in the subsample. Recall refers to the number of true minimum wage workers in a subsample as a proportion of the total number of minimum wage workers in the population. For example, if a subsample contains one observation with a true minimum wage worker, the precision rate is 1 but the recall rate is very small since all but one minimum workers in the population are not in this subsample. For my sample, the precision-recall curve is shown in Figure 2.1. When recall is around 10%, the share of true minimum wage workers in the sample is above 70%. As we increase recall, precision tends to decline since generally there exists a trade off between precision and recall when choosing a specific subsample.

For my main analysis, I define the group of individuals highly exposed to the minimum wage as those above the $90^{th}$ percentile in the distribution of the predicted probability of being a minimum wage worker, and everyone else as not exposed to the policy. The characteristics of both groups are shown in Table 2.2. The group of minimum wage individuals has a precision rate around 70%, and is generally younger and less educated than the rest of the unemployed.

### 2.3.2 Search Effort Measurement

The second step is to obtain a measure of job search effort that can be used for causal inference. Given the limited sample size of the ATUS data where detailed search effort information is present, this dataset cannot be successfully used directly to assess the impact of the minimum wage. However, it can be used in combination with the larger sample size of the CPS Basic to learn about the search behavior of the unemployed, given that the questions on the job search methods (listed in Table 2.1) are present in both datasets. This procedure can thus be seen as a flexible, data-driven method to aggregate the information contained in the search methods, weighting them according to their relative importance as inputs into the search effort production function.

**Prediction Algorithm.** I build on Mukoyama et al. (2018) who exploit both datasets to understand job search behavior over the cycle. I deviate from them by applying machine learning methods for the prediction model, which allows me to obtain higher precision. Specifically, I start by dividing the ATUS data into a training and a test dataset and apply a gradient-boosted decision trees algorithm. Using the information provided by unemployed individuals in their diary, I code as daily searchers those who report a positive amount of time spent on job search activities in a given day. This is the main outcome, which the model predicts based on a set of demographics (age, education, gender, marital status, race, Hispanic) and search methods. Tuning parameters are chosen using a 5-fold cross-validation method. Once the model is estimated, I use it to predict the probability of daily search on the sample of unemployed individuals in the CPS Basic sample.

**Descriptive Patterns.** Here I provide a brief descriptive analysis of the search effort measures used among the unemployed, in order to explore patterns over time and make sure they capture a meaningful dimension of job search behavior.

First, Figure 2.A1 depicts the behavior of this predicted measure over the business cycle. The results are broadly consistent with the findings by Mukoyama et al. (2018), who show that aggregate job search exhibits a countercyclical behavior, showing large increases when the unemployment rate goes up.

Second, Figure 2.A2 shows the evolution of the share of the unemployed using each search method over time. A few interesting facts emerge: contacting employers directly, having interviews and sending applications are the most common search methods overall; and contacting friends and relatives also stands out as widely used, with higher importance since the financial crisis.

### 2.3.3 Identification Strategy

The third step once we know who the unemployed individuals affected by the minimum wage are, and how much search effort their exert, is to assess the impact of the minimum wage. To do so, I leverage state-level variation in the minimum wage and compare labor market outcomes in treated and control states following the stacked event study approach proposed by Cengiz et al. (2019). The events used are defined as those policy changes where the (real) minimum wage increased by more than \$0.25 and where at least 2% of the workforce earned between the new and the old minimum wage. Moreover, I restrict the set of events to 49 minimum wage events where the treated states did not experience other (non-trivial) minimum wage increase in the 3 years prior to the event, and where the control states did not have a (non-trivial) minimum wage increase within the 7-year window. I create event-datasets, stack them by event time, and estimate a model of the following form:

$$Y_{hst} = \sum_{\tau=-3}^{3} \beta_\tau \cdot treat_{hst}^{\tau} + \Omega_{hst} + \mu_{hs} + \rho_{ht} + u_{hst} \tag{2.1}$$

where $Y_{hst}$ corresponds to outcome in event-dataset $h$, state $s$ and event-time $t$, and $\Omega_{hst}$ captures small and federal MW changes. This design uses only within event variation, which prevents negative weighting issues highlighted in the recent difference-in-differences literature (e.g., Callaway and Sant'Anna, 2021; Borusyak et al., 2022).

## 2.4 Results

**Main Results.** Figure 2.2 shows the main results. Panel (a) depicts that for the period considered, the statutory minimum wage increased persistently by around 12% in treated states relative to control ones. This outcome exhibits no pre-trend prior to the minimum wage change, which is expected given the selection procedure of events.

At the same time, as shown in Panel (b), the job search effort of the highly exposed unemployed individuals increased by around 6.1%. The outcome of interest presents similar trends in treated and control states prior to the reform, and suddenly reacts when the minimum wage is increased. After that, search effort remains elevated at approximately the same relative level.

In Panel (c), I investigate whether the change in search effort led to higher job finding, exploiting the panel structure of the CPS. The results reveal that the higher search effort did not lead to increased job finding. This outcome is flat prior to the policy change and remains unaltered afterwards.

**Effect on Individuals Not Exposed to the Minimum Wage.** One possibility is that the increase in search effort is driven by a confounding shock that hits treated states at the same time as the minimum wage. In order to alleviate this concern, here I repeat the same analysis as above but focus on the rest of unemployed individuals who are not predicted to earn the minimum wage upon employment. For them, the increase in the minimum wage should not lead to large effort responses since they are not directly affected by the policy.

Figure 2.3 shows the results. Reassuringly, individuals located in treated states but less exposed to the minimum wage do not change their search behavior relative to control states (Panel (b)). Moreover, job finding remains also constant for these individuals (Panel (c)).

**Selection.** With heterogeneity in search effort, the observed increase in effort could be driven not by a change in behavior but by a selection effect, where individuals who search harder are more likely to remain unemployed after the reform. Nevertheless, the empirical evidence indicates that this concern has limited relevance for several reasons. First, evidence from Cengiz et al. (2022) shows that the minimum wage does not induce responses along the participation margin, so that changes in the unemployed composition driven by different people entering the labor market are not quantitatively important.

Second, as shown in Figure 2.4, I assess the robustness of the main effect to iterative inclusion of several controls, and I find that the main effect remains approximately constant once these covariates are accounted for. The first row shows the baseline specification. The specification in the second row controls for the predicted probability of being a minimum wage individual, in order to assess whether the composition of affected unemployed changes towards more/less exposed individuals. The third row adds a state-level unemployment rate control. The fourth row adds several demographic controls (age, sex, race, rural status, education). Overall, the results remain robust to the inclusion of a variety of controls, underscoring that selection plays a limited role in explaining the results.

**High Frequency.** Here I consider 6-month periods in order to assess whether the observed pattern is driven by unexpected underlying dynamics. Although results are somewhat more noisy, Figure 2.A3 reveals that search effort remains flat prior to the reform and reacts in a permanent way as soon as the minimum wage is enacted. This implies that the dynamics of job search effort in response to the policy appear to be as expected also when assessed with higher frequency.

**Search Time.** So far, the main outcome of interest has been the probability of daily search. As an alternative outcome, I also investigate search behavior in terms of daily time spent on job search activities. This outcome is much more skewed and has a large proportion of individuals searching zero minutes. In this case, I apply a Poisson model to predict search time in the ATUS data, and then follow a similar procedure as before to predict in the CPS Basic. This variable is somewhat predicted with less precision but, as shown in Figure 2.A4, event-study results show a similar qualitative and quantitative pattern relative to the outcome used in the main analysis. This highlights that the results do not depend on the specific definition of the search effort variable.

**Comparison with "Naive" Approach.** Here I assess to what extent my approach that applies machine learning methods to exploit information in both ATUS and CPS datasets improves upon a method that only uses the search information present in the CPS Basic. To that end, I construct a measure of search effort in the CPS Basic defined as the count of the number of search methods used by each individual as in Shimer (2004). The results are depicted in Figure 2.A5. Although the pattern is consistent with my main results where effort increases after the reform, now point estimates are much more noisy, not statistically significant and exhibit a decline following the event. This highlights the value of employing the methodology used for the main analysis.

**Individual Search Methods.** Additionally, even though precision is limited, I also explore how each of the different methods reacts individually to the minimum wage. For that, I estimate search method-specific event studies and compute the average effects for all of them, which are depicted in Figure 2.A6. As can be seen, after the reform, individuals tend to read more about job openings and send applications, and they seem to contact friends and relatives less often to find jobs.

## 2.5 Economic Implications

Given the main results obtained in the previous section—minimum wage increases effort but does not increase job finding—, here I proceed to interpret the estimates within the framework of a standard random search model. This allows me to assess the importance of the different mechanisms at play behind the equilibrium outcome and use the structure to evaluate the welfare consequences of the policy.

### 2.5.1 Equilibrium Employment Effect of the Minimum Wage

In particular, I focus on a DMP model with unemployed's search effort (Pissarides, 2000; Acemoglu, 2001; Lalive et al., 2015; Landais et al., 2018). Firms post vacancies $v$, and unemployed individuals $u$ exert search effort $s$. The total number of matches is given by a constant returns to scale matching function (Petrongolo and Pissarides, 2001). Market tightness is $\theta = \frac{v}{su}$. Endogenous job finding rate is $h = sf(\theta)$ and exogenous job destruction rate is $\delta$. Steady state employment probability is $e = \dfrac{sf(\theta)}{\delta + sf(\theta)}$.

**Individual's problem.** Value functions for an individual whose potential wage is the minimum wage are:

$$V^u = \max_s b - \psi(s) + sf(\theta)[V^e - V^u] \tag{2.2}$$

$$V^e = MW + \delta[V^u - V^e] \tag{2.3}$$

where $V^u$ and $V^e$ denote the value of being unemployed and employed respectively, $\psi(\cdot)$ is and increasing and convex search cost, $b$ refers to income while unemployed, and $MW$ refers to the income earned upon employment, which corresponds to the minimum wage.
This problem leads to an optimal search effort that is given by:

$$\underbrace{s^*(MW)}_{\textbf{search effort}} = \psi'^{-1}\big( \underbrace{f(\theta)}_{\substack{\textbf{return to search} \\ (+)}} \quad \underbrace{[V^e(MW) - V^u]}_{\substack{\textbf{value of employment} \\ (+)}} \big) \tag{2.4}$$

That is, the model predicts that search effort should be higher when $f(\theta)$—the return to effort in terms of job finding per unit of effort— is higher, and when $V^e(MW) - V^u$—the gap between the value of employment and unemployment— is larger.

**Mechanisms of Equilibrium Employment.** Here I analyze the mechanisms highlighted by this framework to explain the employment effect of the minimum wage. The objective is to fix ideas and to quantify the different forces at play behind the null impact in equilibrium. Given that job destruction is exogenous, I focus on how the job finding rate responds to the change in the minimum wage[3]. The impact of the minimum wage on job finding can be decomposed as follows:

$$\frac{dh}{dMW} = \frac{d(s \cdot f(\theta))}{dMW} = \underbrace{\frac{\partial s}{\partial MW} \cdot f(\theta)}_{\textbf{effort response}} + \underbrace{s \cdot \frac{\partial f(\theta)}{\partial MW}}_{\textbf{market-level adjustment}} \tag{2.5}$$

The first term captures the change in search effort induced by the change in the minimum wage while market tightness is held fixed. The second term is the market-level adjustment, which captures the impact on job finding that comes from changes in market tightness, keeping effort fixed. In equilibrium, the minimum wage increases equilibrium effort (and may cause equilibrium vacancies to decrease[4]), which unambiguously decreases tightness. That is, for a given level of effort, the returns in terms of job finding are lower when tightness is lower, so that job finding may decrease via labor market adjustment.

In order to quantify the importance of each mechanism, I leverage the estimates of the main analysis on the job finding and the effort responses, and back up the implied market-level response.

[3]Given the findings in the literature that job destruction may decrease in response to the minimum wage (e.g. Dube et al., 2016), my findings here are a lower bound, implying that the quantified forces could be larger if job destruction is allowed to adjust.

[4]See Kudlyak et al. (2023) for some recent evidence in this dimension.

In the main analysis, I estimated the impact on job finding ($\frac{dh}{dMW}$) and effort ($\frac{\partial s}{\partial MW}$).[5] However, we also need to estimate $f(\theta)$ in the effort response term, in order to know how effort affects job finding. I estimate it in Appendix 2.B , where I provide a full explanation of the empirical design and show the estimates obtained. The key idea of the approach is an instrumental variables strategy where I use Unemployment Insurance extensions as a shifter of search effort, and compare the search behavior of UI eligible individuals relative to ineligible individuals within the same labor market. In that way, I identify the parameter of interest, being able to net out market level effects (Lalive et al., 2015). The results are shown in Table 2.3, where I find that a 1% increase in search effort leads to a 0.014 increase in the 2-month job finding probability.

**Calibration.** To recap, I found that a 12% increase in the minimum wage increases effort by 6.1%. I also found that a 1% increase in search effort leads to a 0.014 increase in job finding. In addition, I found that a 12% increase in the minimum wage has zero impact on equilibrium employment. Given the data, I also have that employment probability is $e = 0.8$, and job destruction rate is $\delta = 0.065$. Putting all together, this implies that the effort response term in equation (2.5) equals 4 p.p. In other words, given the observed effort increase, the model predicts a sizable increase in job finding. Since, in equilibrium, job finding does not change, then it must be the case that the market-level adjustment term goes in opposite direction with a similar magnitude. Specifically, this means that market tightness goes down, bringing job finding back to the pre-reform level.

### 2.5.2 Welfare Effect of the Minimum Wage

Given the results and framework above, I now provide a simple assessment of the welfare impact of the minimum wage. The effort response obtained in this setup allows me to investigate whether the policy increased welfare for minimum wage individuals. The key idea is that the change in search effort by the unemployed after the minimum wage is increased reveals information about the welfare impact of the policy. Intuitively, by increasing effort after the policy is implemented, the unemployed reveal that the search cost incurred must be at least as large as the expected gains, considering their impact on the employment probability and the higher income earned. With information on the expected gains,

[5]To be precise, although $\frac{\partial s}{\partial MW}$ refers to the *partial* impact of the minimum wage on search effort while keeping tightness fixed, my design isolates the *total* impact on search effort allowing tightness to adjust. As a result, my estimate corresponds to a lower bound, which would mean that the estimated market-level adjustment obtained in the empirical exercise could be even larger in absolute terms. I estimate a lower bound because effort responds positively to tightness and the value of employment as shown in equation (2.4). Since the minimum wage increases the value of employment an unambiguously decreases tightness (even more so if vacancies decrease), the estimated effect already incorporates the potential decrease in effort due to lower tightness in equilibrium.

the effect on search effort can then be used to provide bounds on the change in search cost, and thus, on the welfare impact.

For simplicity of exposition, consider a two period model. Before the policy, when $MW = MW^0$, unemployed's indirect utility is given by:

$$V^u_{MW^0} = b - \psi(s_0^*) + s_0^* f(\theta^{s_0^*}_{MW^0}) MW^0 + (1 - s_0^* f(\theta^{s_0^*}_{MW^0}))b \tag{2.6}$$

where $s_0^*$ corresponds to the optimal search effort when $MW = MW^0$, and $\theta^{s_0^*}_{MW^0}$ refers to market tightness when both vacancies and effort adjust to the policy. After the minimum wage is increased ($MW = MW^1$), the unemployed's indirect utility becomes:

$$V^u_{MW^1} = b - \psi(s_1^*) + s_1^* f(\theta^{s_1^*}_{MW^1}) MW^1 + (1 - s_1^* f(\theta^{s_1^*}_{MW^1}))b \tag{2.7}$$

where, similarly, $s_1^*$ refers to the optimal search effort, and $\theta^{s_1^*}_{MW^1}$ is the tightness under optimal vacancies and effort when $MW = MW^1$. Note that individuals could choose between adjusting search effort in response to the minimum wage, or keeping it at the previous level. If effort remained constant, their expected utility would be:

$$b - \psi(s_0^*) + s_0^* f(\theta^{s_0^*}_{MW^1}) MW^1 + (1 - s_0^* f(\theta^{s_0^*}_{MW^1}))b \tag{2.8}$$

As documented before, we know that the change in the policy led to an increase in search effort ($s_1^* > s_0^*$), which implies that the former individual's utility is higher ( i.e., (2.7) $\geq$ (2.8)). Using this inequality, I obtain an upper bound for the welfare cost incurred due to increased search effort:

$$(s_1^* f(\theta^{s_1^*}_{MW^1}) - s_0^* f(\theta^{s_0^*}_{MW^1}))(MW^1 - b) \geq \psi(s_1^*) - \psi(s_0^*) \tag{2.9}$$

This allows me to finally combine the framework with the estimates from the empirical exercise to assess the overall welfare impact:

$$
\begin{aligned}
\Delta W &= V^u_{MW^1} - V^u_{MW^0} \\
&= b - \psi(s_1^*) + 0.8 \cdot 11.2 + 0.2b - (b - \psi(s_0^*) + 0.8 \cdot 10 + 0.2b) \\
&= -(\psi(s_1^*) - \psi(s_0^*)) + 0.8 \cdot 1.2 \\
&= -(s_1^* f(\theta^{s_1^*}_{MW^1}) - s_0^* f(\theta^{s_0^*}_{MW^1}))(MW^1 - b) + 0.8 \cdot 1.2 \\
&= -0.04(11.2 - b) + 0.8 \cdot 1.2 \\
&> 0.
\end{aligned}
\tag{2.10}
$$

where the fourth equality uses the upper bound argument from (2.9), and the final expression holds for any plausible value of unemployed's income $b$.[6] As a lower bound, for the case of $b = 0$, I find that a $\$1$ increase in the minimum wage increases welfare by $\$0.43$.

