Essays on Labor Market Institutions

By

Santiago Hermo

B.A., Universidad de Buenos Aires, 2015
M.A., Universidad del CEMA, 2018
M.A., Brown University, 2019

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Date: ________________ ____

Jesse M. Shapiro, *Advisor*

Recommended to the Graduate Council

Date: ________________ ____

Peter Hull, *Reader*

Date: ________________ ____

Lorenzo Lagos, *Reader*

Date: ________________ ____

Neil Thakral, *Reader*

Approved by the Graduate Council

Date: ________________ ____

Thomas A. Lewis, *Dean of the Graduate School*

CURRICULUM VITAE

The author was born in La Plata, Buenos Aires, Argentina, and was raised in Baradero, Buenos Aires, Argentina. He completed his undergraduate studies at the *Universidad de Buenos Aires*, where he worked as a Teaching and Research Assistant. In 2016 and 2017 he undertook a master's program in *Universidad del CEMA*, where he worked as a Teaching Assistant. Before starting his PhD, he worked as a Consultant at the *Instituto Nacional de Tecnología Agropecuaria* in Argentina. In 2018, he entered the PhD program in Economics at *Brown University*.

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I thank my family.

I thank my colleagues.

I thank my committee.

PREFACE

The subject of this dissertation is

In the third chapter, stuff.

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CHAPTER 1

From Workplace to Residence: The Spillover Effects of Minimum Wage Policies on Local Housing Markets

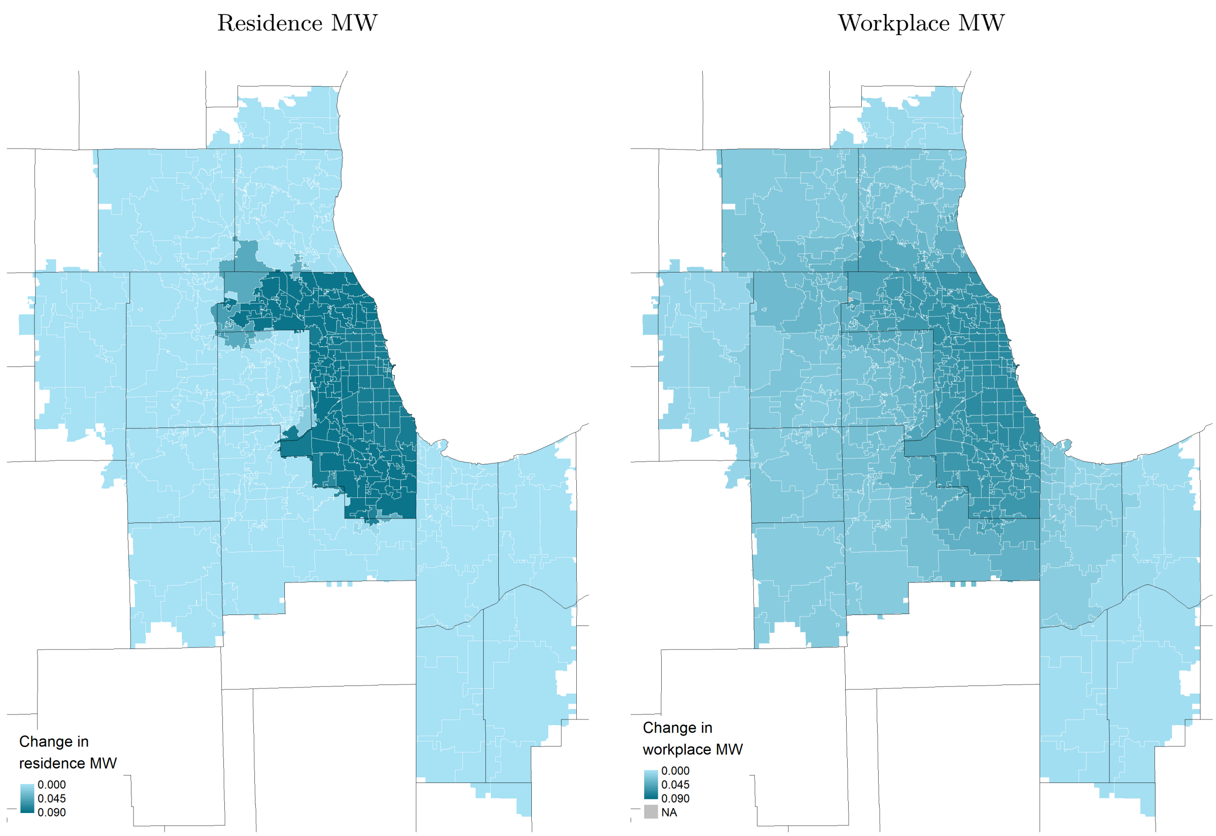
1.1 Introduction

Many US jurisdictions have recently enacted minimum wage policies surpassing the federal level of $7.25, creating considerable variation in minimum wage (hereafter MW) levels across and even within metropolitan areas. These policies are inherently *place-based* in that they are tied to a location, and workers may live and work in locations under different statutory MW levels, suggesting the presence of spatially heterogeneous policy effects. While most research on the effects of the MW has focused on employment and wages irrespective of residence and workplace location (e.g., Card & Krueger, 1994; Cengiz et al., 2019), a full account of the welfare effects of MW policies requires an understanding of how they affect different markets and how their effects spill over across neighborhoods. In fact, while the MW appears to lower income inequality through the labor market (Autor, Manning, & Smith, 2016; Lee, 1999), its overall effect on income for low-wage workers may be smaller if there is a significant pass-through from MW changes to prices, including housing (MaCurdy, 2015).

In this paper, we study the effect of MW policies on local rental housing markets estimating their effects across neighborhoods within a metropolitan area. Consider the introduction of a new MW policy in certain neighborhoods of a metropolitan area in which low-wage workers are more likely to reside outside the jurisdiction that enacted it. The higher MW will cause an increase in the wage income of those low-wage workers that commute to work in the affected neighborhoods, causing a boost in housing demand and rental prices in their residence neighborhoods. This effect, arising from the MW at the workplace, could undermine the distributional objective of the policy. Additionally, the MW hike may affect the jurisdiction that enacted the policy, for instance by increasing prices of non-tradable consumption. This effect, operating through the MW at the residence, will affect the demand for housing as well, and consequently rental prices. Thus, commuting patterns become an essential ingredient to understand the heterogeneous effects of local MW policies.

To operationalize this insight we collect granular data on commuting patterns and construct, for each USPS ZIP code (hereafter ZIP code) and month, the *workplace MW*, defined as the log statutory MW where the average worker of the ZIP code works. We also define the *residence MW*, which is just the log statutory MW in the same ZIP code. Figure 1.1 visually represents these MW-based measures by illustrating their changes for the Chicago-Naperville-Elgin Core-Based Statistical Area (hereafter CBSA) in July 2019, when the city of Chicago and Cook County increased the MW from \$12 to \$13 and from \$11 to \$12, respectively. Even though the statutory MW only changed in some locations in the CBSA, the increase affected the workplace MW of most locations. We formulate a simple partial-equilibrium model that suggests that these measures are sufficient to determine the change in rents in a local housing market.

Figure 1.1: Changes in minimum wage measures in the Chicago-Naperville-Elgin CBSA, July 2019



Notes: Data are from the MW panel described in Section 1.3.1 and from LODES. The figures show changes in the MW measures in July 2019 in the metropolitan area of Chicago. The figure on the left shows the change in the residence MW. The figure on the right shows the change in the workplace MW. The residence MW is defined as the log of the statutory MW of the given ZIP code. The workplace MW is defined as the weighted average of the log of the statutory MW levels in workplace locations of a ZIP code's residents, where weights are given by commuting shares. Smaller colored polygons correspond to ZIP codes, and larger polygons correspond to counties.