In summary, the increase in the minimum wage led to a positive welfare impact on individuals exposed to the policy.

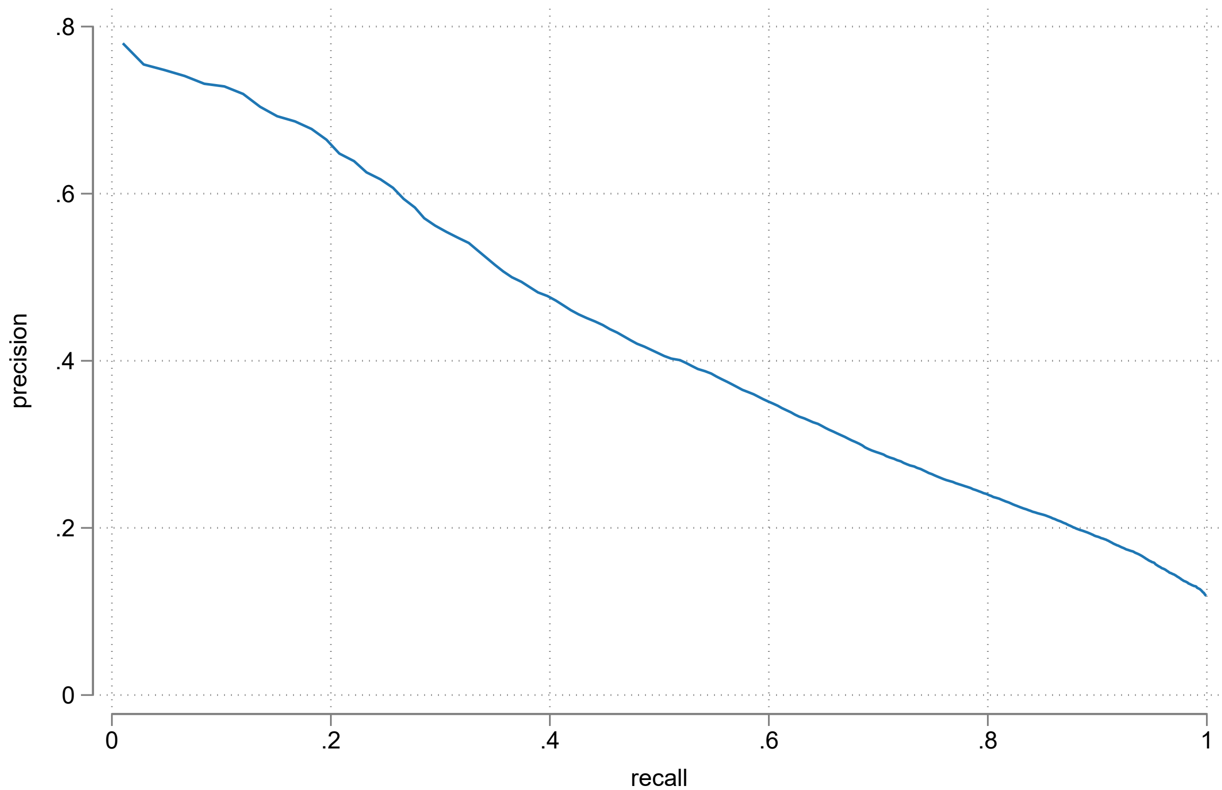
## 2.6 Conclusion

Supply side responses by the unemployed through search effort may offer an alternative explanation for the observed equilibrium impacts of minimum wage policies. Yet, existing evidence is limited. Using a combination of datasets, machine learning methods, and an event-study approach exploiting 49 minimum wage events over 20 years in the United States, I find that the search effort channel has empirical relevance. Seen through the lens of a standard search model, the minimum wage may reduce market tightness, but the increased search effort by the unemployed acts as a counteracting force leading to a null employment effect in equilibrium. Moreover, the policy increases welfare.

[6] Note that given the empirical exercise, this calibration assumes $f(\theta^{s_1^*}_{MW^1}) = f(\theta^{s_0^*}_{MW^1})$. Nevertheless, since $f(\cdot)$ is decreasing in $s$, we have that $f(\theta^{s_1^*}_{MW^1}) < f(\theta^{s_0^*}_{MW^1})$, and so the result remains true because the assumption just makes the bound looser than it really is.

## Figures and Tables

**Figure 2.1: Precision-Recall Curve**



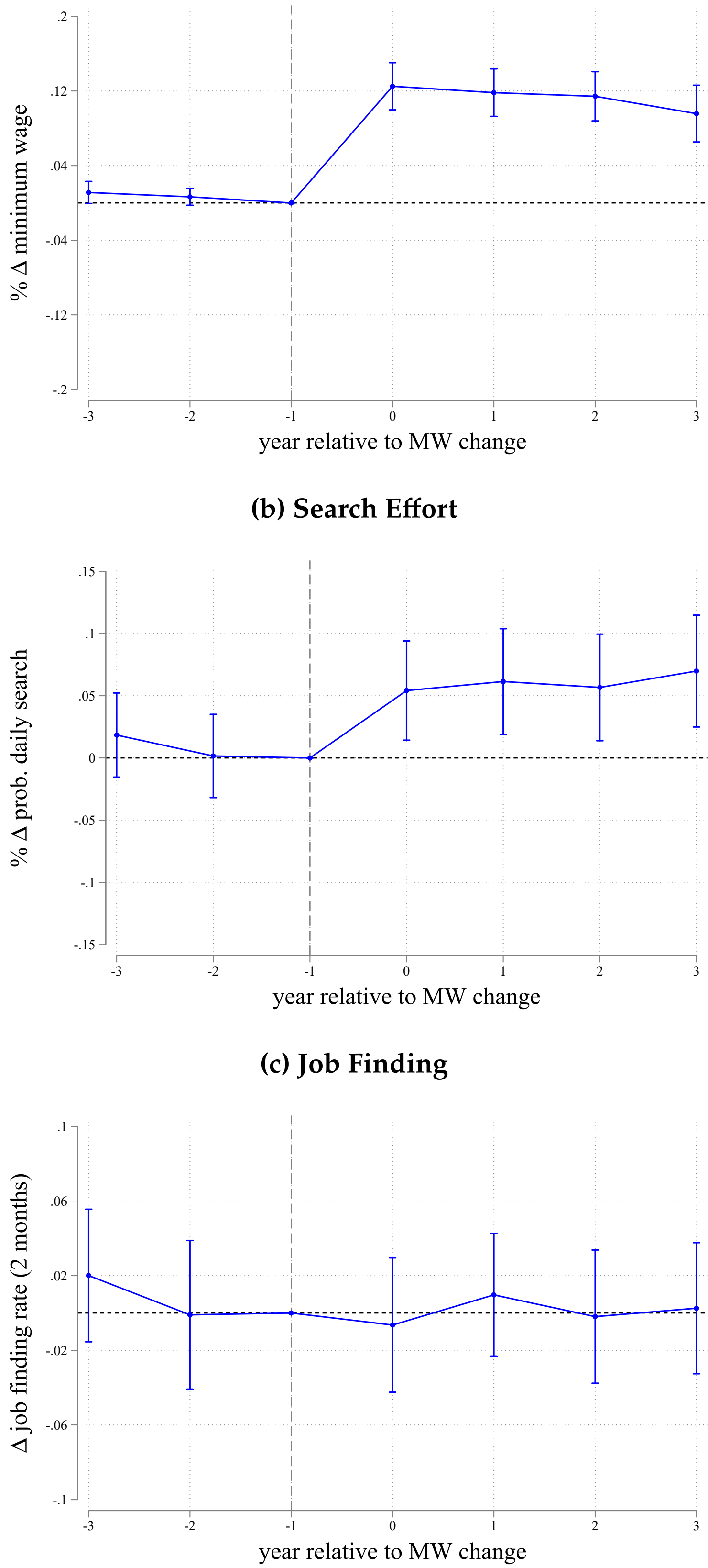

*Note:* This graph depicts the precision-recall curve as explained in Section 2.3.1. The precision rate is the number of minimum wage individuals as a proportion of the number of individuals in a given sample. The recall rate is the number of minimum wage individuals as a proportion of the total number of minimum wage individuals in the population. The graph highlights the key trade off faced when choosing a sample in order to study the impact of the minimum wage: a higher recall will yield a larger sample size but it will be composed of a lower share of true minimum wage individuals.

## Figure 2.2: Minimum Wage and Labor Market Outcomes - Exposed Individuals

### (a) Minimum Wage

### (b) Search Effort

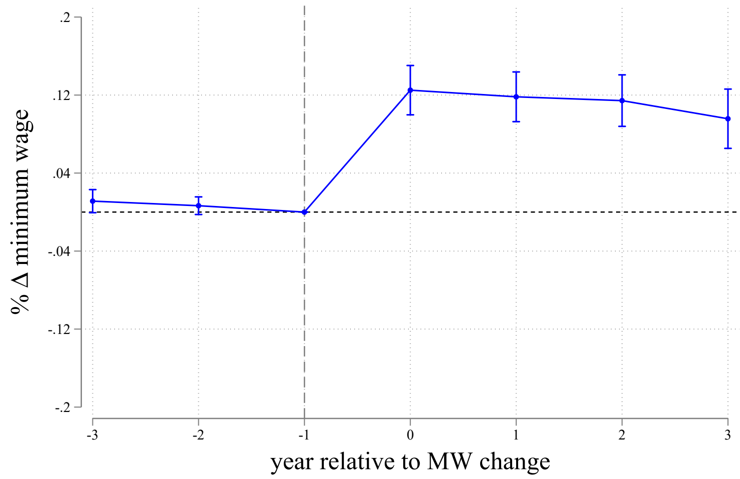
### (c) Job Finding



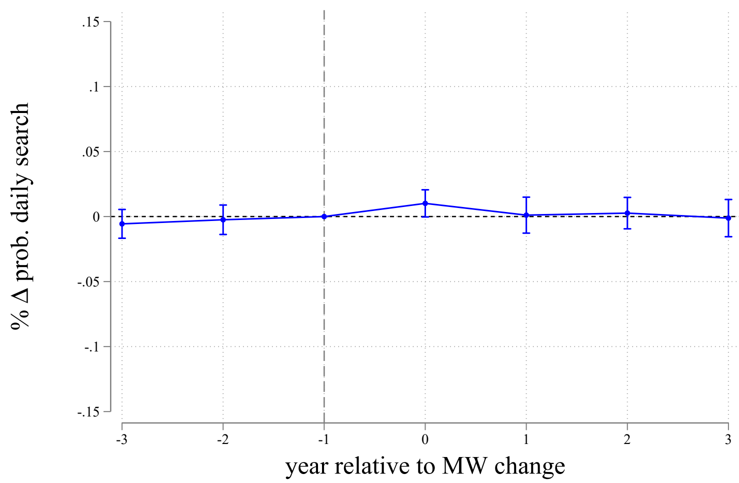

*Note:* This figure shows the impact of the minimum wage on several labor market outcomes, following the stacked-event study specification explained in Section 2.3.3. Panel (a) shows the increase in the statutory minimum wage, Panel (b) shows the impact on the probability of daily search, and Panel (c) shows the impact on the 2-month job finding rate. The sample comprises the group of highly exposed individuals, defined as being above the $90^{th}$ percentile in the distribution of predicted probability of being a minimum wage individual. Point estimates are shown along with 95% confidence intervals.

## Figure 2.3: Minimum Wage and Labor Market Outcomes - Non-Exposed Individuals

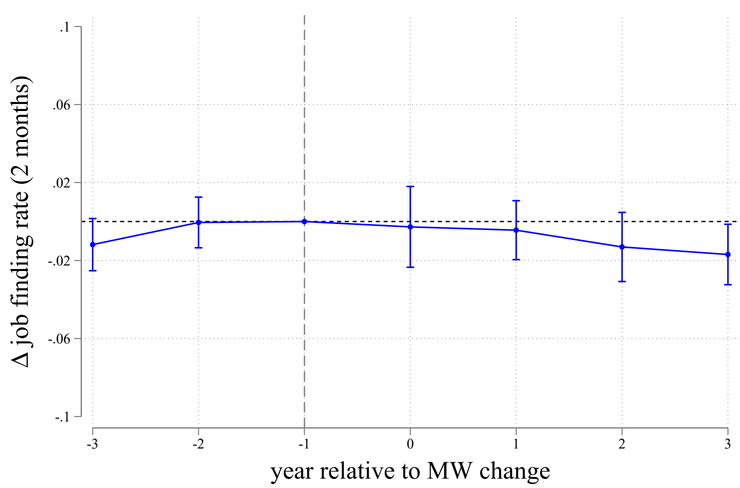
### (a) Minimum Wage




### (b) Search Effort

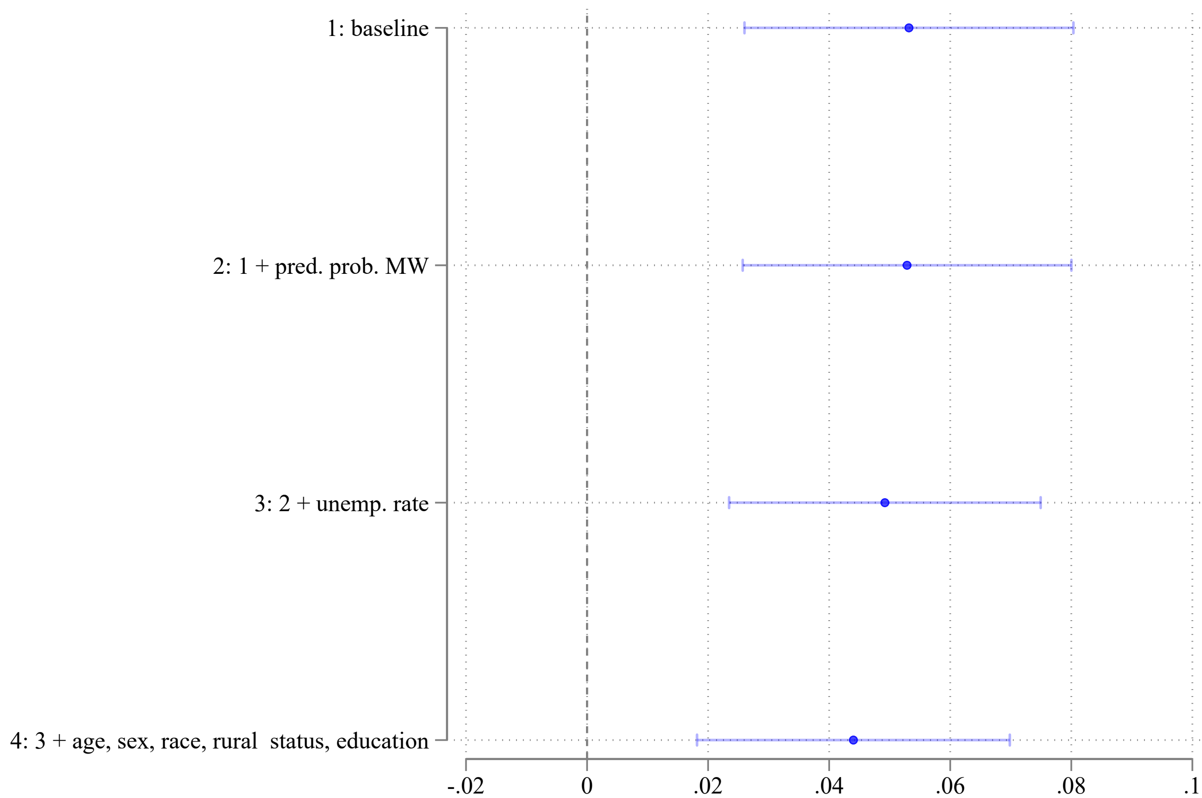



### (c) Job Finding




*Note:* This figure shows the impact of the minimum wage on several labor market outcomes, following the stacked-event study specification explained in Section 2.3.3. Panel (a) shows the increase in the statutory minimum wage, Panel (b) shows the impact on the probability of daily search, and Panel (c) shows the impact on the 2-month job finding rate. The sample comprises the group of least exposed individuals, defined as being below the $90^{th}$ percentile in the distribution of predicted probability of being a minimum wage individual. Point estimates are shown along with 95% confidence intervals.

**Figure 2.4: Effect on Search Effort - Robustness with Controls**




*Note:* This graph shows whether the main (post-reform averaged) estimate from the stacked-event study specification described in Section 2.3.3 is robust to iteratively including different controls. First row shows the baseline specification. Second row adds the predicted probability of being a minimum wage individual. Third row adds the state-level unemployment rate. Fourth row adds several demographic characteristics (age, sex, race, rural status and education).

**Table 2.1: Questions on Search Methods**

| **Questions on search methods in CPS Basic and ATUS** |
|---|
| Contacting an employer directly or having a job interview |
| Contacting a public employment agency |
| Contacting a private employment agency |
| Contacting friends or relatives |
| Contacting a school or university employment center |
| Checking union or professional registers |
| Sending out resumes or filling out applications |
| Placing or answering advertisements |
| Other means of active job search |
| Reading about job openings that are posted in newspapers or on the internet |
| Attending job training program or course |
| Other means of passive job search |

*Note*: The table shows the different job search methods present in the questions of both the ATUS and CPS Basic. These are the questions exploited to construct the main search effort variable as explained in Section 2.3.2.

**Table 2.2: Demographics**

| | **MW individuals** | **Non-MW individuals** |
|---|---|---|
| Precision rate | 0.69 | 0.10 |
| Age | 17.33 | 36.49 |
| High school | 1.00 | 0.53 |
| College | 0.00 | 0.18 |
| Male | 0.54 | 0.54 |
| Rural residency | 0.82 | 0.86 |
| Black | 0.20 | 0.21 |
| Hispanic | 0.19 | 0.18 |
| Married | 0.01 | 0.34 |
| Veteran | 0.01 | 0.06 |
| N | 245,713 | 1,745,085 |

*Note*: The table shows average demographics for the two groups of unemployed individuals: exposed and non-exposed to the minimum wage, defined as being above or below the $90^{th}$ percentile in the distribution of the predicted probability of being a minimum wage individual. The groups are defined after applying the prediction model explained in Section 2.3.1.

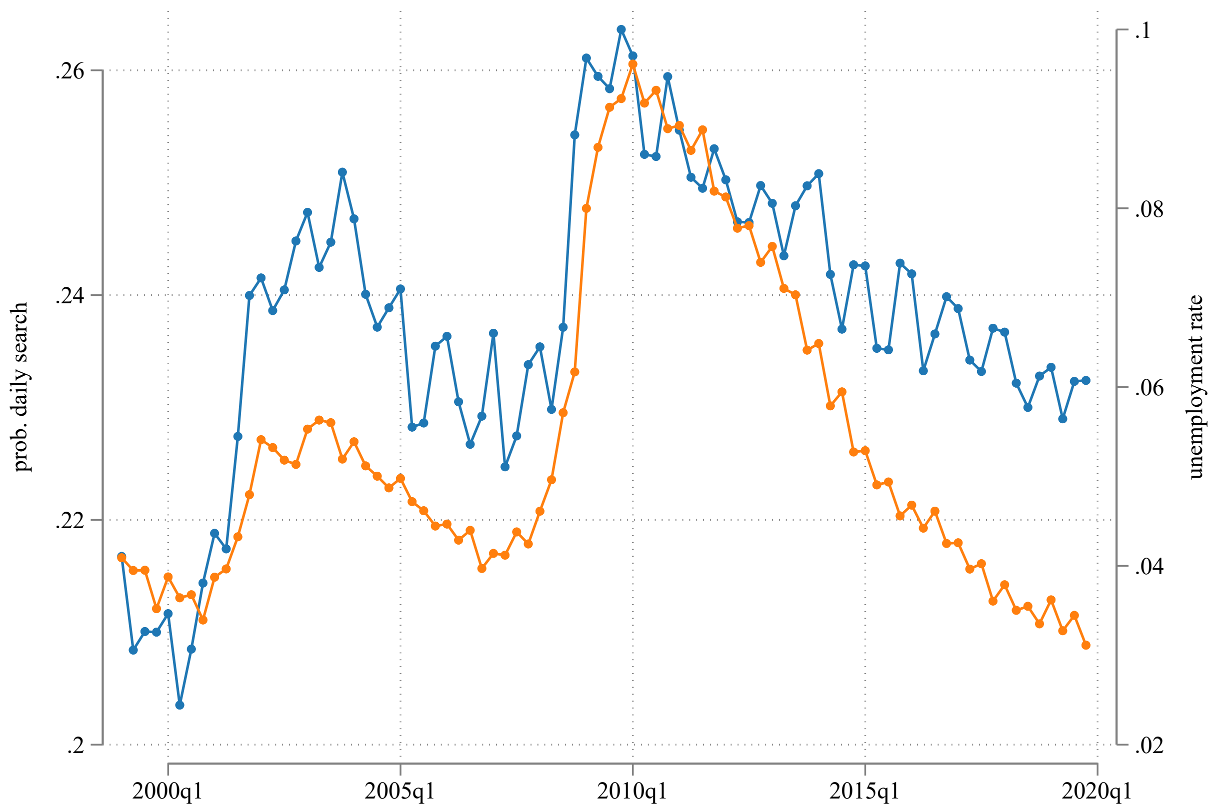
**Table 2.3: IV - Probability of Daily Search**

| | Job finding (2 months) | | Job finding (12 months) | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| | (1) | (2) | (3) | (4) |
| Log(Prob. daily search) | -0.007* | 1.386*** | 0.009** | 1.327*** |
| | (0.004) | (0.385) | (0.004) | (0.534) |
| State x month FEs | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Observations | 185808 | 185808 | 182045 | 182045 |
| First-stage F | | 23.22 | | 11.76 |

*Note*: This Table shows the results on the estimated relationship between probability of daily search and job finding. It is obtained by employing an instrumental variables strategy as described in Appendix 2.B. The OLS columns estimate equation (11), while the IV columns instrument search effort with the interaction between UI eligibility and UI duration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

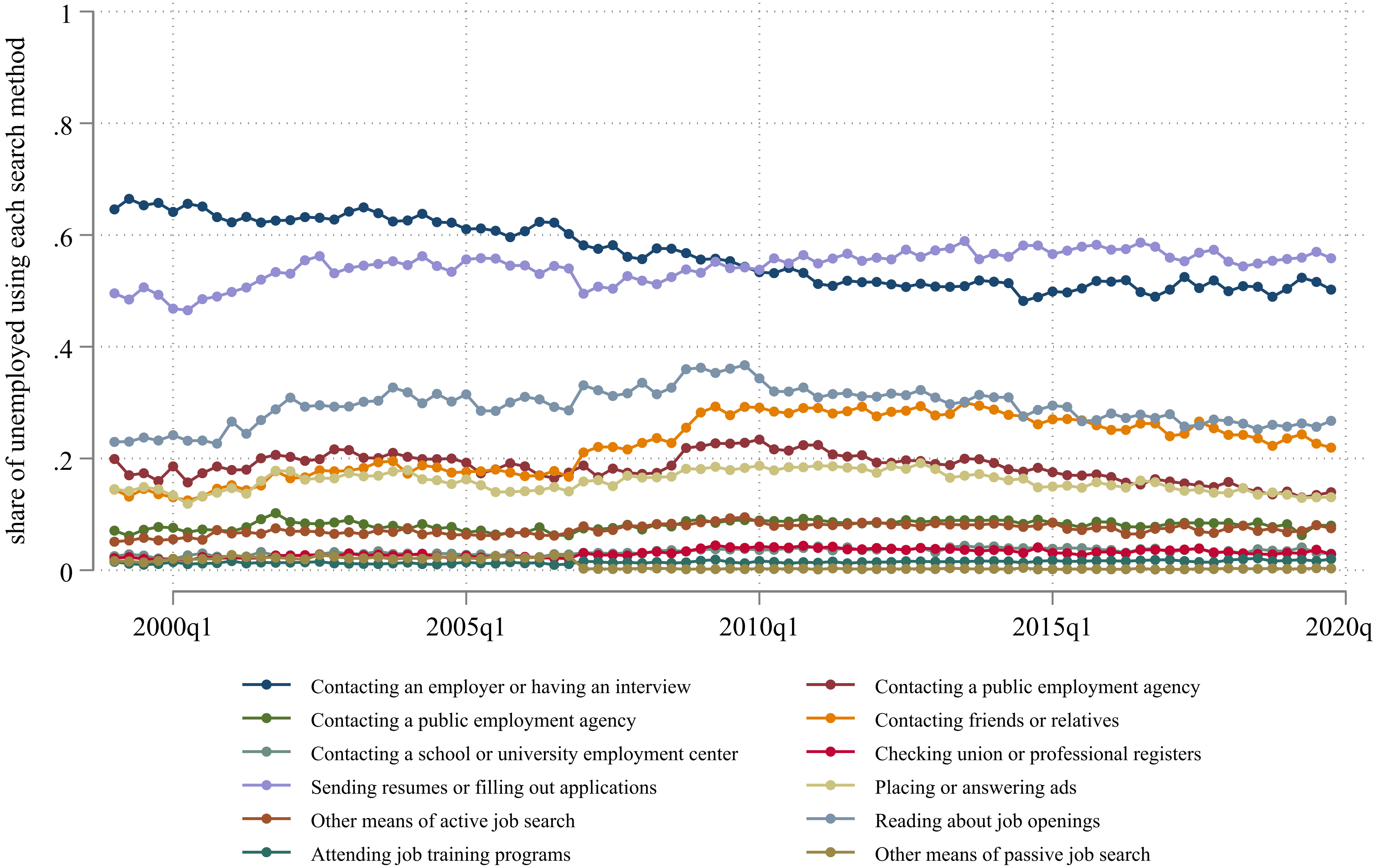
## 2.A Additional Figures and Tables

**Figure 2.A1: Search Effort over the Cycle**




*Note:* This plot depicts the behavior of the main search effort measure—the probability of daily search—along with the national unemployment rate over the years 1999-2019. Search effort is depicted in blue, while unemployment rate is depicted in orange.

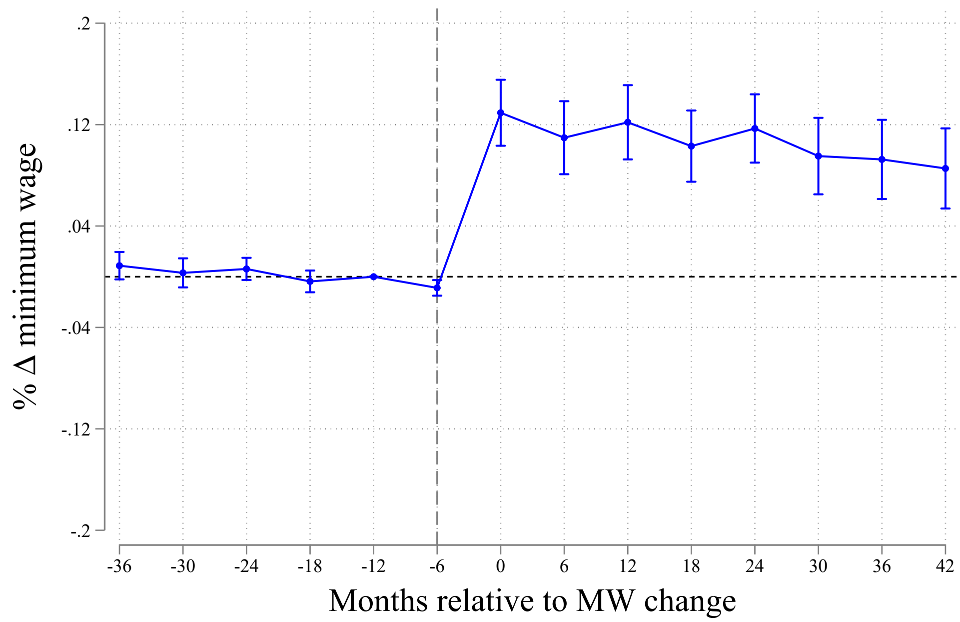
**Figure 2.A2: Job Search Methods over Time**



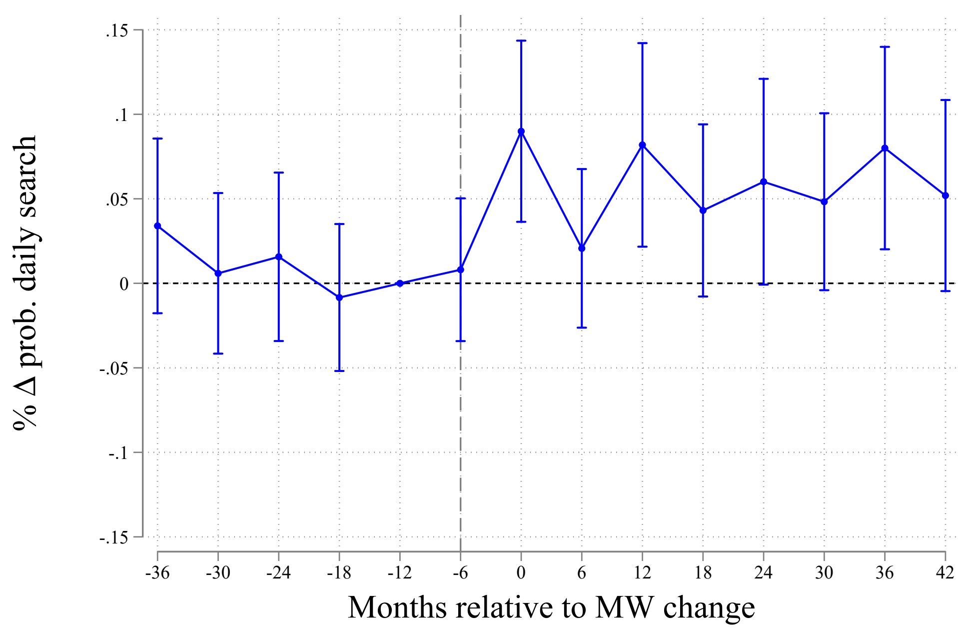

*Note:* This plot depicts the share of unemployed that report using each of the different search methods listed in Table 2.1 over the years 1999-2019.