Studying the within-city spillover effects of the MW requires granular data on rents, which is why we employ a novel ZIP code-level panel dataset from Zillow. Our main rent variable is calculated as the median rental price per square foot for listings within a specific ZIP code-month for Single Family houses, Condominiums, and Cooperative units (SFCC).[1] This variable captures the posted price of newly available units, thereby avoiding tenure biases and more accurately reflecting current market conditions (Ambrose, Coulson, & Yoshida, 2015). We find that low-wage households are more likely to be renters, tend to reside in these housing types, and that rents per square foot are surprisingly uniform across the income distribution. These findings suggest that the Zillow data can feasibly capture any MW effects. Moreover, the data varies monthly, aligning with the frequency of MW changes, thus allowing us to construct an estimation strategy that exploits the exact timing of hundreds of policy changes staggered across jurisdictions and months.

To estimate the spillover effects of MW policies on rents, we develop a novel difference-in-differences strategy that exploits our granular and high-frequency data to compare the evolution of rents across ZIP codes differentially exposed to workplace MW changes, conditional on the residence MW. To further illustrate the importance of commuting patterns in the propagation of MW shocks, we use our simple model and our main estimated elasticities to evaluate two MW policies: a federal MW increase and a local MW increase in the city of Chicago. We estimate the share of each dollar of extra income (generated by the MW) that accrues to landlords both combining all affected areas and in each particular location. We then discuss our results' implications for assessing the distributional impact of MW policies.

We start by introducing a motivating partial equilibrium model of a ZIP code's rental market, which is part of a larger geography. The model is populated by workers who demand housing, and the interaction with a supply of rental units by absentee landlords determines the equilibrium rental price. Importantly, residents of the ZIP code can commute to work in other ZIP codes, possibly under a different MW policy. Workers' demand for square footage of homogeneous housing space is modelled as a function of prices of non-tradable consumption and income, both of which are influenced by the MW levels at residence and workplace locations. The model illustrates that the impact of a change in MW legislation would vary across ZIP codes, depending on whether it alters the MW at the workplace, the residence, or both. The model implies that the impact of MW changes in certain ZIP codes on rents can be summarized by the workplace and residence MW measures, emphasizing the need to account for the residence MW in the empirical analysis.

Guided by the theoretical model, we pose an empirical model where log rents in a location depend linearly on leads and lags of the workplace MW, the residence MW, ZIP code and time period fixed effects, and time-varying controls. This compares ZIP codes that are differentially exposed to the workplace MW but

[1] Single family houses are standalone housing units, while condominiums and cooperatives are multi-unit buildings with varying ownership structures (Zillow, 2023a).

equally affected by the residence MW, conditional on other factors that affect the evolution of rents. The identification assumption is that, within a ZIP code, changes in the workplace MW are strictly exogenous with respect to unobserved changes in rents after partialing out the confounding variation generated by the residence MW. Given that MW policies are not typically enacted considering their spillover effects on local rental markets, we argue that this assumption is plausible. In an appendix, we discuss a general potential outcomes framework following Callaway, Goodman-Bacon, and Sant'Anna (2021). We demonstrate that, under the assumptions of *parallel trends* and *no selection on gains*, the effects of the workplace MW and residence MW are identified from the conditional slope of log rents with respect to each MW measure.

Our preferred specification implies that a 10 percent rise in the workplace MW (holding constant the residence MW) increases rents by 0.69 percent (SE=0.29). Failing to control for the residence MW results in an estimated effect of 0.45 (SE=0.16), and a model that uses the residence MW only results in an even lower estimate of 0.37 (SE=0.15). The reasons these estimates are lower are two-fold. First, by accounting for the difference between workplace and residence locations, the workplace MW is a better measurement of the change in the MW that is relevant for a ZIP code's wage income, thus reducing measurement error. Second, controlling for the residence MW removes confounding variation in rents that is generated by unobserved factors that may respond to it, such as prices of non-tradable consumption. Using a rough approximation to the share of MW workers in each ZIP code, we show that the elasticity of rents to the workplace MW is larger in locations with more MW residents, consistent with the fact that the effect operates by changing the income of low-wage workers. Likewise, we find a lower elasticity in locations with larger average incomes. These results imply that MW changes spill over spatially through commuting, affecting local housing markets in places beyond the boundary of the jurisdiction that originally enacted the policy.

When including both the workplace and residence MW in our model, we find that the coefficient on the residence MW is negative, consistent with the notion that increases in local non-tradable consumption ameliorate the effect of the MW on rents, as in the theoretical model. However, the coefficient is not statistically significant in our baseline estimates, and the lack of data on prices of non-tradable consumption of a ZIP code's residents prevents us from drawing strong conclusions about this effect.

We provide support for our identification assumptions with a battery of additional analysis. First, we test for pre-period coefficients and construct a non-parametric analysis of the relationship between log rents and the MW measures. We find that future MW changes do not predict rents, and the conditional relationship of log rents with respect to each MW measure is nearly linear, suggesting that the identification assumptions are plausible. Second, we estimate our model using a rental index constructed by Zillow that controls for variation in the available housing stock at each time. This variable alleviates concerns that changes in the composition of available units, coinciding with MW changes, drive our estimates. Third, our estimates are robust to using commuting shares for different years, and they are stronger when we use shares based on jobs below a certain nominal income threshold or on younger workers, both of which are more likely to be affected by the MW. This is consistent with the view that identification arises from the "shares," as in Goldsmith-Pinkham, Sorkin, and Swift (2020). Finally, we construct a "stacked" regression model, similar to Cengiz et al. (2019), that explicitly compares ZIP codes within metropolitan areas where some but not all experienced a change in the statutory MW. This helps alleviate concerns that our estimates stem from undesired comparisons in difference-in-differences models with staggered treatment timing, as highlighted by recent literature (de Chaisemartin & D'Haultfoeuille, 2022; Roth et al., 2022).[2]

Our results remain robust across different sets of controls, alternative samples of ZIP codes, and reweighing observations to match demographics of the population of urban ZIP codes. We find similar (but noisier) results when we use median rents in different housing categories, which are available for smaller samples of ZIP codes.

In the final part of the paper, we use our motivating model and simulate counterfactual exercises to capture the incidence of MW policies on landlords. We compute the share pocketed by landlords in each ZIP code, and also compute the total incidence summing across locations. We simulate two counterfactual MW policies in January 2020, keeping all other MW policies in their December 2019 levels. In the first scenario, we change the federal MW from \$7.25 to \$9. In the second, we propose a rise in the Chicago City MW from \$13 to \$14. We estimate that landlords capture 9.3 cents of each dollar across locations in affected CBSAs in the former, and 11.2 cents of each dollar across locations in the Chicago-Naperville-Elgin CBSA in the latter. We find systematic spatial variation in incidence, with the share pocketed usually being larger in locations that experience an increase in the workplace MW but not in the residence MW. These exercises illustrate that commuting patterns are essential to understanding the spatial incidence of MW policies within metropolitan areas.

Our analysis has some important limitations. First, while our workplace MW measure is consistent with a large body of empirical work relying on variation generated by shift-share instruments (e.g., see recent work by Goldsmith-Pinkham, Sorkin, and Swift, 2020 and Borusyak, Hull, and Jaravel, 2021), our formal justification in the theoretical model relies on strong constant-elasticity assumptions. We discuss their plausibility in the body of the paper. Second, while our model is useful to motivate our empirical strategy, it does not account for general equilibrium effects such as changes in commuting patterns. We

[2]We also estimate a model that includes the lagged first difference of rents as a control, and is estimated via instrumental variables following Arellano and Bond (1991).

discuss the potential consequences of relaxing this assumption for our empirical results in the context of our model. Additionally, we caution that our counterfactual simulations based on the model should be taken as an approximation to the effects of a small change in the MW. Third, our exercises do not capture the full welfare effect of MW policies in the US. Such an effort would require a general equilibrium model that accounts for responses to the MW across several margins. However, as low-wage households are more likely to rent and thus will be more affected by rent effects, our analysis suggest that such a model should consider the homeownership status of households.

Our findings expand our understanding of the effects of MW policies on housing rents by showing how they spill over across local housing markets. To our knowledge, the only papers whose goal is to estimate the effect of the MW on rents in the same location are Tidemann (2018) and Yamagishi (2021). Agarwal, Ambrose, and Diop (2022) studies whether the MW affects eviction probabilities, and presents complementary estimates of the effect of the MW on rents. Our paper also relates to Hughes (2020), who studies the effect of MW policies on rent-to-income ratios and also presents estimates of their effect on rents. None of these papers account for the spatial spillover effects of the MW, which is essential in a context of within-city variation in MW policies, such as the recent experience in the US.

We also contribute to the understanding of place-based policies and the spatial transmission of shocks. Kline and Moretti (2014) argue that place-based policies may result in welfare losses due to finite housing supply elasticites. Allen et al. (2020) estimate the within-city transmission of expenditure shocks in Barcelona. We contribute by, first, showing the MW policies result in rent increases and real income losses for affected workers, and second, by showing that that local MW policies also transmit across space.

More broadly, our paper relates to the large literature estimating the effects of MW policies on employment (see Dube, 2019a and Neumark and Shirley, 2021 for recent reviews of the literature), the distribution of income (e.g., Autor, Manning, & Smith, 2016; Dube, 2019b; Lee, 1999), and the overall welfare effect of the MW (Ahlfeldt, Roth, & Seidel, 2022a; Berger, Herkenhoff, & Mongey, 2022).[3] Our contributions are to incorporate spillovers across locations (as in the recent work by E. S. Jardim et al., 2022) and to show that rent increases erode some income gains of low-wage workers. We also contribute by developing a novel panel dataset of MW levels at the ZIP code level for the entire US.

Finally, our paper relates to work in econometrics that focuses on spillover effects across units, both in the context of MW policies (E. S. Jardim et al., 2022; Kuehn, 2016), and more generally of any policy that spills over spatially (Butts, 2021; Delgado & Florax, 2015). Our approach is similar to Giroud and Mueller

[3] Our paper is also related to work studying the effects of local MW policies (e.g., Dube & Lindner, 2021; E. Jardim et al., 2022), the effect of MW policies on commuting and migration (e.g., Cadena, 2014; Monras, 2019; Pérez Pérez, 2021), and prices of consumption goods (e.g., Aaronson, 2001; Allegretto & Reich, 2018; Leung, 2021).

(2019): we specify an explicit model for spillovers across units that allows us to estimate rich effect patterns of the MW on rents.

The rest of the paper is organized as follows. Section 1.2 introduces a motivating model of the rental market. In Section 1.3 we discuss the empirical relationship between income and housing and present our estimation data. In Section 1.4 we discuss our empirical strategy and identification assumptions. In Section 1.5 we present our estimation results. Section 1.6 discusses counterfactual MW policies, and Section 1.7 concludes.

1.2 A Partial-Equilibrium Model

In this section we lay out a simple demand and supply model of local rental markets. We use the model to motivate our research design and interpret our empirical findings. Specifically, we obtain two results. First, the model shows that a new MW legislation will have a different effect depending on whether it affects the workplace location, residence location, or both. Second, the model shows that under certain conditions the effect of a MW policy on rents can be summarized in two MW-based measures: the workplace MW and the residence MW. Derivations rely on several assumptions, the importance of which is discussed in the last part of this section.

1.2.1 Setup

We consider the rental market of some ZIP code i embedded in a larger geography composed of a finite number of ZIP codes $\mathcal{Z}$. Workers with residence i work in ZIP codes $z \in \mathcal{Z}(i)$, where $\mathcal{Z}(i) \subseteq \mathcal{Z}$. We let L_{iz} denote the number of i's residents who work in z and $L_i = \sum_{z\in\mathcal{Z}(i)} L_{iz}$ the number of residents in i.[4] Commuting shares are given by $\pi_{iz} = \frac{L_{iz}}{L_i}$. We assume that the vector of shares $\{\pi_{iz}\}_{z\in\mathcal{Z}(i)}$ is fixed, which we think is a good approximation for our empirical setting where we observe MW changes at a monthly frequency.[5] We discuss the consequences of relaxing this assumption later in this section.

Minimum Wages. Each ZIP code has a binding nominal minimum wage. The vector of binding MW levels relevant for i is $\{\underline{W}_z\}_{z\in\mathcal{Z}(i)}$.

[4]To simplify, we assume that all of i' residents work, so that the number of residents equals the number of workers.

[5]Allen et al. (2020) study the within-city transmission of expenditure shocks by tourists within Barcelona over a period of two years. The authors maintain an analogous assumption of constant shares of income that each location in the city earns from every other location.

Housing demand. Each group (i, z) consumes square feet of living space H_{iz}, a non-tradable good produced in their residence C_{iz}^{NT}, and a tradable good C_{iz}^{T}. A representative (i, z) worker chooses between these alternatives by maximizing a quasi-concave utility function $u_{iz} = u\left(H_{iz}, C_{iz}^{\mathrm{NT}}, C_{iz}^{\mathrm{T}}\right)$ subject to a budget constraint $R_i H_{iz} + P_i(\underline{W}_i) C_{iz}^{\mathrm{NT}} + C_{iz}^{\mathrm{T}} \leq Y_{iz}(\underline{W}_z)$. In this equation R_i gives the rental price of housing per square feet, $P_i(\underline{W}_i)$ gives the price of local consumption, the price of tradable consumption is normalized to one, and $Y_{iz}(\underline{W}_z)$ is an income function. We specify the effect of MWs on these functions below.

Assumption 1 (Effect of Minimum Wages)**.** *We assume that (i) the price of non-tradable goods is increasing in i's MW,* $\frac{dP_i}{d\underline{W}_i} > 0$*, and (ii) incomes are weakly increasing in z's MW,* $\frac{dY_{iz}}{d\underline{W}_z} \geq 0$*, with strict inequality for at least one* $z \in \mathcal{Z}(i)$*.*

The problem's structure, along with Assumption 1, is aligned with the prevailing literature. First, Miyauchi, Nakajima, and Redding (2021) show that individuals tend to consume close to home. Consequently, it's expected that they would be sensitive to local consumption prices within their own neighborhoods, justifying the assumption that workers consume non-tradables in the same ZIP code.[6] Second, MW hikes have been shown to increase prices of local consumption (e.g., Leung, 2021), and also to increase wage income even for wages above the MW level (e.g., Cengiz et al., 2019; Dube, 2019b).[7]

For convenience, we define the per-capita housing demand function as $h_{iz} \equiv \frac{H_{iz}}{L_{iz}}$. The solution to the worker's problem for each z then yields a set of continuously differentiable per-capita housing demand functions $\{h_{iz}(R_i, P_i, Y_z)\}_{z \in \mathcal{Z}(i)}$. The following assumption summarizes the properties of these functions.

Assumption 2 (Housing demand)**.** *Consider the set of functions* $\{h_{iz}(R_i, P_i, Y_z)\}_{z \in \mathcal{Z}(i)}$*. We assume that (i) housing is a normal good,* $\frac{dh_{iz}}{dY_z} > 0$ *for all* $z \in \mathcal{Z}(i)$*, and (ii) housing demand is decreasing in prices of non-tradable consumption,* $\frac{dh_{iz}}{dP_i} < 0$*.*

Using the first assumption, standard arguments imply that $\frac{dh_{iz}}{dR_i} < 0$. For the second assumption to hold, a sufficient (albeit not necessary) condition is that housing and non-tradable consumption are complements.[8] While direct empirical evidence on this particular channel is lacking, we view the evidence of workers sorting

[6] An extension of the model would allow workers to consume in any ZIP code. While theoretically feasible, this extension would require data on consumption trips, which we lack. We think of our model as an approximation.

[7] An extension would allow separate wage income and business income in the budget constraint. If firm owners tend to live where they work, and MW increases damage profits (as found by, e.g., Draca, Machin, & Van Reenen, 2011; Harasztosi & Lindner, 2019), then business income would depend negatively on the MW level.