**Figure 2.A3: Minimum Wage and Search Effort - Exposed Individuals, High Frequency**

**(a) Minimum Wage**

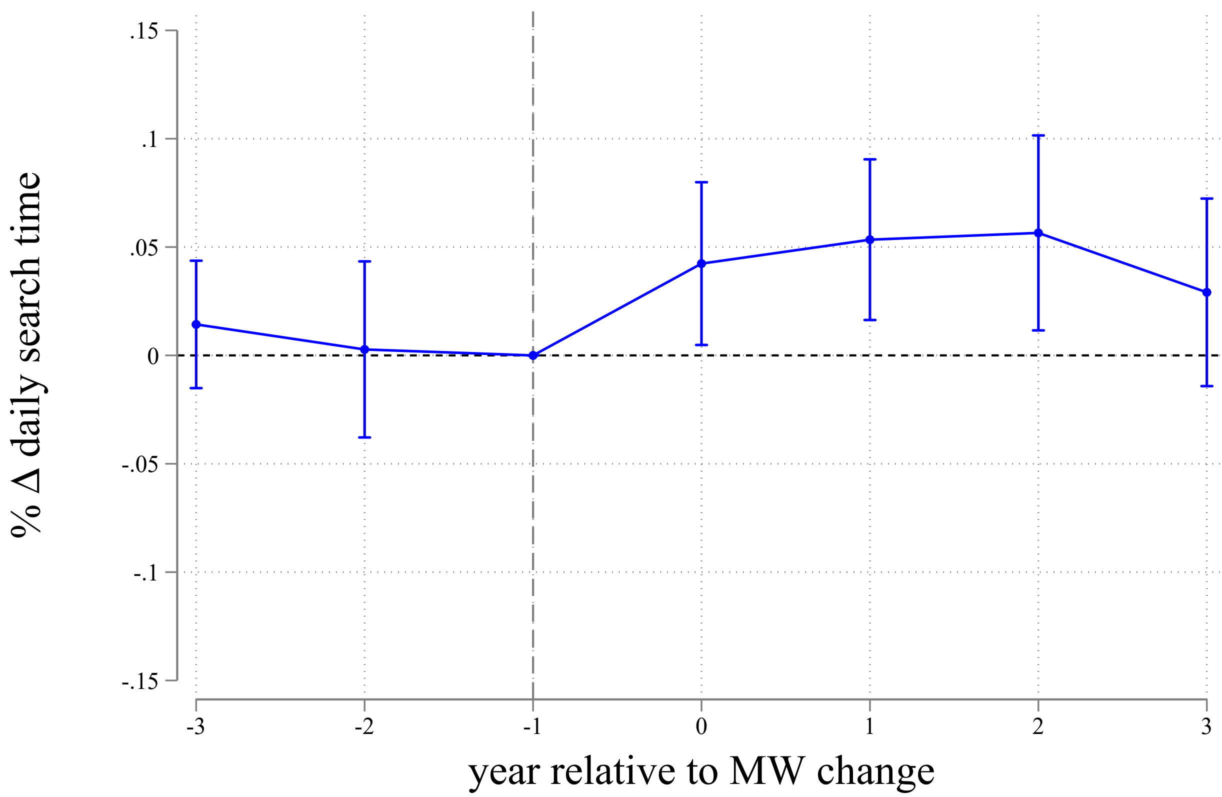



**(b) Search Effort**



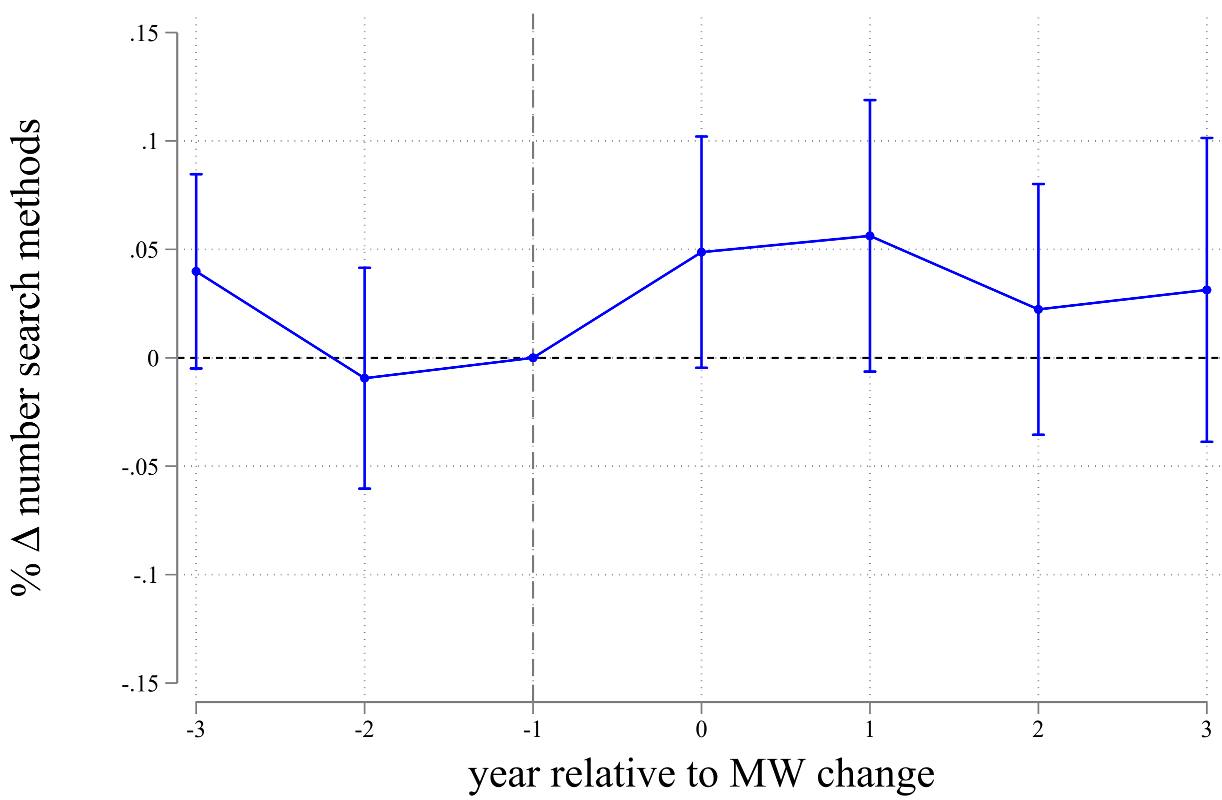

*Note:* This figure shows the impact of the minimum wage on several labor market outcomes, following the stacked-event study specification explained in Section 2.3.3. Panel (a) shows the increase in the statutory minimum wage, and Panel (b) shows the impact on the probability of daily search. The sample comprises the group of highly exposed individuals, defined as being above the $90^{th}$ percentile in the distribution of predicted probability of being a minimum wage individual. The period length here is 6 months, as opposed to 1 year in the main specification. Point estimates are shown along with 95% confidence intervals.

**Figure 2.A4: Search Effort - Daily Search Time**



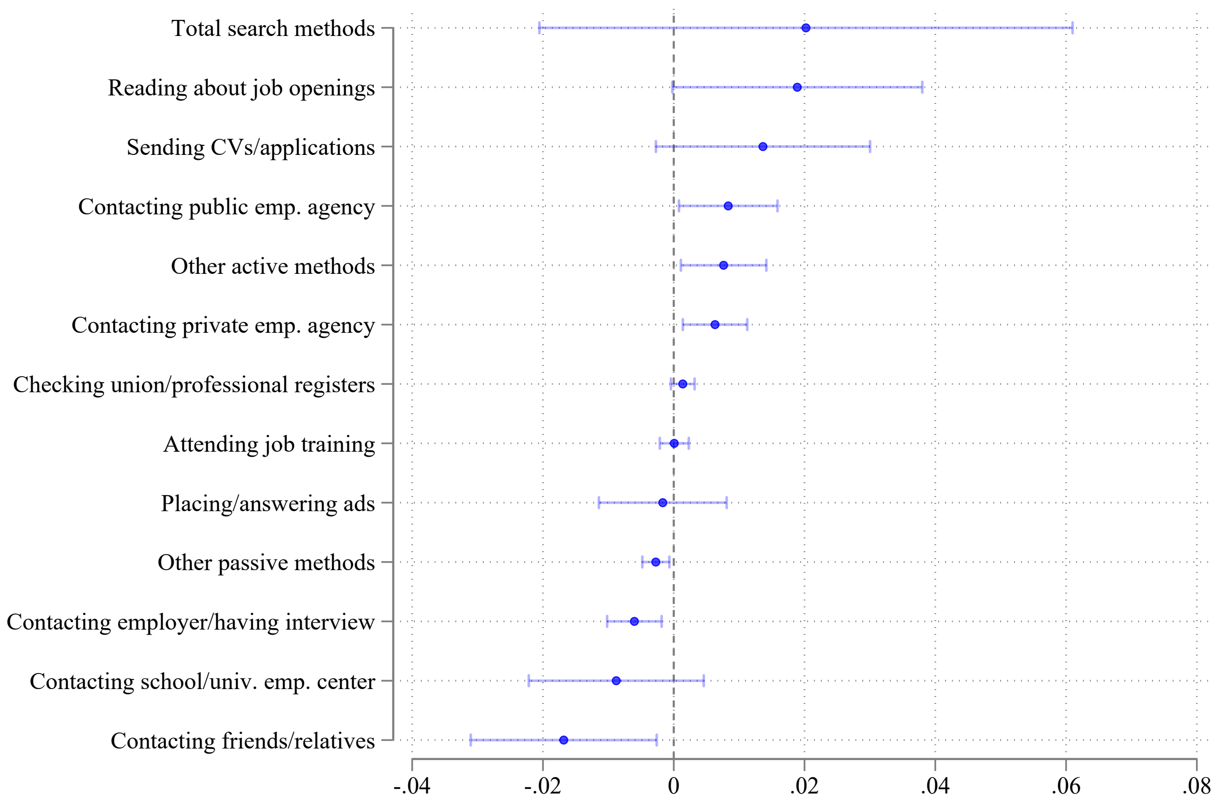

*Note:* This figure shows the impact of the minimum wage on time spent on job search activities, following the stacked-event study specification explained in Section 2.3.3. This search effort model is obtained using a Poisson model for prediction. The sample comprises the group of highly exposed individuals, defined as being above the $90^{th}$ percentile in the distribution of predicted probability of being a minimum wage individual. Point estimates are shown along with 95% confidence intervals.

**Figure 2.A5: Search Effort - Number of Search Methods**




*Note:* This figure shows the impact of the minimum wage on search effort, following the stacked-event study specification explained in Section 2.3.3. This search effort measure corresponds to the total number of methods used by an individual as in Shimer (2004), which only uses CPS Basic information. The sample comprises the group of highly exposed individuals, defined as being above the $90^{th}$ percentile in the distribution of predicted probability of being a minimum wage individual. Point estimates are shown along with 95% confidence intervals.

**Figure 2.A6: Search Effort - Effect by Search Method**




*Note:* This figure shows the impact of the minimum wage on different search methods. Each row is a post-reform average of the effect of the minimum wage on the probability of using each job search method. The sample comprises the group of highly exposed individuals, defined as being above the $90^{th}$ percentile in the distribution of predicted probability of being a minimum wage individual. Point estimates are shown along with 95% confidence intervals.

**Table 2.A1: IV - Daily Search Time**

| | Job finding (2 months) | | Job finding (12 months) | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| | (1) | (2) | (3) | (4) |
| Log(Daily search time) | 0.008*** | 0.913*** | 0.017*** | 0.688*** |
| | (0.003) | (0.232) | (0.003) | (0.213) |
| State x month FEs | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Observations | 185808 | 185808 | 182045 | 182045 |
| First-stage F | | 25.41 | | 30.32 |

*Note*: This Table shows the results on the estimated relationship between time spent on job search activities and job finding. It is obtained by employing an instrumental variables strategy as described in Appendix 2.B. The OLS columns estimate equation (11), while the IV columns instrument search effort with the interaction between UI eligibility and UI duration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

## 2.B Estimating Returns to Search Effort

In order to quantify the effort response term in equation (2.5), we need an estimate of the relationship between search effort and job finding (i.e., $f(\theta) = \frac{\partial h}{\partial s}$). In particular, we would like to estimate the following model:

$$h_{ist} = \alpha + log(s_{ist}) + \beta UIeligible_{ist} + \lambda X_{ist} + \mu_{st} + u_{ist} \tag{11}$$

However, there are multiple endogeneity issues such as measurement error (since my variable is predicted) and omitted variable bias (there can be heterogeneity where high/low job finding individuals search more/less, producing an upward/downward bias). I approach this challenge by using an instrumental variables strategy where the instrument is the interaction between Unemployment Insurance (UI) duration and UI eligibility. The idea is to use Unemployment Insurance duration extensions as a shifter of search effort, and compare individuals within the same labor market to net out market level effects (Lalive et al., 2015). I exploit the fact that within a labor market, some individuals are eligible and some ineligible for UI, so that an UI extension should differentially affect these groups of individuals (i.e., eligible reducing effort relative to eligible as documented by the literature). Importantly, the model includes $\mu_{st}$, which refers to month-state fixed effects so that we effectively compare individuals within the same labor market with different UI eligibility status. The first-stage relationship is:

$$log(s_{ist}) = \pi + \eta UIeligible \times UIduration_{ist} + \tau UIeligible_{ist} + \gamma X_{ist} + \mu_{st} + v_{ist} \tag{12}$$

I use data on UI extensions during the Great Recession from Boone et al. (2021). This strategy requires to have individuals that are eligible and ineligible, so I use all the unemployed individuals, except unemployed on temporary layoffs and job quitters. The results are shown in Table 2.3. I find that a 1% increase in search effort leads to a 0.014 increase in the 2-month job finding. The magnitude for the 12-month job finding is very similar. Alternatively, I also estimate this relationship using daily search time predicted by the Poisson model instead of the main effort outcome, the probability of daily search from the machine learning model. These results are qualitatively similar and are shown in Table 2.A1.

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# Chapter 3

# The Asymmetric Effect of Wage Floors: A Natural Experiment with a Rising and Falling Minimum Wage

*with Emiliano Huet-Vaughn*

## 3.1 Introduction

It is a near truism that minimum wages, if they change, move only in one direction – up. But what happens to firm pay and employment when a wage floor covering all workers suddenly falls?

This paper studies such an unusual scenario, a very large increase in the minimum wage followed by an equivalent, unexpected, and precipitous drop. We document asymmetric responses to the increase and decrease in minimum wage, and leverage this shock to reveal new insights on broader labor market dynamics.

Our main result is novel evidence of wage setting hysteresis from temporary labor policy: the minimum wage increase causes a permanent increase in affected worker pay, with the wage gains being sustained long (5 years) after the precipitating policy has ceased. Our findings also challenge conventional wisdom about the interaction of downward nominal wage rigidity and inflation, specifically, the countervailing impact that inflation is thought to have on real wages in the face of such nominal rigidities (Tobin, 1972). Finally, we investigate the possible mechanisms at play, finding that our evidence is most consistent with the effect the policy shock has on worker reservation wages à la the fairness hypothesis of Falk et al. (2006); we provide the first empirical confirmation of their predictions from a quasi-experimental design that directly mirrors their lab experiment. Overall, the work reveals the value of implementing even transitory labor policy as it can have lasting positive footprints on the earnings of low-wage workers.

Our identification strategy is made possible by the proliferation in the last decade of county and city specific minimum wages across the United States, and, the conflict between different levels of government that has consequently arisen. In Johnson County, Iowa this conflict led to a significant reduction in the statutory minimum wage. Figure 3.1 shows the anomalous recent history of the minimum wage there. In late 2015, the local Johnson government initiated dramatic increases in the minimum wage that unfolded in just over a year - from \$7.25 up to \$8.20 then \$9.15 and finally \$10.10.[1] For 17 months in total, an elevated local minimum wage was in effect in the county (above the \$7.25 federal rate that had otherwise governed in the state) – until the state government suddenly made it illegal for sub-state localities to have their own minimum wage level. As a result, upon the sudden repeal in spring of 2017 the minimum wage in Johnson fell by almost \$3, getting back to the state level of \$7.25. This unique event was followed in the ensuing years by two unusual periods, first a

[1] This total increase of 39% in the minimum wage is far larger than the minimum wage increases studied in most existing work, though, in the last decade research has analyzed some cases of comparably large minimum wage increases (Clemens and Strain, 2021).

Pandemic Recession in 2020, and then a period of high inflation cresting in 2022, that allow us to study the persistence of the minimum wage policy's footprint in the face of first a negative demand shock and then rapidly rising prices.

To examine the labor market effects of this large temporary minimum wage, we employ a synthetic control approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010) where we compare the labor market behavior in the restaurant sector (the leading minimum wage-employing industry) of Johnson County relative to that in a large set of similar counties. To obtain a comprehensive picture, we leverage near-census information on wages and employment at the county level, together with firm-level job posting records.

The main results on wage setting hysteresis from the temporary minimum wage policy are stark. First, the introduction of the higher minimum wage has a direct impact on restaurant wages in Johnson county while the policy is in effect, increasing pay by around 7%. In the first year after the minimum wage is repealed, there is no indication of a wage reduction for affected workers, with wages remaining higher than synthetic Johnson by a similar magnitude as when the policy was in effect. Longer term, we find that the earnings boost endures even five years after the minimum wage reduction, both through the Pandemic Recession and the high inflation of late 2021 thru 2022. While theories of downward nominal wage rigidity (Keynes, 1936; Bewley, 1999) clearly offer an explanation for the short-term persistence of raised wages for Johnson county workers after firms were no longer required to pay them, such theories also predict that inflationary episodes should serve to reduce real wages in Johnson relative to non-treated places, and thus "grease the wheels of the labor market" (Tobin, 1972; Akerlof et al., 1996; Card and Hyslop, 1997). Yet, this is plainly not the case. Further analysis on firms' job posting behavior offers suggestive evidence that employers increased posted wages in response to the policy shock and continued doing so after the policy reversal. While this latter evidence is less precisely estimated, the findings are consistent with not only incumbents enjoying the legacy of higher overall wages from retired minimum wage policy, but, also new workers.

In terms of employment, we do not find a significant change in the number of employed restaurant workers while the elevated minimum wage is in effect. With no jobs immediately lost from the increase, there is, naturally, no "rebound" when the minimum wage is lowered: in the year after revocation there remains no significant change in employment in either an economic or statistical sense. Beginning more than three years post-implementation (and almost two years post-revocation), there is a downward trend in employment which gets more pronounced upon the onset of COVID-19 and its associated public health and demand shocks. A delayed reduction in employment would potentially be consistent with theoretical models of adjustment costs. However, the overall negative estimate of -13.8% is not statistically significant, and additional evidence shows that this potential effect would be, at least in part, explained by other county-level shocks that are orthogonal to the minimum wage policy.

In terms of posted vacancies, we find suggestive evidence of a reduction, consistent with Dube et al. (2016) and Kudlyak et al. (2023), beginning with the implementation of the policy and continuing after the policy reversal, although results are imprecise and cannot rule out a zero impact on vacancies.[2] In comparison to the much clearer wage results, the employment results are thus more nuanced. We think the most reasonable interpretation is one of no contemporaneous or medium term impact of the minimum wage policy on total restaurant employment, with uncertain and only suggestive evidence of a delayed longer term legacy of reduced employment and a reduction in new hires.

To understand the mechanism underlying the wage hysteresis – why are are firms paying such high wages when they no longer legally have to – we consider several possible explanations drawn from the existing literature. Some of these mechanisms share an underlying theme of firm learning from forced experimentation (Larcom et al., 2017).[3] Under this thinking, abiding by the higher minimum wage taught them something about the value to them (in higher revenue/ reduced costs) of paying workers more, which turned out to be more beneficial than they had anticipated. This includes mechanisms that appeal to efficiency wages (Akerlof, 1982; Akerlof and Yellen, 1990); turnover cost reduction due to greater job attachment by employees receiving higher pay (Dube et al., 2016); and, firm hiring of more productive workers (perhaps drawn from neighboring non-Johnson county restaurants or other sectors internally) when they pay a higher wage (Butschek, 2022). Alternatively, firms may not have learned anything new about how to operate more profitably from the temporary minimum wage. Rather, workers may have learned what to expect as a "fair" wage and this permanently changed their reservation wage and future bargaining behavior, consistent with the lab experimental findings in Falk et al. (2006). In such a case, firms continue to offer high wages after the minimum wage falls not because it increases their profits, but, because they realize that they will lose their workers, who have

[2]Note that a reduction in posted vacancies, building over time, may be consistent with an eventual reduction in employment, but, it is not necessarily associated such a reduction as the stock of employees is affected not only by the flow in of employees but also the flow out. Evidence in the minimum wage literature (e.g. Dube et al., 2016) tends to find lower quit rates in response to a higher minimum wage, which could also cause an offsetting reduction in vacancies without an effect on total employment.

[3]Larcom et al. (2017) explores a totally different context - public transit commuter routes - finding that forced disruptions to the commute route lead to replacement of a commuter's sub-optimal (in terms of time spent) commute path with an improved one.

come to demand a higher pay and more of the total surplus, should they choose a lower, now legally allowed wage.

For a separating test to determine which of these mechanisms may be at play, we leverage the wage posting behavior of multi-establishment firms that posted restaurant job vacancies in Johnson prior to the revocation of the raised minimum wage and that also posted such vacancies in another county where their establishment operates with only the federal minimum wage. Mechanisms in the first category above (*i.e.* those that predict learning-by-forced-experimentation and increased firm profit from paying workers more) would expect these firms to some time after 2015 begin raising their wages in their non-Johnson establishments as well, as they learn it profits them to do so. In comparison, those firms outside Johnson that never were exposed to the revelatory, profit-enhancing, elevated minimum wage would not be expected to raise their workers' wages following a policy that they never experienced. In contrast, the reservation wage mechanism would predict the two kinds of firms (those exposed and not exposed to the Johnson minimum wage) should not dramatically deviate post-2015 from the baseline difference in their wage offerings (as there is no profit to be gained by the exposed, or "treated", firms and no need to lift wages for non-Johnson workers who did not experience a shock to their reservation wage). In fact, as we show, the evidence is more consistent with the latter pattern.

This paper contributes to three strands of literature. First, it adds to an increasing body of evidence documenting permanent effects of temporary policies, or, hysteresis - the failure of an effect to reverse itself when its underlying cause has reversed (e.g. Blanchard and Summers, 1986; Charness and Gneezy, 2009; Giné et al., 2010; Bryan et al., 2014; Miller, 2017; Brandon et al., 2017; Ito, 2015; Costa and Gerard, 2021; Saez et al., 2021; Benzarti et al., 2020). In terms of hysteresis induced by labor policy, Goldin and Margo (1992) provide suggestive evidence that the short-lived National War Labor Board impacted wage compression in the decades after World War II, while, to our knowledge, existing quasi-experimental evidence is limited to Saez et al. (2021) who document *employment* hysteresis from an active labor market policy (a payroll tax-cut) affecting labor demand. Our work represents the first quasi-experimental evidence of *wage* hysteresis from temporary labor market policy, specifically, a permanent increase in low-wage worker earnings from a short-run minimum wage increase, persisting more than five years after policy repeal.

Second, it contributes to the macro-labor literature on downward nominal wage rigidity and the phenomena's interaction with high inflation (e.g. Keynes, 1936; Tobin, 1972; Bewley, 1999; Akerlof et al., 1996; Card and Hyslop, 1997; Schmitt-Grohé and Uribe, 2013; Dupraz et al., 2019; Kaur, 2019).

Theories of downward nominal wage rigidity would rationalize the observed absence of a wage drop upon the removal of the Johnson minimum wage because of a constraint firms face in lowering workers' nominal wages. The prevailing view is that when inflation is high, however, firms can freeze nominal pay in order to achieve reductions in real wages and therefore reduce costs. Elsby (2009) traces the argument back to Tobin (1972), summarizing the intuition as follows: "if workers are reluctant to accept reductions in their nominal wages, a certain amount of inflation may grease the wheels of the labor market by easing reductions in real labor costs that would otherwise have been prevented." We contribute by testing empirically how the effects of the policy evolve under different inflation scenarios. And, our finding is novel. While the observed absence in wage change after the policy repeal can clearly be explained by nominal wage rigidity, in contrast to the prior literature, we also document for the first time persistence of the wage differential *even in high inflation periods*.[4]

Third, our paper relates generally to the empirical study of the labor market effects of the minimum wage (e.g. Card and Krueger, 1994; Giuliano, 2013; Cengiz et al., 2019; Azar et al., 2023; Dustmann et al., 2022). Most directly, it relates to the seminal work by Falk et al. (2006), who emphasize the role of reservation wages and fairness concerns for understanding minimum wage impacts. The authors test in the lab the effects of a temporary minimum wage of the type studied in this paper. They show that an increased lab minimum wage positively updates lab workers' reservation wage in a way that persists even after the minimum wage is removed.[5] Responding to this changed reservation wage, firms maintain a permanent increase in wages even after the wage floor falls. Our paper contributes to the literature by providing the first empirical confirmation of this finding outside the lab drawn from a quasi-experimental design that directly mirrors their lab set up. Not only do we confirm their key prediction - that profit-maximizing firms will continue to pay a much higher wage after the removal of a minimum wage than before its introduction - but in distinguishing between alternative mechanisms we find evidence more consistent with the Falk et al. (2006) reservation wage mechanism than other leading mechanisms. Beyond Falk et al. (2006), our work relates to the literature on sub-minimum wages (Freeman et al., 1981; Katz and Krueger, 1992), which refer to policies allowing lower than minimum wage pay to a subset of the labor force such as teenagers or students. These studies find

[4]The 12-month percent change in the US Consumer Price Index was 2.2% in April 2017, the first month after the repeal. During the peak of the high inflation period, it reached 9.1% in June 2022. More consistent with the standard view, Kaur (2019) finds that when inflation is over 6%, prior positive shocks to wages (such as, in our case, the minimum wage increase in Johnson) do not lead to significant increases in real wages (or specifically, she "cannot reject that lagged positive shocks have no impact on current real wages"). Our result suggest otherwise.

[5]In the context of the German minimum wage — a national increase in the legal minimum — Fedorets and Shupe (2021) find partial support for a positive impact of the increase on reservation wages of job seekers, although the effect seems to vanish after a few years. No reduction in the minimum wage is observed in this setting.

generalized low utilization of the sub-minimum wage by employers, raising a similar question as to why profit maximizing firms pay more than legally required in such instances. In these cases, horizontal pay equity concerns may play an important role when, as is common, workers of different ages work together, and so firms may face a constraint when trying to pay differently to workers performing similar work within the firm. We contribute by providing further insights from a case where all firms in the market are allowed to pay less to *all* workers than before the policy, and, thus, this horizontal pay equity constraint at the firm level is not binding in our setting (and so is not a factor in explaining our main result). In light of our findings, we believe that horizontal pay equity considerations, while certainly important, may not be the only explanation for the under-utilization of sub-minimum wages and that reservation wages may be an additional or nesting explanation.

The rest of the paper proceeds as follows. Section 3.2 describes the data and the institutional background. Section 3.3 presents the identification strategy and the main results. Section 3.4 investigates the potential mechanisms and assesses the robustness, and Section 3.5 concludes.

## 3.2 Background and Data

**Institutional Setting.** Though a rarity in the United States, reversals of minimum wage policies have occurred on occasion. Before the establishment of the first federal minimum wage in 1938, some states had already enacted minimum wage policies in the early 1900s, and, were continually taken to court by opponents demanding "freedom of contract." Exceptional cases where a court struck down an active minimum wage include the District of Columbia in 1923 and New York in 1936 (Fishback and Seltzer, 2021). In more recent decades, states have had wide latitude to set their own minimum wages (above the federal rate) without interference, and, top-down reversals of sub-federal minimum wages have happened under circumstances where sub-state jurisdictions (counties or cities, led by Democrats) have set higher local minimum wages and then been challenged by a state government (led by Republicans). To our knowledge, this has happened in 6 other cases outside of Johnson County, Iowa: Louisville KY, Lexington KY, Linn County IA, Wapello County IA, St. Louis MO, and, Kansas City MO. In every one of these instances the local minimum wage increase that was revoked was either not more than a dollar or not not allowed to be in effect for more than a few months (and sometimes for just days).

The case of Johnson County, Iowa is remarkable relative to these other episodes for both the size of the minimum wage increase and the duration. On 1st November 2015, the local minimum wage was raised from the Federal level of \$7.25 to \$8.20. After that, it was further increased twice — on 1st May 2016 and 1st January 2017— reaching \$10.10. It was repealed by the Republican-led state legislature on 1st April 2017, going back to the initial level of \$7.25. See Figure 3.1 for the Johnson County minimum wage schedule over this time period.[6] Given the sharp and prolonged nature of the shock, the episode in Johnson county is the best test case for detecting any immediate or lingering (post-reversal) effects of a minimum wage increase that was actually big enough (almost \$3, or, 39% above the prior state minimum wage) to substantially raise labor costs and binding for long enough (17 months) to plausibly not be perceived as transitory by firms. Press record of the repeal effort by state Republicans indicates that it was only in the last couple of months of the local policy reign, in the beginning of 2017, that the legislature began to craft a repeal, thus, making for a rather sudden and unexpected reversal.[7] Moreover, the large mandated increase to the wage bill enables us to study a situation where adjustment costs or inertia should play a minor role given the sizable costs of inaction (Chetty et al., 2011). Following the reversal by the state, the Johnson county government was left without any legal power to set the minimum wage and instead resorted to publicly imploring firms to keep wages at the high (no longer legally required) Johnson schedule, and, to, in the future, follow a suggested local schedule of increases.

We use two main data sources that allow us to learn about different dimensions affected by the policy.

**Employment and Wages.** We leverage information on employment and wages from the Quarterly Census of Employment and Wages (QCEW) over the years 2011-2022.[8] This dataset contains a quarterly near-census of employment counts and weekly wages for employers subject to unemployment insurance laws (covering more than 95 percent of all U.S. jobs). Our main analysis focuses on information at the county level, and for an important 3-digit low-wage industry: Food Services and Drinking Places (NAICS 722), known to contain one of the largest shares of workers affected by minimum wage increases. We complement the dataset with county-level shares of employment and average wages at

[6]For tipped workers in Johnson county, who at baseline in 2015 had a sub-minimum wage of about 60% of the level for other workers, the schedule of increases in Johnson also moved their minimum wage up dramatically as well with each step in the local increase, keeping their tipped worker wage floor at about 60% of the new broader minimum wage level.

[7]The legislature sent the repeal bill, HR 295, to the Governor on March 29, 2017, and it was signed the next day, and, became effective on April 1. This represented a tight turnaround from introduction to passage, with the bill being first introduced to the legislature for discussion on Feb 14, following a subcommittee review that began on Feb 7 (the earliest legislative record of the effort).

[8]See the Bureau of Labor Statistics county-level QCEW files at https://www.bls.gov/cew/.

the super-sector level, which corresponds to a high-level aggregation of NAICS sectors.[9]

**Job postings.** To learn about the effect of the policy on new jobs and wages, we use job posting data from Burning Glass Technologies (Burning Glass Technologies, 2018) from 2013 thru the middle of 2018. The data covers the near universe of online U.S. job vacancy postings (culled from some 40,000 websites). Burning Glass cleans the data to remove vacancy duplicates and to extract key characteristics for each vacancy. We are primarily interested in the number of vacancies in the restaurant industry (722 NAICS) and in the posted wages for the jobs. While the Burning Glass data is impressive in its scope, it has two limitations for our purpose. First, during much of the time period we are studying the restaurant sector utilized online platforms to search for jobs at a lower rate than other sectors, as documented in Azar et al. (2023), making this data less representative of the overall (online and offline) job postings. Thus, if there are disparities in the volume of new postings or wage rates across online and offline sources, our conclusions will be less representative of the total. Second, within the Burning Glass posting data, the wage information during our time period is particularly sparse, being present for only a little over 4% of adverts in the industry. Still, given the millions of total adverts in the BG data this leaves us with a moderate amount of wage information (over a hundred thousand observations nationally with wage information in the restaurant sector during the time period studied, and, hundreds of individual postings with wage information in a moderately sized county like Johnson). In cases of a posted wage range we use the minimum of that range.

## 3.3 Main Results

In order to understand the labor market effects of the temporary local minimum wage policy, we start by visually inspecting the evolution of average wages around this unique event. As shown in Figure 3.A1, raw average wages in the restaurant sector for our treated county, Johnson, evolved in parallel with the rest of the state of Iowa. When the first minimum wage was enacted, wages in Johnson increased relative to the rest of the state, creating a gap that remained approximately constant until the end of our sample period. This suggests a clear and permanent positive effect of the policy, which we formally investigate below.

[9] Excluding the public sector, we use the following supersectors: Natural Resources and Mining; Construction; Manufacturing; Trade, Transportation, and Utilities; Information; Financial Activities; Professional and Business Services; Education and Health Services; Leisure and Hospitality; Other Services.

**Identification strategy.** We construct our counterfactual by adopting a synthetic control approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010). This method utilizes a data-driven strategy to construct a control county for Johnson as a weighted average of the outcomes of non-treated counties present in the donor pool. For our baseline specification, we use potential donor counties from states that do not require firms to pay more than the federal minimum wage (19 states besides Iowa that have not updated their minimum wage above $7.25 since the last federal change in 2009) and counties in the rest of Iowa with the exclusion of those counties that border Johnson (to exclude places possibly affected by policy spillovers) and three additional counties in Iowa that had in early 2017 also attempted to increase their minimum wage before being stopped by the state.[10] Moreover, we restrict the donor pool to counties in these places with average pre-treatment employment in the sector of interest (restaurants) between 2000 and 11000.[11]

Our main outcomes are average wages and employment in the restaurant industry at the county level. We work with a specification where we normalize both variables by their value in the quarter pre-reform.[12] In terms of predictors, we use averages of both wage and employment for each year in the pre-reform period, together with averages over the whole pre-period of wages and employment shares in each supersector. The inclusion of sector wages and industry shares is motivated by recent literature (Dube and Lindner, 2021) showing the importance of accounting for differences in industry composition and wage distribution.[13] We estimate synthetic counterfactuals at the quarterly frequency, and take annual averages to obtain estimates at the annual level. In relation to inference, we follow the permutation approach proposed by Abadie et al. (2010; 2015), which is based on the rank of the empirical distribution of the ratio of mean squared prediction error (RMSPE). In addition to classic synthetic control estimates, in Section 3.4.2 we also report bias-corrected estimates to account for discrepancies in predictor values between a treated unit and its donor pool units (Ben-Michael et al., 2021; Abadie and L'Hour, 2021), though this does not change our conclusions in any meaningful way.[14]

[10] In Wapello county the local minimum wage was in effect for 3 months before repeal; in Lee and Polk counties it was yet to go into effect when the state law prohibiting local minimum wages became effective law.

[11] Johnson county has a pre-treatment average restaurant employment of around 6700, so this constitutes an approximately symmetric window on both sides of the size of our treated unit.

[12] This specification is similar to the one used by Harasztosi and Lindner (2019); Saez et al. (2019) in their difference-in-differences settings, which helps interpretation and provides better fit when there is large variation in levels in the cross section. It is also equal to McPherson et al. (2023) in the synthetic control context, and related to Doudchenko and Imbens (2016) and Ferman and Pinto (2021). Our results are similar when matching in levels, although somewhat less precise.

[13] Dube and Lindner (2021) show that without such controls they do not get sensible results on the upper tail of the wage distribution, considered a key falsification test (Autor et al., 2016; Cengiz et al., 2019).

[14] For the empirical implementation, we use the `allsynth` package (Wiltshire, 2021).

**Main results.** The estimates of the effect of the temporary policy change on wages are shown in Figure 3.2. The effects in Johnson county are presented in solid green, and the muted grey lines refer to placebo estimates where the non-treated donor counties are compared to their own synthetic versions.[15] For easier visual inspection, in this and later figures, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE). In the year following the introduction of the minimum wage, wages increase sharply by around 7% relative to the control unit. After removal of the legal minimum, the wages remain elevated at roughly the same level in the years immediately following the policy reversal. These positive effects remain unaltered until the end of the sample period, surviving even through the Covid Pandemic, and also through the period of unusually high inflation. Considering the whole post-policy period, from the first full quarter after introduction of the minimum wage until the end of 2022, we find an overall positive effect of 7.62% with a p-value of 0.049. In the final year alone (2022), when inflation is at its peak, the estimate is of similar size and significance (at 9.18% with a p-value of 0.084).

A potential concern when interpreting these results is that they could be driven by a confounding county-level shock that hit Johnson county at the same time as the minimum wage policy. With the aim of alleviating that concern, we also study the evolution of wages for the rest of the economy in Johnson county, excluding the restaurant sector. Intuitively, if the evident effect seen in the restaurant sector is caused by the minimum wage, we should not see any effect emerging over that period outside low-wage sectors. Reassuringly, Figure 3.4a depicts a clear null effect, with wages in the non-restaurant sectors of our treated county following closely those of the synthetic counterfactual.

In relation to the employment effects, results are depicted in Figure 3.3. We do not find statistically or economically significant effects while the policy is in place, during the removal year or even almost two years after removal. Beginning in 2019, the estimates exhibit an insignificant downward trend, with the magnitude becoming more pronounced over time. Averaging over the whole post-policy period there is an overall insignificant employment effect of -13.8% with a p-value of 0.22.[16]

[15] See Table 3.A1 for the specific weights and counties used to build the synthetic counterfactuals.

[16] While there are theoretical explanations that would predict a delayed negative employment response following a permanent minimum wage shock (Sorkin, 2015; Aaronson et al., 2018), existing empirical evidence remains inconclusive in this respect (Meer and West, 2016; Harasztosi and Lindner, 2019). Moreover, note that these models postulate that one-time nominal minimum wage increases, as in our case, are effectively temporary shocks due to inflation, and thus observed long-term responses should be relatively small. Thus, a potential disemployment effect would be consistent with the theoretical models only if the reservation wage mechanism that we have in mind (see Section 3.4.1) actually means a permanent shift in effective real labor cost – regardless of statutory revocation of the minimum wage – making the Johnson-induced wage

The analysis on the evolution of employment for the rest of the economy in Johnson county, excluding the restaurant sector, provides evidence that there are broad-based downward employment trends in Johnson county for reasons having nothing to do with minimum wage policy. As shown in Figure 3.4b, employment in the all sectors exlcuding restaurants also fell gradually relative to the counterfactual over the period after the policy change. The average effect in the period post reform is around -5% (p-value 0.29), and it reaches almost -10% by the last year of the sample. Although this result is not statistically significant, it sheds light on the fact that aggregate downward employment trends were taking place in the county, in a way that was orthogonal to the policy change. This underscores that the statistically insignificant but large negative effect detected in the restaurant sector can be attributed, at least in part, to other county-level shocks.