[8] To formalize the required condition, let h_{iz} and c_{iz} denote per-capita Marshallian demands resulting from the choice problem, and $\tilde{h}_{iz}$ denote the corresponding Hicksian housing demand. The Slutsky equation implies that

$$\frac{\partial h_{iz}}{\partial P_i} = \frac{\partial \tilde{h}_{iz}}{\partial P_i} - \frac{\partial h_{iz}}{\partial Y_{iz}} c_{iz}.$$

To obtain $\frac{\partial h_{iz}}{\partial P_i} < 0$, we require that $\frac{\partial \tilde{h}_{iz}}{\partial P_i} < \frac{\partial h_{iz}}{\partial Y_{iz}} c_{iz}$, i.e., the income effect of an increase in non-tradable prices is larger than the corresponding substitution effect.

towards locations with high housing costs and more expensive amenities as consistent with it (e.g., Couture et al., 2019).

Housing supply. We assume that absentee landlords supply square feet in i according to the function $S_i(R_i)$, and we assume that this function is weakly increasing in R_i, $\frac{dS_i(R_i)}{dR_i} \geq 0$. Note that this formulation allows for an upper limit on the number of housing units at which point the supply becomes perfectly inelastic.

1.2.2 Equilibrium and Comparative Statics

Total demand of housing in ZIP code i is given by the sum of the demands of each group. Thus, we can write the equilibrium condition in this market as

$$L_i \sum_{z\in\mathcal{Z}(i)} \pi_{iz} h_{iz}\left(R_i, P_i(\underline{W}_i), Y_z(\underline{W}_z)\right) = S_i(R_i). \tag{1.1}$$

Given that per-capita housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.[9] Equilibrium rents are a function of the entire set of minimum wages, formally, $R_i^* = f(\{\underline{W}_i\}_{i\in\mathcal{Z}(i)})$.

We are interested in two questions. First, what is the effect of a change in the vector of MWs $(\{d\ln \underline{W}_i\}_{i\in\mathcal{Z}(i)})'$ on equilibrium rents? Second, under what conditions can we reduce the dimensionality of the rents function and represent the effects of MW changes on equilibrium rents in a simpler way? We start with the first question.

Proposition 1 (Comparative Statics)**.** *Consider residence ZIP code i and a change in MW policy at a larger jurisdiction such that for $z \in \mathcal{Z}_0 \subset \mathcal{Z}(i)$ binding MWs increase and for $z \in \mathcal{Z}(i) \setminus \mathcal{Z}_0$ binding MWs do not change, where $\mathcal{Z}_0$ is non-empty. Under the assumptions of unchanging $\{\pi_{iz}\}_{z\in\mathcal{Z}(i)}$ and Assumptions 1 and 2, we have that*

(a) for any $z' \in \mathcal{Z}_0 \setminus \{i\}$ for which $\frac{dY_{iz'}}{d\underline{W}_{z'}} > 0$, the policy has a positive partial effect on rents, $\frac{d\ln R_i}{d\ln \underline{W}_{z'}} > 0$;

(b) the partial effect of the MW increase in i on rents is ambiguous, $\frac{d\ln R_i}{d\ln \underline{W}_i} \lesseqgtr 0$; and

(c) as a result, the overall effect on rents is ambiguous if $i \in \mathcal{Z}_0$ and positive if $i \notin \mathcal{Z}_0$.

Proofs are available in Online Appendix 1.A.1.

[9] To see this, assume that $S_i(0)/L_i - \sum_{z\in\mathcal{Z}(i)} \pi_{iz} h_{iz}(0, P_i, Y_z) < 0$ and apply the intermediate value theorem. Intuitively, we require that at low rental prices per-capita demand exceeds per-capita supply.

The first part of Proposition 1 shows that, if at least some low-wage worker commutes to a ZIP code z' where the MW increased (so that $\frac{dY_{iz'}}{d\underline{W}_{z'}} > 0$), then the MW hike will tend to increase rents. This follows from an increase in housing demand in i due to the increase in income of workers who commute to z'. The second part of Proposition 1 establishes that decreasing rents may follow if the minimum wage also increases in ZIP code i. This is because the increase in i lowers housing demand by a substitution effect, so that the overall effect on rents is ambiguous. Consequently, the sign of the overall effect of the policy in i is not determined a priori.

As apparent from the proof of Proposition 1, the effect of the MW on rents at workplaces depends on the elasticities of per-capita housing demand to incomes $\xi_{iz}^{Y} = \frac{dh_{iz}}{dY_z}\frac{Y_z}{\sum_z \pi_{iz}h_{iz}}$ and on the elasticities of income to minimum wages $\epsilon_{iz}^{Y} = \frac{dY_z}{d\underline{W}_z}\frac{\underline{W}_z}{Y_z}$. These (i,z)-specific terms weigh the change in MW levels at workplaces, and their sum over z impacts rents. The following proposition establishes conditions under which we can reduce the dimensionality of the rent gradient.

Proposition 2 (Representation)**.** *Assume that for all ZIP codes $z \in \mathcal{Z}(i)$ we have (a) homogeneous elasticity of per-capita housing demand to incomes, $\xi_{iz}^{Y} = \xi_{i}^{Y}$, and (b) homogeneous elasticity of income to minimum wages, $\epsilon_{iz}^{Y} = \epsilon_{i}^{Y}$. Then, we can write*

$$dr_i = \beta_i d\underline{w}_i^{wkp} + \gamma_i d\underline{w}_i^{res} \tag{1.2}$$

*where $r_i = \ln R_i$, $\underline{w}_i^{wkp} = \sum_{z\in\mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_z$ is ZIP code i's **workplace MW**, $\underline{w}_i^{res} = \ln \underline{W}_i$ is ZIP code i's **residence MW**, and $\beta_i > 0$ and $\gamma_i < 0$ are parameters.*

Proposition 2 shows that, under a homogeneity assumption on the elasticities of per-capita housing demand to income and of income to the MW,[10] the change in rents following a small change in the profile of MWs can be expressed as a function of two MW-based measures: one summarizing the effect of MW changes in workplaces $z \in \mathcal{Z}(i)$, and another one summarizing the effect of the MW in the same ZIP code i. This motivates our empirical strategy, where we regress log rents on the empirical counterparts of these measures.

1.2.3 Summary and discussion

Under the stated assumptions, Proposition 1 shows that the effect of a MW increase on rents depends on whether it affects the ZIP code via the workplace or the residence. ZIP codes exposed via their workplace only will tend to experience an increase in rents. ZIP codes exposed to both via their workplace and residence will tend to experience smaller increases, or even decreases, in rents. Controlling for the residence MW is

[10]The assumptions stated in Proposition 2 are actually stronger than needed. It is enough to have that the product $\xi_{iz}^{Y}\epsilon_{iz}^{Y}$ does not vary by z.

thus important to characterize the spatial effects of MW policies. Proposition 2 provides guidance for the empirical analysis of spillover effects of the MW by formally justifying the workplace MW, our summary measure of the changes in the MW at workplaces.

In this subsection we discuss the plausibility of the assumptions that yield these results and the consequences of relaxing them.

The mechanics of rent adjustments. The model is static, in the sense that everyone chooses their housing demand simultaneously. Once a MW policy is enacted, a new equilibrium is reached where rents adjust to the new demand levels to clear the market. This adjustment takes place because workers can in principle move within ZIP codes, and even across ZIP codes, in search of housing that satisfies their demand, as long as the commuting shares remain unchanged. In fact, Agarwal, Ambrose, and Diop (2022) finds an increase in the probability of moving after a MW increase, suggesting changes in housing demand. Online Appendix 1.A.2 discusses an extension of the model with discrete time periods in which the process of workers moving within the ZIP code to renew expiring rental contracts is modelled explicitly. Therefore, we see that rents adjust because workers can demand more or less housing in response to the MW change.[11]

Homogeneity assumptions and the workplace MW. How likely are the assumptions that yield Proposition 2 to hold? The assumption that the elasticity of income to the MW is constant will fail if the income of some (i, z) groups is more sensitive to the MW than others. This would be the case if, for example, the share of low-wage workers within each π_{iz} varies strongly by workplace. The assumption that the elasticity of housing demand to income is constant will hold trivially for preferences $h_{iz} = g\left(R_i, P_i\right) Y_i$ for some $g\left(\cdot\right)$, such as those in Cobb-Douglas or Constant Elasticity of Substitution utility functions. However, one would expect the elasticity of (i, z) groups with many low-wage workers to be larger, suggesting that this type of preferences may not be appropriate.