## 3.4 Mechanisms and Robustness

### 3.4.1 Mechanisms

Why do we see wage hysteresis? What makes firms continue to pay much higher wages to workers after they are no longer legally required to do so? The existing literature offers several possible mechanisms.

One cluster of explanations are those mechanisms that appeal in some way to firms learning from forced experimentation (Larcom et al., 2017). That is, by being required by law to pay a higher minimum wage, firms learn that offering higher pay is actually profitable and continue to do so after the revocation of the requirement. This presupposes some sub-optimizing behavior of firms in the status quo. Explanations of this type are varied. For instance, efficiency wage theories predict greater productivity from existing workers in response to higher wages (Akerlof, 1982; Stiglitz, 1984; Ku, 2022; Coviello et al., 2022). Higher pay is also known to reduce turnover costs due to increased job attachment (Dube et al., 2016) and reduction in time needed to fill vacancies (Cullen et al., 2023). Other work indicates higher minimum wages lead to worker compositional shifts through the hiring of more productive workers (Butschek, 2022). In each of these cases, the mechanism could motivate firms to continue on the elevated wage path even after no longer required, because, again, they learn it is profitable to do so.[17]

Distinct from these mechanisms is an alternative hypothesis. Rather than firms learning something

shock effectively permanent. Though, again, the employment effect is not estimated to be statistically significant here.

[17]Note that a necessary condition for this mechanism to be responsible for sustained higher wages post repeal is that the minimum wage increases profits in the first place. While we cannot investigate this directly, we assess its implications via establishment wage setting in response to the policy.

new about how to operate more profitably, the new minimum wage may have taught workers what to expect as a "fair" wage and this permanently changed their reservation wage and future bargaining behavior, consistent with the lab experimental findings in Falk et al. (2006).[18] In such a case, firms continue to offer high wages after the minimum wage falls not because it increases their profits, but, because they realize that they will lose their workers, who have come to demand a higher pay and more of the total surplus, should they choose a lower, now legally allowed wage.[19]

To develop a test that discriminates between alternative mechanisms we turn to Burning Glass wage posting data in the restaurant industry (NAICS 722). Specifically we look at firms that both operated in Johnson county prior to the revocation of the minimum wage (*i.e.* before the second quarter of 2017) and that also had establishments outside of Johnson county (with firm presence in either place measured by firm posting a job with wage there). Our interest is in the development of their wage postings in their non-Johnson county establishments in comparison to other firms operating outside of Johnson county that never operated in Johnson county prior to the second quarter of 2017. The first group can be thought of as firms "treated" by exposure to the high local minimum wage policy (since some of their stores had to deal with operating under it), while the second group was not exposed and serves as a control.

What do the different mechanisms outlined above predict should happen to the difference in non-Johnson wage postings for treated versus control firms? Under the efficiency wage, turnover cost, and reallocation mechanisms, low-wage firms in Johnson county discover that paying their low-wage workers more yields gains (of various sources) to firms. If firms discover that their Johnson county establishments are less costly or more profitable as a result of the higher worker wage, then the logic of these mechanisms predicts they should raise wages in their non-Johnson firms in order to experience similar gains there. Thus, the posted wage differential in non-Johnson counties should grow between treated and control firms after the minimum wage increase relative to before the policy onset.[20] The

[18]That an increased minimum wage would actually raise workers' expectations about the wage they should expect may not be surprising given the wrong understanding that workers have about their pay options (Jäger et al., 2022).

[19]Note the reservation wage channel has a clear prediction on increased pay after the repeal, however the effects we find through high-inflation periods would also imply that reservation wages are permanently impacted in real, and not only in nominal terms. Driving this could be a natural updating of reservation wages in response to changing prices, or, a change in reservation wages induced by the signal sent through the recommended (evolving) non-binding wage guidelines that have been advertised by Johnson local officials since 2017 (recommendations which local officials and businesses admit, in conversations with the authors, have no teeth and carry only the power of suasion). Regarding this latter channel, Falk et al. (2006) also include a treatment in their experiment with a nonbinding recommended wage guideline that resembles what Johnson county officials have done since they were stripped of their statutory authority. The effects on experimental earnings from the wage guideline in Falk et al. (2006) are similar to the wage floor treatment.

[20]Growth in the gap between treated and control firms would also be consistent with Hazell et al. (2022) if firms are attempting to harmonize pay for workers across establishments (though this explanation would not necessarily involve any learning from forced experimentation about what is profitable).

reservation wage mechanism, by contrast, predicts no such divergence because the treated firms are not experiencing any increase in profit from paying workers more. Rather, they are effectively forced to do so well after 2017, even though it reduces their profit, because of the policy-induced increase in the reservation wage for Johnson county workers. Workers outside of Johnson county have no shock to their reservation wage so there is no need to pay them more and no desire to otherwise since it does not increase profit under this mechanism alone.

Figure 3.5 shows what we observe empirically. It plots the gap in treatment and control firms' lowest posted log wage in non-Johnson counties before and after the increase in the minimum wage in Johnson County, Iowa. Data comes from the rest of Iowa and the nearby states without a state-level minimum wage (Kansas and Wisconsin).[21] The specification includes county, quarter, and employer fixed effects. As the figure reveals, the difference in the log wage between treatment and control firms is stable prior to the 4th quarter of 2015 (when the treated firms were exposed to the elevated Johnson county minimum wage). After exposure to the policy, these treated firms do not seem to change what they pay their workers outside of Johnson county. Averaging across all post-treatment periods, there is a statistically insignificant 0.004 estimate (p-value of 0.89), or, less than half of a percent change in the wage gap. Thus, the reservation wage mechanism is supported and the alternative mechanisms that predict an increase in non-Johnson county establishment wage are rejected.[22] Specifically, we can rule out the possibility that firms are extending the observed 8% increase in earnings experienced in Johnson county to their non-Johnson county establishments, as upper and lower bounds of the 95% confidence interval rule out an approximately 5% or greater reduction and a 5.5% or greater increase in posted wages.

### 3.4.2 Additional Results and Robustness

**Bias-corrected synthetic control.** Our main results reported above were obtained applying a classic synthetic control methodology. Here we also provide bias-corrected synthetic control estimates (Ben-Michael et al., 2021). This procedure, which accounts for discrepancies in predictor values between treated and control units, delivers very similar estimates to the main ones. In terms of wages, as shown in Figure 3.A2a, there is a permanent wage effect of 6.11 percent increase with a p-value of 0.055. In terms of employment, results are also very similar as shown in Figure 3.A2b. This underscores that

[21] Using the whole country as a control leads to poor pre-trends.

[22] To be precise, this finding may result from minimum wages not affecting productivity, or by the potential increase in productivity as a consequence of the policy not being enough to compensate for the increased wage bill (Coviello et al., 2022). Regardless of the specific reason, the evidence indicates that firms do not appear to exhibit sub-optimizing pre-reform behavior.

the findings are robust to the specific synthetic control methodology employed.

**In-time placebo.** As an additional placebo test, we assess the wage effects of a placebo minimum wage increase 8 quarters prior to the actual increase. Figure 3.A3a reports the results for the classic, and Figure 3.A3b for the bias-corrected synthetic control approach. Reassuringly, this exercise shows estimated earnings effects that are very small in magnitude and statistically insignificant, implying that no effects are detected when there is no policy change.

**Posted wages and vacancies.** A key statistic often highlighted to understand the nature of rigidities is the wage of new hires (Pissarides, 2009; Hazell and Taska, 2023). For this reason, we investigate here how this dimension evolves in the context of our natural experiment. Figure 3.A4 presents results on the posted wage for new restaurant industry jobs from the Burning Glass online job posting data. Figure 3.A5 presents results on the number of new job postings in the industry from the same data. The specifications used to generate these Burning Glass results are the same as above with the addition of predictors averaging the posted wage (for Figure 3.A4) and the number of vacancies (for Figure 3.A5) for each year in the pre-reform period; we additionally remove the sector wages and industry shares as predictors since pre-trend fit is poor when they are included. The posted wage results are consistent with new workers benefiting from higher offered wages even after the local minimum wage increase has been repealed (though this is not a statistically significant effect). In terms of the volume of new vacancies posted in the restaurant sector, the pattern is consistent with reduced new hires through 2018, which if continued over time might explain the delayed reduction in employment seen after 2019 in Figure 3.3. Nevertheless, again the reduction in new vacancies seen in Figure 3.A5 is not statistically significant. Overall, we find that these results using Burning Glass data are consistent with the main ones obtained using QCEW, although the lack of precision here calls for caution when interpreting them.

## 3.5 Conclusion

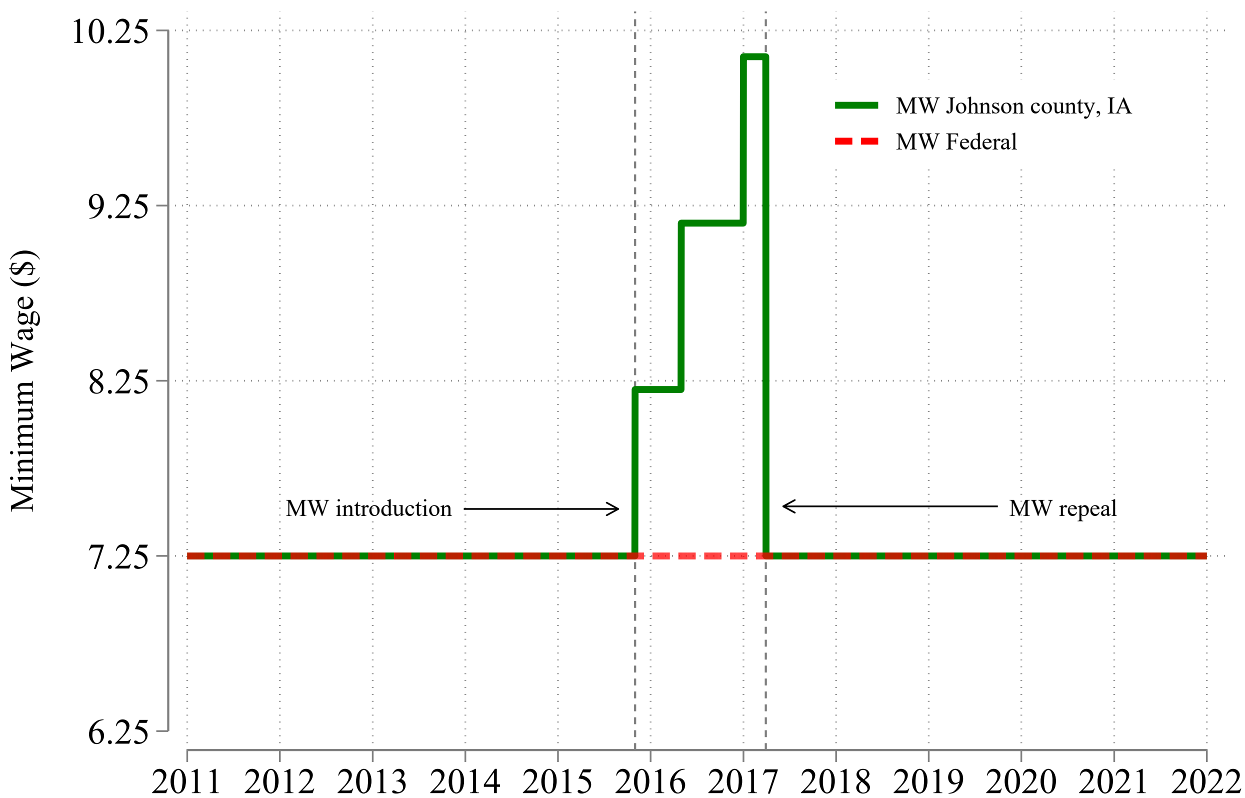
The battle between the minimum wage and inflation has been one on the minds of economists for generations. Stigler, for instance, playfully noted in 1946 that "the minimum wage provisions of the Fair Labor Standards act of 1938 have been repealed by inflation." While this accepted view makes sense, in this work we present evidence that this dynamic is not true in our related context.

We leverage a unique natural experiment for identification, a large increase in the minimum wage followed by an unexpected reversal with a correspondingly large drop in the wage floor. Our main finding is that a relatively transitory policy footprint can cast a very long shadow, with workers experiencing permanent earnings gains five years after the policy ends. This is novel evidence of wage hysteresis from temporary labor market policy. While theories of downward nominal wage rigidity could explain the persistence of raised wages for workers treated by the policy (even when high wages are no longer legally required), such theories predict that inflationary episodes should serve to reduce real wages in treated places relative to non-treated places. Surprisingly, this is not the case: the wage gains persist even in a period of generalized high inflation, with no sign of a negative real wage adjustment, i.e. an erosion of the wage gap that treated workers enjoy relative to unaffected workers. A leading candidate explanation for our results is a model where reservation wages are permanently affected by the temporary minimum wage policy, as in Falk et al. (2006). This model seems to explain the facts better than some alternative theories.

More broadly, our findings provide greater understanding of the dynamics of wage-setting behavior, indicating that equilibrium wages depend on the history of policy changes and not only on current policy - even when it contradicts past policy. For policymakers, especially those operating in environments with divided governments and uncertain policy survival, the upshot of these results is also clear: even transitory labor market reforms can achieve earnings gains for workers that last long after the policy lifetime. In other words, if one has the policymaking reigns for the moment, play it if you've got it.

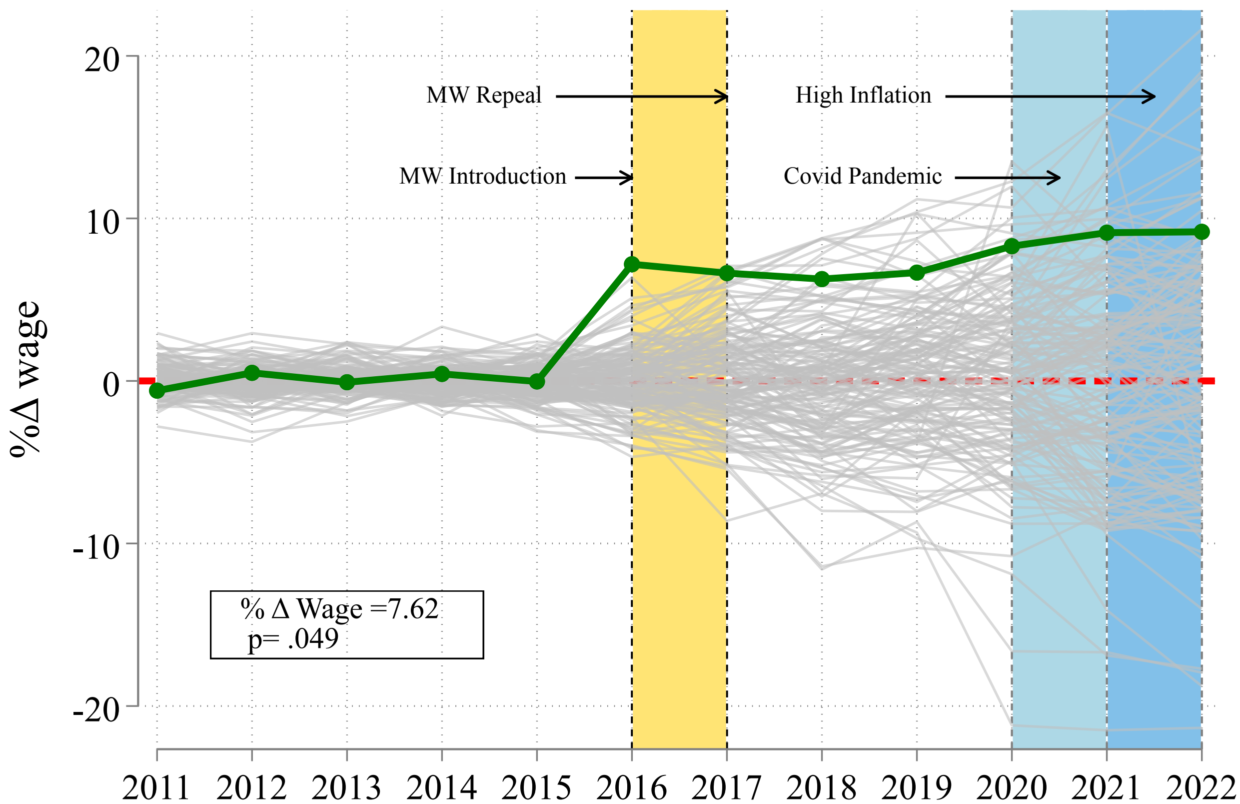
## Figures and Tables

**Figure 3.1: Minimum Wage**



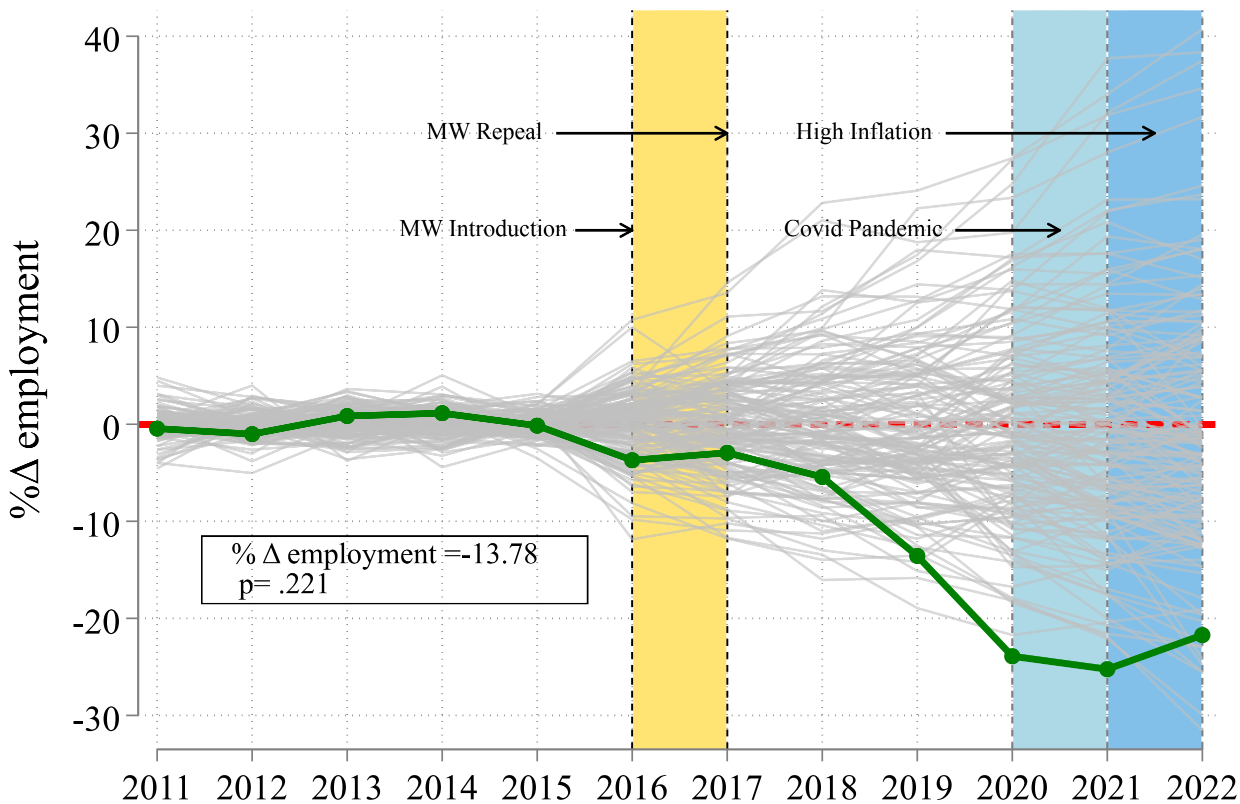

*Notes:* The graph shows the recent evolution of the statutory minimum wage in Johnson County, IA (green line) relative to the Federal minimum wage (red line). The minimum wage was first introduced on 1st November 2015, and then increased twice: on 1st May 2016 and 1st January 2017. It was repealed on 1st April 2017, getting back at the initial level: the Federal minimum wage.

**Figure 3.2: Effect on Wages**




*Notes:* The graph shows the impact of temporary minimum wage policy on restaurant wages in Johnson county, Iowa. The green line depicts the relative gap in average wages in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average wages for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers in Johnson county were legally obliged to pay above Federal minimum wage (see Figure 3.1). The two blue areas correspond to the Pandemic Recession and the high inflation period. The annual outcomes are aggregated from quarterly frequency.

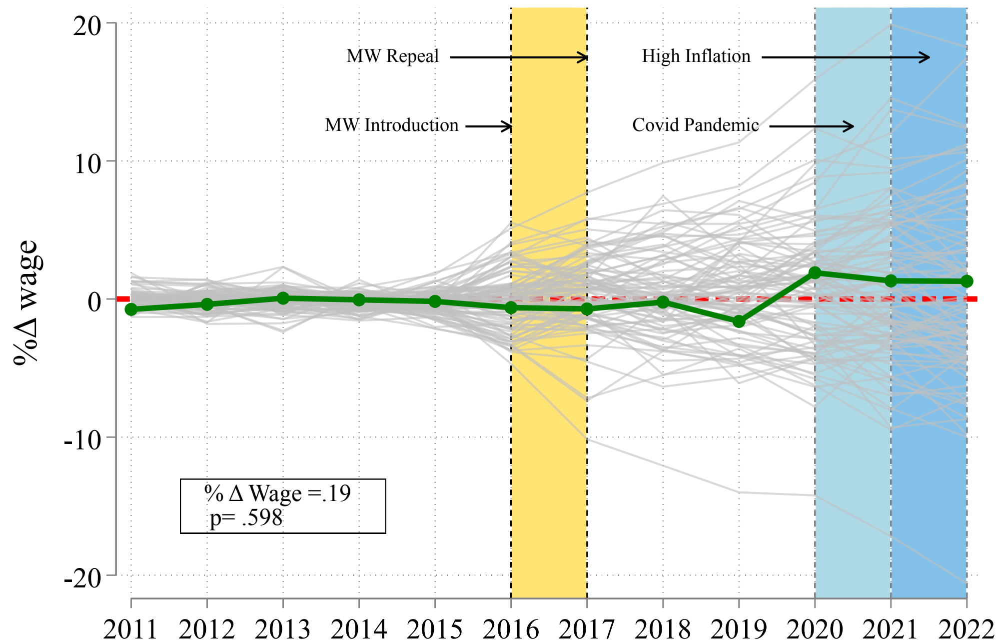
**Figure 3.3: Effect on Employment**



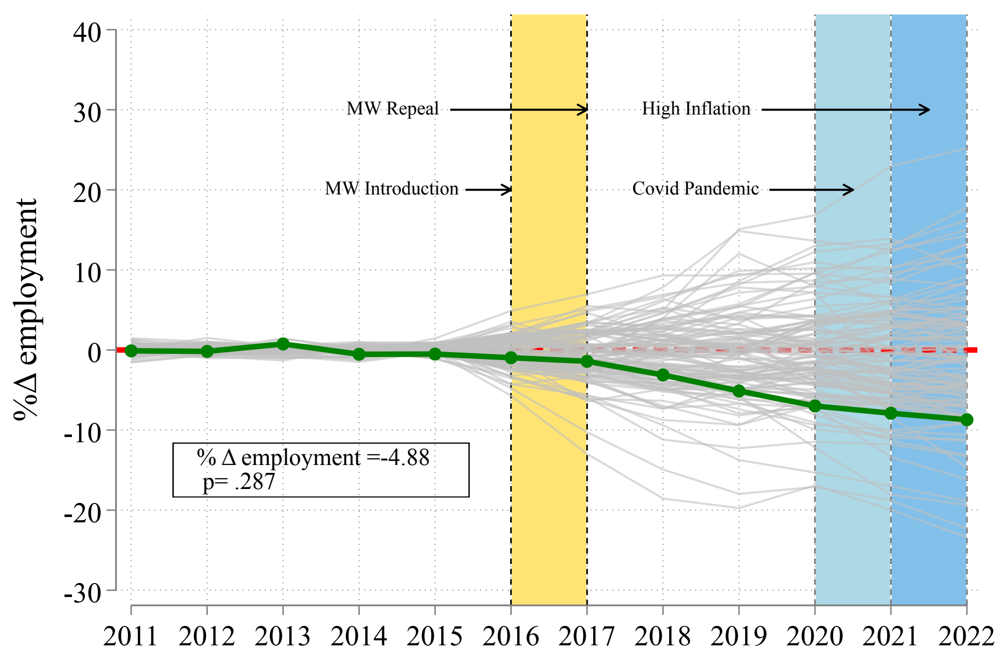

*Notes:* The graph shows the impact of temporary minimum wage policy on restaurant employment in Johnson county, Iowa. The green line depicts the relative gap in average employment in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average employment for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The two blue areas correspond to the Pandemic Recession and the high inflation period. The annual outcomes are aggregated from quarterly frequency.

## Figure 3.4: Effect on Wages and Employment - Excluding Restaurants

### (a) Wage

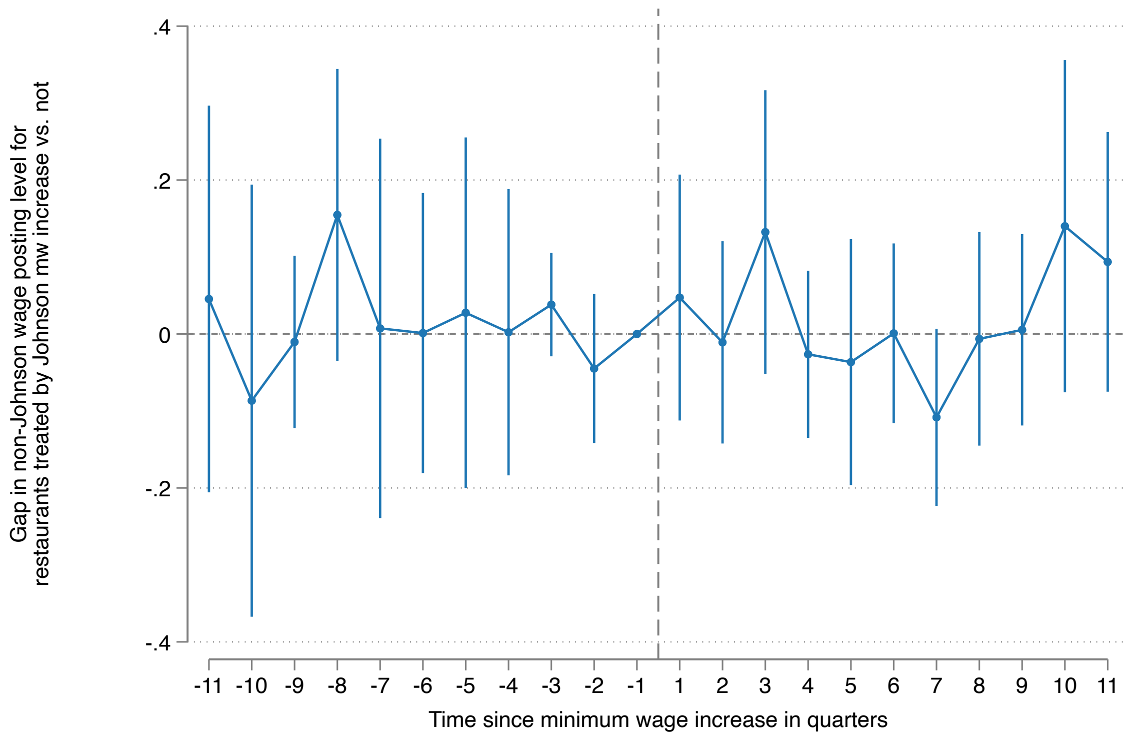



### (b) Employment




*Notes:* The graph shows the impact of temporary minimum wage policy on county-level wages and employment, after excluding the restaurant sector (NAICS 722). Panel (a) refers to the impact of the policy on wages, while Panel (b) depicts the impact on employment. The green lines depict the relative gap in average wage or employment in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average employment for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The two blue areas correspond to the Pandemic Recession and the high inflation period. The annual outcomes are aggregated from quarterly frequency.

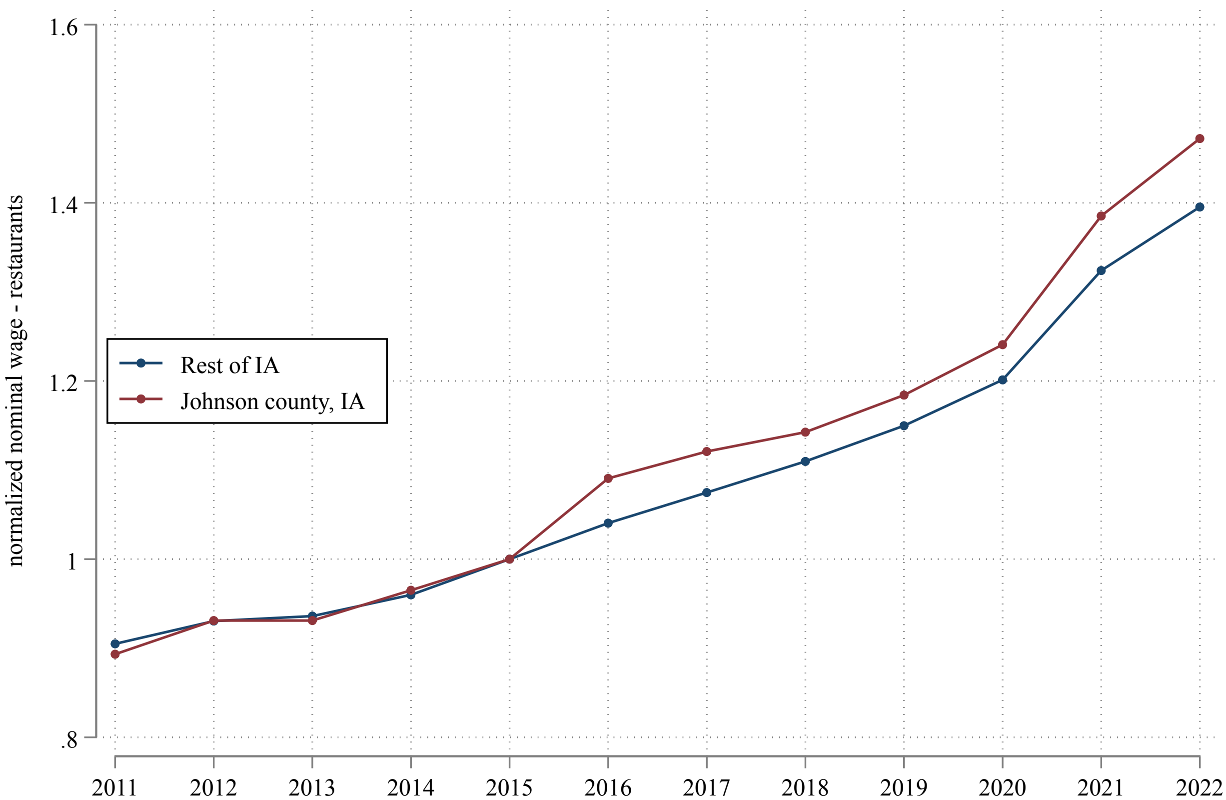
**Figure 3.5: Mandated Minimum Wage Increase for One Establishment Induces no Change in Advertised Pay in External Establishments**




*Note:* The graph shows the development of the average gap in log posted wage for non-Johnson county restaurant jobs between treated and control firms. Treatment is defined by exposure of a firm that operates both inside and outside Johnson county to the Johnson county minimum wage law (i.e. posting of jobs in Johnson prior to repeal of the Johnson minimum wage). Control firms are those that operated outside Johnson county but not in Johnson county before repeal (and thus were never exposed to the Johnson minimum wage). The firms operate establishments in a county in Iowa outside of Johnson or in neighboring states that also have no state minimum wage, Kansas and Wisconsin. The vertical dashed line represents the onset of the local minimum wage in Johnson (it took effect in quarter 1). The difference-in-difference results are scaled relative to the gap in posted wage just prior to the local policy onset (quarter -1). The specification includes county, quarter, and employer fixed effects. Averaging across all post-treatment periods, there is a statistically insignificant 0.004 estimate (p-value of 0.89), or, less than half of a percent change in the posted wage gap of treated and control firms, consistent with the reservation wage interpretation of the mechanism underlying the main wage hysteresis result (see the main text). Under the 95% confidence intervals for this average post-treatment period estimate, we can rule out an approximately 5% or greater reduction and a 5.5% or greater increase in posted wages by treated firms to their non-Johnson establishments.