We thus see that the homogeneity assumptions are strong and will likely not hold exactly. However, we expect our empirical model based on Proposition 2 to offer a reasonable approximation to study the spillover effects of MW policies on the housing market. In fact, unless the heterogeneity in $\{\xi^Y_{iz}\epsilon^Y_{iz}\}_{z\in\mathcal{Z}(i)}$ has a strongly asymmetric distribution across workplace locations, we expect to correctly capture the average contribution of the workplace MW on rents. In other words, the value of $\beta_i d\underline{w}^{\text{wkp}}_i$ is likely to be close to the value of the elasticity-weighted changes in workplace MW levels that, according to the model, determine rents.[12] Moreover, in our empirical exercises we allow for heterogeneity in elasticities based on observable

[11] This is a supply and demand story. Another story that would yield increasing rents as response to workplace MW hikes is one where landlords and tenants bargain over rents, and the MW improves the outside options of tenants.

[12] More precisely, say that $\xi^Y_{iz}\epsilon^Y_{iz} = \overline{\xi\epsilon}_i + \nu_{iz}$ where ν_{iz} has a mean of zero. In that case, a similar logic than the one in the

characteristics of workers, such as the share of MW workers residing in each location, empirically exploring this channel.

Representation under changing commuting shares. Online Appendix 1.A.3 discusses the consequences of relaxing the assumption of fixed commuting shares. In particular, we assume that the commuting shares are declining in the MW level at the workplace (as found by, e.g., Pérez Pérez, 2021). In this case, Proposition 3 in the online appendix shows that MW hikes at workplace locations will affect rents through two channels: a positive channel via increases in housing demand, and a negative channel via decreases in commuting shares. Therefore, potential declines in commuting shares at the month of the MW change will tend to attenuate the positive effect of the workplace MW on rents, biasing our estimates towards not finding an effect. Alternatively, if one thinks that commuting shares adjust more slowly than posted rental prices, then an increase in the workplace MW will tend to lower rents after the MW change. We do not find evidence of this in our estimates, suggesting that the assumption of fixed commuting shares when focusing on monthly changes in rents is reasonable.

1.3 Context and Data

We begin the section by describing the construction of a ZIP code by month panel of MW levels in the US. We use our panel to describe trends in MW policies in the 2010s. Later, we discuss the relationship between income and housing consumption at the household level. The data suggests that rents are likely to respond to MW changes. We also explore how housing expenditure varies across ZIP codes. Finally, we document the construction of our analysis sample and discuss its strengths and limitations.

1.3.1 Minimum Wage Policies in the 2010s

We collected data on federal-, state-, county-, and city-level statutory MW levels from Vaghul and Zipperer (2016). We extended their data, available up to 2016, using data from UC Berkeley Labor Center (2022) and from official government offices for the years 2016–2020.[13] Most ZIP codes are contained within a

proof of Proposition 2 will result in the following expression for rents changes:

$$dr_i = \gamma_i d\underline{w}_i^{\text{res}} + \frac{\overline{\xi\epsilon}_i}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} \sum_z d\ln \underline{W}_z + \frac{1}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} \sum_z \nu_{iz} d\ln \underline{W}_z.$$

The second term on the right-hand side is equivalent to $\beta_i \underline{w}_i^{\text{wkp}}$ in Proposition 2. The third term reflects the heterogeneity. If ν_{iz} has a symmetric distribution, and $d\ln \underline{W}_z$ is the same across workplaces (because it originates from a single jurisdiction), then this third term will equal zero.

[13] Some states and cities issue different MW levels for small businesses (usually identified by having less than 25 employees). In these cases, we select the general MW level as the prevalent one. In addition, there may be different (lower) MW levels for

jurisdiction, and for them the statutory MW is simply the maximum of the federal, state, and local levels. Some ZIP codes cross jurisdictions, and so are bound by multiple statutory MW levels. In these cases we assign a weighted average of the statutory MW levels in its constituent census blocks, exploiting a novel correspondence table between blocks and ZIP codes detailed in Appendix 1.B.1, where weights correspond to the number of housing units. The result is a ZIP code-month panel of statutory MW levels in the US between January 2010 and June 2020. More details on the construction of the panel can be found in Appendix 1.B.2.

Appendix Figure 1.A shows the different levels of binding MW policies over time in our data. Panel A focuses on state-level MW policies. There are 30 states with MW policies in 2010–2020, all of which started prior to January 2010. Panel B shows sub-state MW policies. In total, there are 37 counties and cities with some binding MW policy in this period. The number of new local jurisdictions instituting a MW policy increases strongly after 2013 and declines after 2018. Overall, we observe strong variations in MW levels across jurisdictions.

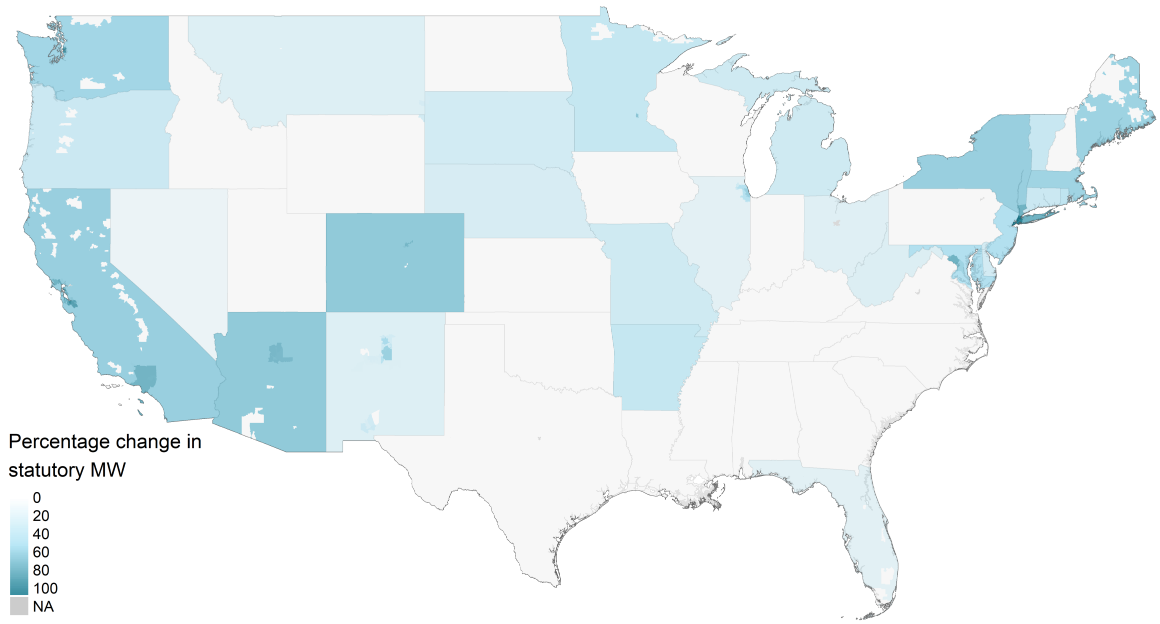
Figure 1.2 maps the percentage change in the statutory MW level from January 2010 to June 2020 in each ZIP code. We observe a great deal of spatial heterogeneity in MW levels within the US. Importantly, many metropolitan areas across and within state borders have differential MW changes, which will be central to distinguishing the effect of the two MW-based measures proposed in Section 1.2. We describe the construction of these measures later in this section.

1.3.2 Households, Income, and Housing

We compare individuals and households within metropolitan areas using data from the 2011 and 2013 waves of the American Housing Survey (US Department of Housing and Urban Development, 2020a). Figure 1.3 shows that low-income households are much more likely to rent. While only 12 percent of households in the top income quintile are renters, around 60 percent of them are when focusing on the bottom one. Appendix Figure 1.B shows that, while low income individuals are less likely to be household heads, many of them are. The average probability for the bottom three income deciles is 50 percent. Appendix Figure 1.C shows that, among households that rent, rents per square foot are surprisingly constant across household income levels. These facts suggest that the MW is likely to affect household income, at least for lower income households, and that rents per square foot can plausibly respond to MW changes. Appendix Figure 1.D shows the type of building households live in by household income decile. Low-income households are more likely to live in buildings with more units, though they are spread across all building types.

tipped employees. We do not account for them because employers are typically required to make up for the difference between the tipped MW plus tips and the actual MW.

Figure 1.2: Spatial distribution of minimum wage changes between January 2010 and June 2020, mainland US



Notes: Data are from the MW panel described in Section 1.3.1. The figure maps the percentage change in the statutory MW level in each ZIP code from January 2010 to June 2020.

We explore variations over space in housing expenditure. To do so, we collected Individual Income Tax Statistics aggregated at the ZIP code level from the IRS (Internal Revenue System, 2022b), and Small Area Fair Market Rents (SAFMRs hereafter) data from the HUD (US Department of Housing and Urban Development, 2020b).[14] For each ZIP code in 2018, we constructed a housing expenditure share dividing the 2 bedroom SAFMR rental value from the HUD by the average monthly wage per household from the IRS.[15,16] Appendix Figure 1.E maps our estimates for the Chicago CBSA. There is considerable variation in housing expenditure over space, with poorer areas generally spending a higher share of their income on housing.

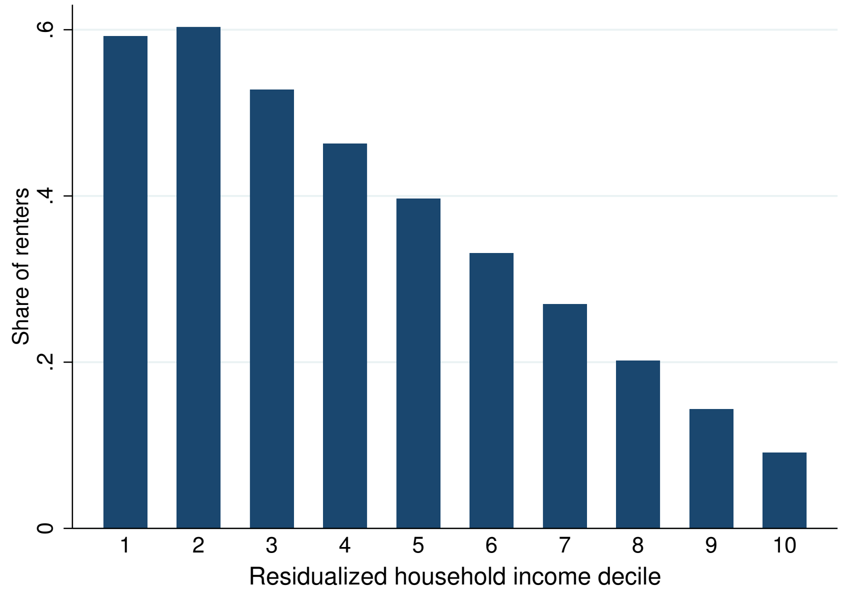
To get a sense of the spatial distribution of minimum wage earners we construct a proxy variable using the number of workers across income bins in the 5-year 2010-2014 American Community Survey (ACS; US Census Bureau, 2022a). See details in Appendix 1.B.2. Our variable for the share of MW workers is

[14]SAFMRs data are constructed by the HUD as an extension of the Fair Market Rents (FMRs) data using, for each year, ZIP code-level information from previous years' American Community Survey (US Department of Housing and Urban Development, 2018, p. 35). SAFMRs are an estimate of the 40th percentile of the rents distribution based on constant housing quality (US Department of Housing and Urban Development, 2018, p. 1).

[15]We impute a small share of missing values using a regression model where the ZIP code-level covariates include data from LODES and the US Census. See Appendix 1.B.3 for details.

[16]This computation will be a good approximation for the housing expenditure share insofar total housing expenditure and total wage income are roughly proportional to their averages under the same constant of proportionality. This computation also assumes away differences in the number of bedrooms across ZIP codes.

Figure 1.3: Probability of being a renter by household income decile, full sample



Notes: Data are from the 2011 and 2013 American Housing Surveys. The figure shows the probability of a household living in a rented unit by household income. We construct the figure as follows. First, we residualize an indicator for being a renter and household income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized household income variable. Finally, we take the average of the residualized renter indicator within each decile. We exclude from the calculation non-conventional housing units, such as mobile homes, hotels, and others.

negatively correlated with median household income from the ACS (corr. = -0.26) and positively correlated with our estimate of the housing expenditure share (corr. = 0.30). This latter correlation also suggests that the MW is likely to affect rents.

1.3.3 Estimation Data and Samples

1.3.3.1 Rents Data

Zillow is the leading online real estate platform in the US, hosting more than 170 million unique monthly users in 2019 (Zillow, 2020a). Zillow provides the median rental and sales price among units listed on the platform for different house types and at different geographic and time aggregation levels (Zillow, 2020b).[17] We collected the ZIP code level data, available from February 2010 to December 2019. There is variation in the entry of a ZIP code to the data, and locations with a small number of listings are omitted.

Our main analyses use the median rental price per square foot among housing units listed in the category

[17] As of the release of this article, the data are no longer available for download. See Internet Archive (2021) for a snapshot of the website as of February 2020, the last month the data were available.

Single Family houses, Condominium and Cooperative units (SFCC). This is the most populated time series, as it includes the most common US rental house types (Fernald, 2020). We focus on rents *per square foot* to account for systematic differences in housing size. It is important to note that these data reflect rents of newly available units, for which new information is likely to be quickly incorporated into prices (Ambrose, Coulson, & Yoshida, 2015). As a result, we expect them to react quickly to economic shocks, such as changes in the MW. On the other hand, rents among the universe of leased units should react more slowly, as they are only updated when the lease is renewed. This is the pattern of results in Agarwal, Ambrose, and Diop (2022) who use data from contract rents.

We use Zillow's Observed Rental Index (ZORI) for a robustness check, computed following the repeat rent index methodology used by Ambrose, Coulson, and Yoshida (2015), among others. To compute the index, Zillow (2023b) uses all units with more than one transaction in a ZIP code and estimates a weighted regression of the log of the change in rents between two months on year-month indicators, upweighting observations that correspond to housing types that are underrepresented in the Zillow sample relative to census data. The published index is a smoothed version of these coefficients, achieved through a three-month moving average that uses data from previous months. To account for this smoothing, we shift our MW measures when using the index as outcome in our regression models.

The Zillow data have several limitations. First, Zillow's market penetration dictates the sample of ZIP codes available. Appendix Figure 1.F shows that the sample of ZIP codes with SFCC rents data coincides with areas of high population density. Second, we only observe the median rental value. No data on the distribution of rents, nor the number of units listed for rent, are available. Finally, we observe posted rents rather than contract rents. We did not find any information on how correlated posted rents and contract rents are, so we decided to ask this as a question in the online platform Quora. Appendix 1.B.4 shows a selection of quotes from the replies, which overwhelmingly suggest that contract rents generally do not differ from posted rents. Some answers also suggest that rents of long-tenured landlords may not reflect current market conditions.

1.3.3.2 The residence and workplace minimum wage measures

Using the panel described in Section 1.3.1 at hand, computing the residence MW is straightforward. We define it as $\underline{w}_{it}^{\text{res}} = \ln \underline{W}_{it}$.

To construct the workplace MW we need commuting data, which we obtain from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES; US Census Bureau,

2021) for the years 2009 through 2018. We collected the datasets for "All Jobs." The raw data are aggregated at the census block level. We further aggregate it to ZIP codes using the original correspondence between census blocks and USPS ZIP codes described in Appendix 1.B.1. This results in residence-workplace matrices that, for each ZIP code and year, indicate the number of jobs of residents in every other ZIP code.