## 3.A Additional Figures and Tables

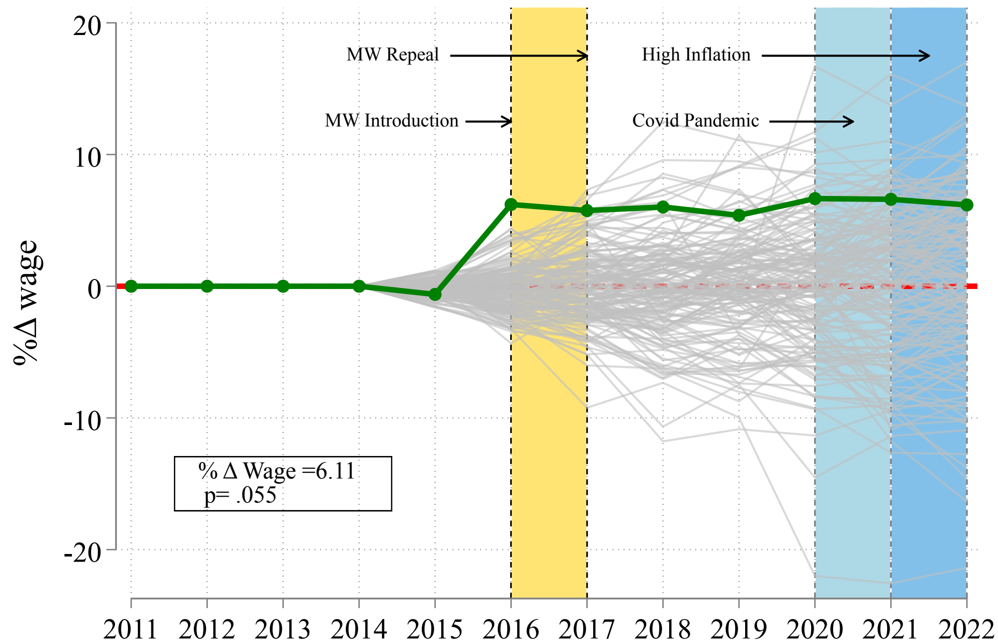
**Figure 3.A1: Average Wages around the Minimum Wage Changes**



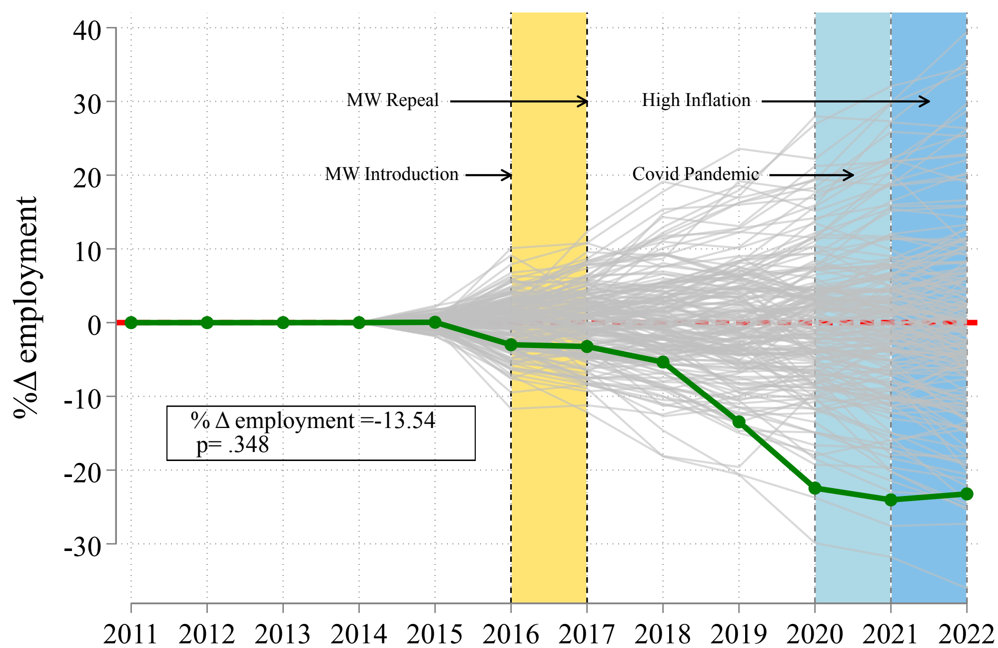

*Notes:* The graph shows the evolution of average wages in the restaurant sector (722 NAICS) from QCEW around the temporary minimum wage policy on restaurant wages in Johnson county, Iowa. The red line depicts the average wages in Johnson. The dark blue line refers to the employment-weighted average wages in the rest of counties in the state of Iowa. Raw averages are normalized to one in the year prior to the first minimum wage increase.

**Figure 3.A2: Effect on Wages and Employment - Bias Corrected SC**

**(a) Wage**



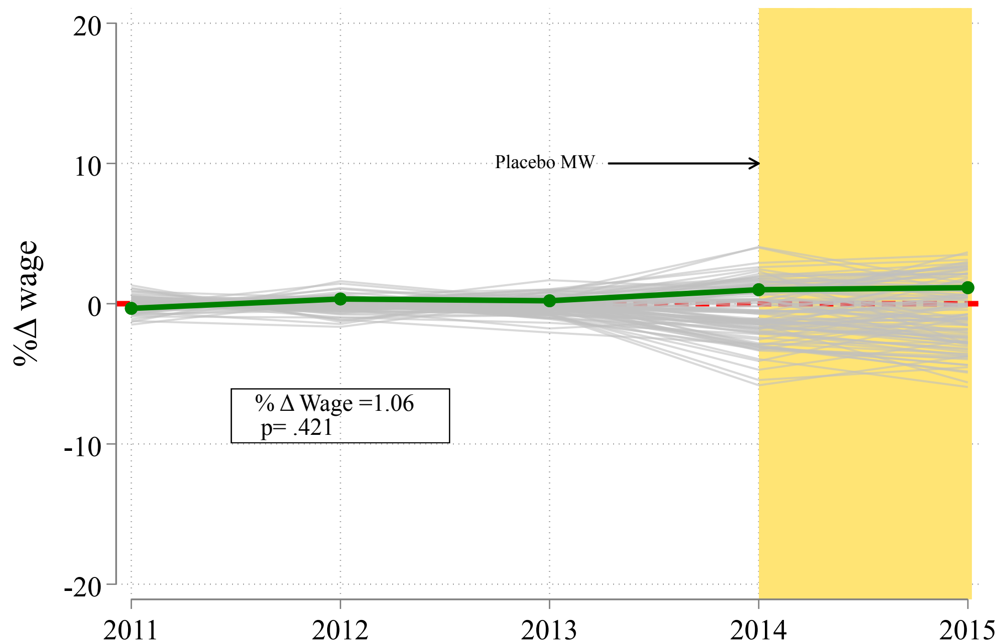

**(b) Employment**



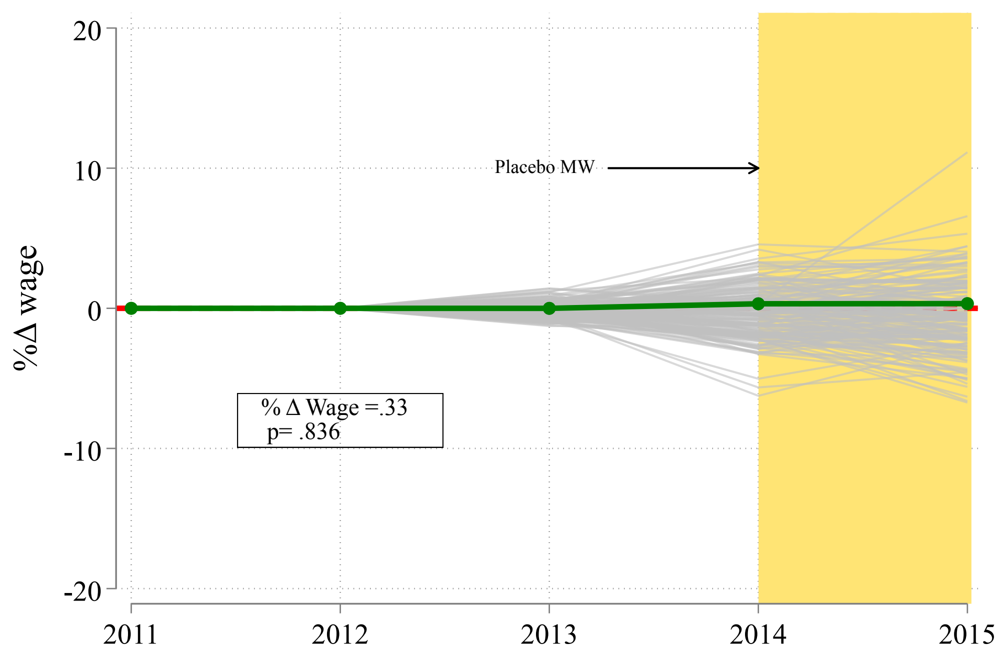

*Notes:* The graph shows the impact of temporary minimum wage policy on wages and employment for the restaurant sector (NAICS 722), using a bias-corrected synthetic control approach (Ben-Michael et al., 2021). Panel (a) refers to the impact of the policy on wages, while Panel (b) depicts the impact on employment. The green lines depict the relative gap in average wage or employment in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average employment for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The two blue areas correspond to the Pandemic Recession and the high inflation period. The annual outcomes are aggregated from quarterly frequency.

**Figure 3.A3: Placebo Minimum Wage Increase in Johnson**

**(a) Wage - Classic Synthetic Control**

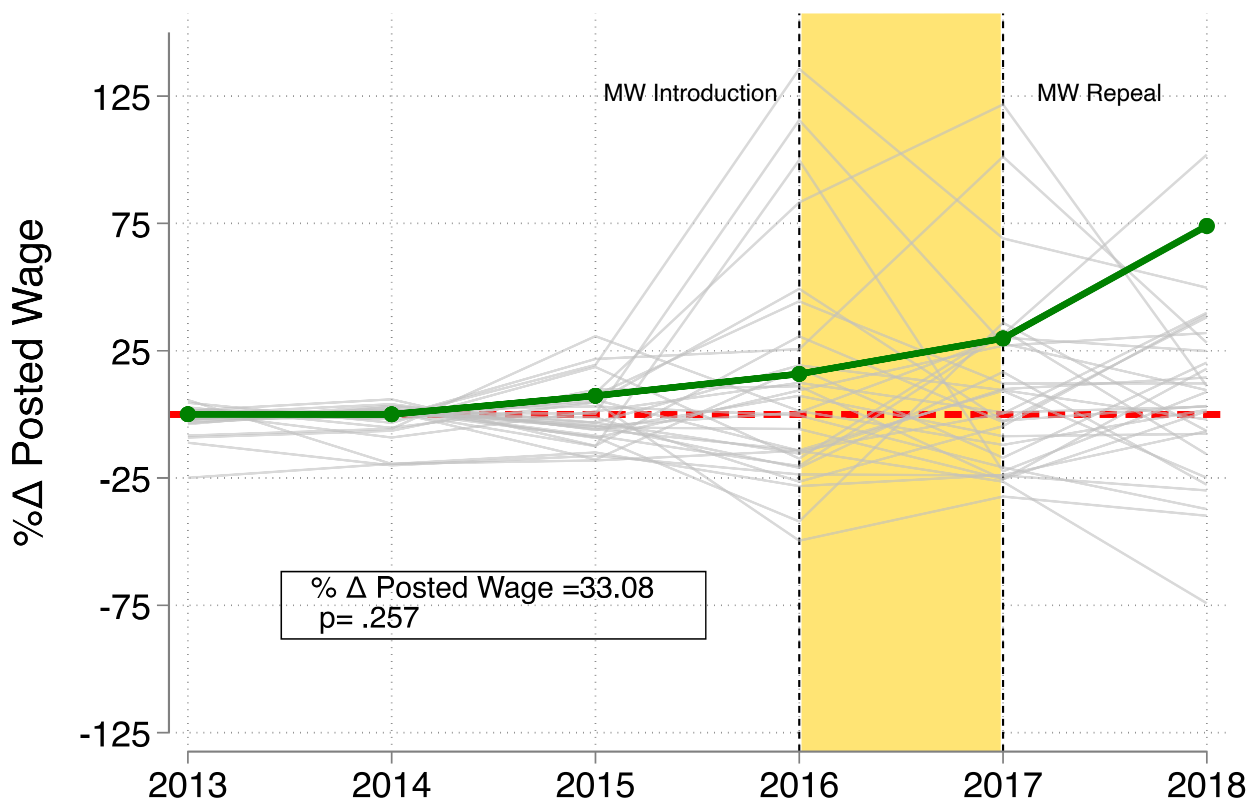



**(b) Wage - Bias Corrected Synthetic Control**



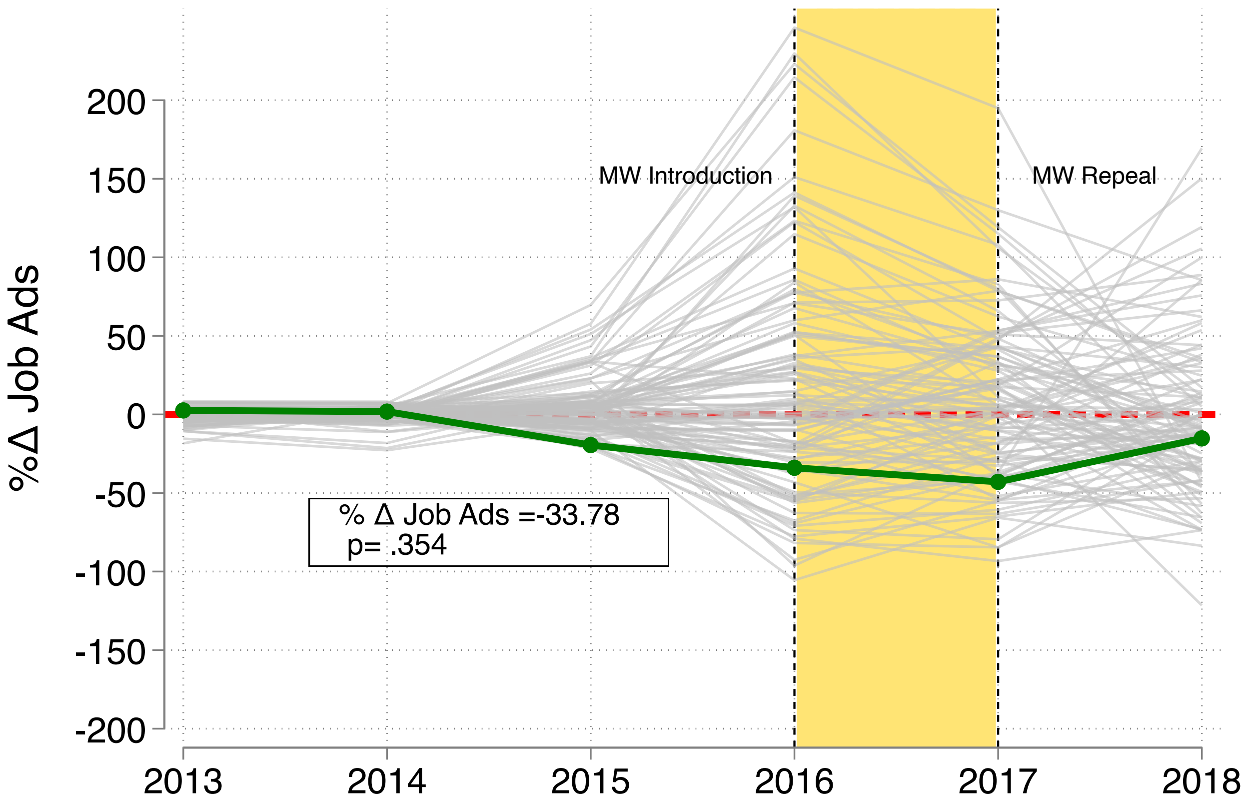

*Notes:* The figure shows a placebo test for an imagined minimum wage increase in Johnson county, Iowa, 8 quarters prior to the actual minimum wage increase. To ensure no contamination from the actual increase in the minimum wage, the analysis is only performed on the quarters prior to the actual increase. Panel (a) reports classic synthetic control wage estimates, and Panel (b) reports bias-corrected wage estimates (Ben-Michael et al., 2021). The underlying specifications follow those used in Figure 3.2, with the following adjustment: pre-reform predictors and donor pool selection is based on the time before the placebo reform. See Figure 3.2 notes for further details.

**Figure 3.A4: Effect on Posted Wages for New Hires**




*Notes:* The graph shows the impact of temporary minimum wage policy on the advertised wage in new online job postings in the restaurant sector in Johnson county, Iowa. Data comes from Burning Glass data that includes wage information (with the lowest posted wage in the wage range used when a range is given). The green line depicts the relative gap in average posted wage in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average posted wage for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The annual outcomes are aggregated from quarterly frequency.

**Figure 3.A5: Effect on Number of Job Postings**




*Notes:* The graph shows the impact of temporary minimum wage policy on online job postings in the restaurant sector in Johnson county, Iowa. Data comes from Burning Glass data. The green line depicts the relative gap in average number of online job ads in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average number of online job ads for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The annual outcomes are aggregated from quarterly frequency.

**Table 3.A1: Donor Counties - Synthetic Control**

***Panel (a): Wage***

| Weight | County |
|---|---|
| 0.362 | Lowndes County, Georgia |
| 0.193 | Bonneville County, Idaho |
| 0.082 | Floyd County, Georgia |
| 0.078 | Bowie County, Texas |
| 0.078 | Cass County, North Dakota |
| 0.076 | Madison County, Mississippi |
| 0.041 | Rutherford County, Tennessee |
| 0.026 | Hendricks County, Indiana |
| 0.025 | Orange County, North Carolina |
| 0.018 | Tangipahoa Parish, Louisiana |
| 0.008 | Webb County, Texas |
| 0.005 | Bell County, Texas |
| 0.004 | Laramie County, Wyoming |
| 0.003 | Dare County, North Carolina |

***Panel (b): Employment***

| Weight | County |
|---|---|
| 0.263 | Bonneville County, Idaho |
| 0.174 | Lowndes County, Georgia |
| 0.142 | Rutherford County, Tennessee |
| 0.096 | Washington County, Tennessee |
| 0.075 | Orange County, North Carolina |
| 0.073 | Maury County, Tennessee |
| 0.057 | Fayette County, Georgia |
| 0.051 | Webb County, Texas |
| 0.048 | Brunswick County, North Carolina |
| 0.015 | Lafayette County, Mississippi |
| 0.005 | Laramie County, Wyoming |

*Note*: The table shows the counties that received a positive weight in the construction of the synthetic counterfactual for the restaurant sector (722 NAICS) of Johnson county, Iowa, as described in Section 3.3. Panel (a) refers to wages and corresponds to the estimates shown in Figure 3.2. Panel (b) refers to employment and corresponds to the estimates shown in Figure 3.3.

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