We use the 2017 residence-workplace matrix to build exposure weights. Let $\mathcal{Z}(i)$ be the set of ZIP codes in which i's residents work (including i). We construct the set of weights $\{\pi_{iz}\}_{z\in\mathcal{Z}(i)}$ as $\pi_{iz} = N_{iz}/N_i$, where N_{iz} is the number of jobs with residence in i and workplace in z, and N_i is the total number of jobs originating in i. The workplace MW measure is defined as

$$\underline{w}_{it}^{\text{wkp}} = \sum_{z\in\mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_{zt} \ .$$

The workplace MW has a shift-share structure. Our strategy, which exploits differential exposure to common shocks for identification, is most related to recent work in this area by Goldsmith-Pinkham, Sorkin, and Swift (2020).

While our baseline uses commuting shares from 2017, for robustness we present estimates in which the workplace MW measure is constructed using alternative sets of weights. In particular, we use different years and alternative job categories, such as jobs for young or low-income workers.[18]

Figure 1.1, already discussed in the introduction, illustrates the difference in the MW-based measures mapping their change in the Chicago CBSA on July 2019. For completeness, Appendix Figure 1.G shows the changes in our main median rents variable around the same date.

1.3.3.3 Other data sources

While our MW assignment recognizes that ZIP codes cross census geographies, we assign to each ZIP code a unique geography based on where the largest share of its houses fall. We do this for descriptive purposes and also to use geographic indicators in our estimates. Additionally, we collect ZIP code demographics from the ACS (US Census Bureau, 2022a) and the 2010 US Census (US Census Bureau, 2022b). We collect these data at the block or tract levels, and assign them to ZIP codes using the correspondence table described in Appendix 1.B.1.

To proxy for local economic activity we collect data from the Quarterly Census of Employment and Wages (QCEW; US Bureau of Labor Statistics, 2020b) at the county-quarter and county-month levels for

[18]The LODES data reports origin-destination matrices for the number of workers 29 years old and younger, and the number of workers earning less than \$1,251 per month. The resulting workplace MW measures with any set of weights are highly correlated among each other (corr. > 0.99 for every pair).

several industrial divisions and from 2010 to 2019.[19] We use these data as controls for the state of the local economy in our regression models.

1.3.3.4 Estimation samples

We put together an unbalanced monthly panel of ZIP codes available in Zillow in the SFCC category from February 2010 to December 2019. This panel contains 7,626 MW changes at the ZIP code level, which arise from 82 state and 121 county and city changes. Appendix Figure 1.H shows the distribution of positive MW increases among ZIP codes in the Zillow data. To prevent our estimates from being affected by changes in sample composition, we construct a "baseline" panel keeping ZIP codes with valid rents data starting on January 2015. The resulting fully-balanced panel contains 2,782 MW changes at the ZIP code level.[20]

Table 1.1 compares the sample of ZIP codes in the Zillow data to the population of ZIP codes along sociodemographic dimensions. The first and second columns report data for the universe of ZIP codes and for the set of urban ZIP codes, respectively. The third column shows the set of ZIP codes in the Zillow data with any non-missing value of rents per square foot in the SFCC category. Finally, the fourth column shows descriptive statistics for our estimation sample, which we call the "baseline" sample. While our baseline sample contains only 11.8 percent of urban ZIP codes, it covers 25.0 percent of their population and 25.8 percent of their households. With respect to demographics, ZIP codes in the baseline sample tend to be more populated, richer, with a higher share of Black and Hispanic inhabitants, and with a higher share of renter households than both the average ZIP code and the average urban ZIP code. This is so because Zillow is present in large urban regions, but it does not usually operate in smaller urban or rural areas. In an attempt to capture the treatment effect for the average urban ZIP code we conduct an exercise where we re-weight our sample to match the average of a handful of characteristics of those.

[19] The QCEW covers the following industrial aggregates: "Natural resources and mining," "Construction," "Manufacturing," "Trade, transportation, and utilities," "Information," "Financial activities" (including insurance and real state), "Professional and business services," "Education and health services," "Leisure and hospitality," "Other services," "Public Administration," and "Unclassified." We observe, for each county-quarter-industry cell, the number of establishments and the average weekly wage, and for each county-month-industry cell, the level of employment.

[20] To avoid losing observations in models with leads and lags we include six leads and lags of the MW measures, so the dataset actually runs from July 2014 to June 2020.

Table 1.1: Descriptive statistics of different samples of ZIP codes

	All ZIP codes	Urban ZIP codes	Zillow sample	Baseline sample
Panel A: 2010 Census				
Total population (thousands)	308,129.6	204,585.8	111,709.2	51,181.1
Total number of households (thousands)	131,396.0	83,919.6	47,424.5	21,628.7
Mean population	9,681.7	18,018.8	33,687.9	38,052.9
Mean number of households	4,128.6	7,391.2	14,301.7	16,080.8
Share of urban population	0.391	0.725	0.960	0.972
Share of renter-occupied households	0.224	0.283	0.340	0.333
Share of black population	0.075	0.100	0.153	0.161
Share of white population	0.834	0.765	0.679	0.667
Panel B: 2014 IRS				
Share of households with wage income	0.820	0.830	0.836	0.843
Share of households with business income	0.152	0.161	0.176	0.182
Mean AGI per household (thousand $)	60.4	76.3	83.0	83.9
Mean wage income per household (thousand $)	39.7	49.8	53.2	55.2
Panel C: 2014 SAFMR				
Mean 40th perc. 2BR apt. rent ($)	936.17	1,028.33	1,087.42	1,131.95
Panel D: Minimum wage				
Min. in Dec. 2014 ($)	7.25	7.25	7.25	7.25
Mean in Dec. 2014 ($)	7.74	7.97	7.94	7.87
Max. in Dec. 2014 ($)	15.00	15.00	11.27	10.74
Min. in Dec. 2019 ($)	7.25	7.25	7.25	7.25
Mean in Dec. 2019 ($)	8.85	9.52	9.40	9.23
Max. in Feb. 2019 ($)	16.09	16.09	16.00	16.00
Panel E: Geographies				
Number of ZIP codes	31,826	11,354	3,316	1,345
Number of counties	3,135	605	487	244
Number of states	51	47	49	41

Notes: The table shows characteristics of different samples of ZIP codes. The first column uses all ZIP codes that are matched to a census block following Online Appendix 1.B.1. The second column restricts to ZIP codes located in urban CBSAs, where we define a CBSA as urban if at least 80% of its population was classified as urban by the 2010 US Census. The third column uses ZIP codes with valid SFCC rents per square foot in any month. The fourth column uses our baseline estimation sample, as described in Section 1.3.3.4. Panel A uses data from the 2010 US Census (US Census Bureau, 2022b). Panel B uses data from the 2014 IRS ZIP code-level aggregates (Internal Revenue System, 2022b). AGI is an acronym for Average Gross Income. Panel C uses data from the 2014 Small-Area Fair Market Rents (SAFMR; US Department of Housing and Urban Development, 2020b). Panel D uses data from the panel of MW levels described in Section 1.3.1. Panel E counts the number of different geographies present in each set of ZIP codes, assigned as explained in Section 1.3.3.3.

Finally, Appendix Table 1.A shows statistics of our baseline panel. The distribution of the residence and workplace MW measures is, as expected, quite similar. We also show median rents in Zillow in the SFCC category. The average monthly median rent is \$1,757.9 and \$1.32 per square foot, although these variables show a great deal of variation. Finally, we show average weekly wage, employment, and establishment count for the QCEW industries we use as controls in our models.

1.4 Empirical Strategy

In this section we discuss our empirical strategy. We start with an intuitive presentation of our identification argument, which is formalized in an appendix. Next, we specialize our discussion under the functional form suggested by the model in Section 1.2. We also discuss alternative estimation strategies, concerns related to the sample of ZIP codes we use, and heterogeneity of estimated effects.

1.4.1 Intuitive Identification Argument

Our data consist of rents, the residence and workplace MW measures, and economic controls. We can learn the effect of the workplace MW from the slope of the relationship between the workplace MW and rents conditioning to places with a similar change in the residence MW. We need to condition on the residence MW to remove confounding variation as it may affect locations through other channels, such as changes in prices of non-tradable consumption. Likewise, a similar argument suggests that identifying the effect of the residence MW requires controlling for the workplace MW.

For these slopes to correspond to causal effects, we need to make two assumptions. The first one is a form of *parallel trends*: among ZIP codes with the same residence MW, ZIP codes with higher and lower workplace MW levels would have had parallel trends in rents if not for the change in the workplace MW. The second one is *no selection on gains*: ZIP codes that receive different levels of the workplace MW must experience a similar treatment effect on average, conditional again on the residence MW. If these assumptions hold, the (conditional) slope of the relationship between the workplace MW and rents is actually the causal effect. We similarly need these assumptions to hold for the residence MW if we hope to give a causal interpretation to its coefficient. Appendix 1.C formalizes these assumptions in a potential outcomes framework following Callaway, Goodman-Bacon, and Sant'Anna (2021). We discuss the plausibility of these assumptions later in this section.

1.4.2 Parametric Model

Consider the two-way fixed effects model relating rents and the MW measures given by

$$r_{it} = \alpha_i + \tilde{\delta}_t + \beta \underline{w}_{it}^{\text{wkp}} + \gamma \underline{w}_{it}^{\text{res}} + \mathbf{X}_{it}^{'} \eta + \upsilon_{it}, \tag{1.3}$$

where i and t index ZIP codes and time periods (months), respectively, r_{it} represents the log of rents per square foot, $\underline{w}_{it}^{\text{wkp}} = \sum_{z \in \mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_{zt}$ is the ZIP code's workplace MW, $\underline{w}_{it}^{\text{res}} = \ln \underline{W}_{it}$ is the ZIP code's residence MW, α_i and $\tilde{\delta}_t$ are fixed effects, and $\mathbf{X}_{it}$ is a vector of time-varying controls. Time runs from January 2015 ($\underline{T}$) to December 2019 $\left(\overline{T}\right)$. The parameters of interest are β and γ which, following the model in Section 1.2, we interpret as the elasticity of rents to the residence MW and the workplace MW, respectively.

By taking first differences in equation (1.3) we obtain

$$\Delta r_{it} = \delta_t + \gamma \Delta \underline{w}_{it}^{\text{res}} + \beta \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta \upsilon_{it}, \tag{1.4}$$

where $\delta_t = \tilde{\delta}_t - \tilde{\delta}_{t-1}$. We estimate the model in first differences because we expect unobserved shocks to rental prices to be serially autocorrelated over time, making the levels model less efficient. Appendix Table 1.B shows strong evidence of serial auto-correlation in the error term of the model in levels. While estimated coefficients are similar in levels and in first differences, standard errors are seven to nine times larger in the former.

A standard requirement for a linear model like (1.4) to be estimable is a rank condition, which implies that both MW measures must have independent variation, conditional on the controls. For instance, if there were a single national minimum wage level or if everybody lived and worked in the same location, then we would have $\Delta \underline{w}_{it}^{\text{res}} = \Delta \underline{w}_{it}^{\text{wkp}}$ for all (i, t). If so, γ and β could not be separately identified. We check in the data that the rank condition is satisfied.

The main results of the paper are obtained under the model in (1.4). In order to compare with the literature we also estimate versions of the model that exclude either one of the MW measures.

1.4.3 Validity of Identification Assumptions

The model in (1.3) imposes a linear functional form. This property rules out selection on gains, since then ZIP codes receiving a particular level of the MW measures will experience the same (constant) effect than

ZIP codes that receive a different level. This is one of the assumptions required for identification according to Appendix 1.C. We view this as a reasonable assumption. For it to not hold, workers would need to anticipate not only future MW policies but also how future rental markets would be affected by them given the commuting structure, and select their residence so that rents react differently to the MW in different ZIP codes with similar levels of the MW measures. We show in the results section that the (conditional) slope of log rents with respect to each of the MW measures appears linear, suggesting that the assumption of no selection on gains is plausible.

For estimates of β and γ from (1.3) to have a causal interpretation we need another assumption: the error term $\Delta\upsilon_{it}$ must be *strictly exogenous* with respect to the MW measures, and in particular with respect to the workplace MW. This is in the spirit of parallel trends, the second assumption required for identification in Appendix 1.C. This assumption implies that rents prior to a change in the workplace MW must evolve in parallel. We test for pre-trends adding leads and lags of the workplace MW in (1.4),[21] though we also experiment with adding leads and lags of the residence MW. We only shift the workplace MW because estimating its effect is our focus, seeing the residence MW as a key control. In fact, Appendix 1.C suggests that we only need to condition on one of the MW measures for parallel trends of the other measure to hold. Under the assumption of no anticipatory effects in the housing market, we interpret the absence of pre-trends as evidence against the presence of unobserved economic shocks driving our results. Given the high frequency of our data and the focus on short windows around MW changes, the assumption of no anticipatory effects seems plausible.[22]

Another implication of the strict exogenity assumption is that it allows for arbitrary correlation between α_i and both MW variables. This means that our empirical strategy is robust to the fact that districts with more expensive housing tend to vote for MW policies.

We worry that unobserved shocks, such as those caused by local business cycles, may systematically affect both rent changes and MW changes, violating the strict exogeneity assumption. To account for common trends in the housing market we include time-period fixed effects δ_t, which in some specifications are allowed to vary by jurisdiction. To control for variation in local labor markets trends we include economic controls from the QCEW in the vector $\mathbf{X}_{it}$. Specifically, we control for average weekly wage and establishment

[21] Specifically, we estimate

$$\Delta r_{it} = \delta_t + \gamma \Delta \underline{w}_{it}^{\text{res}} + \sum_{k=-s}^{s} \beta_k \Delta \underline{w}_{ik}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta \upsilon_{it},$$

where $s = 6$. Our results are very similar for different values of the window s.

[22] We can also interpret the absence of pre-trends as a test for anticipatory effects if we are willing to assume that the controls embedded in $\mathbf{X}_{it}$ capture all relevant unobserved heterogeneity arising from local business cycles. While we find the interpretation given in the text more palatable, the data are consistent with both.

counts at the county-quarter level, and for employment counts at the county-month level, for the sectors "Professional and business services," "Information," and "Financial activities."[23] We also try models where we control for ZIP code-specific linear trends, which should account for time-varying heterogeneity at the ZIP code-level that follows a linear pattern.

A second worry is that changes in the composition of rentals may drive the results. For instance, if on the same month of the MW change more expensive listings go to the market, then what looks like a rent increase may actually be changes in quality.[24] We note that changes in housing size, which seem to be the key driver in price heterogeneity according to Appendix Figure 1.C, are controlled for because we use rents per square foot as our outcome. To more directly address this concern we present evidence using Zillow's observed rental index (ZORI), which is constructed using rental prices for the same housing unit in different moments in time. Given that the index is averaged using three lags, a regression analysis that relates period-t MWs would be expected to affect the ZORI index at $t-3$, and its first difference at $t-4$. To adjust for this, in these models we use the 4th lead of the change in the MW measures.

1.4.4 Alternative Strategies

Recent literature has shown that usual estimators in a difference-in-differences setting do not correspond to well-defined average treatment effects when the treatment roll-out is staggered and there is treatment-effect heterogeneity (de Chaisemartin & D'Haultfoeuille, 2022; Roth et al., 2022). While our setting does not correspond exactly to the models discussed in this literature, we worry about the validity of our estimator. To ease these concerns, in an appendix we construct a "stacked" implementation of equation (1.4) in which we take six months of data around MW changes for ZIP codes in CBSAs where some ZIP codes received a direct MW change and some did not, and then estimate the model on this restricted sample including event-by-time fixed effects. This strategy limits the comparisons used to compute the coefficients of interest to ZIP codes within the same metropolitan area and event.

In a separate exercise we relax the strict exogeneity assumption. We do so in an appendix as well, where we propose a model that includes lagged rents as an additional control. In such a model, β and γ have a causal interpretation under a weaker *sequential exogeneity* assumption (Arellano & Bond, 1991; Arellano & Honoré, 2001). This alternative assumption requires innovations to rents to be uncorrelated only with past changes in the MW measures, and thus allows for feedback of rent shocks onto MW changes in future periods.

[23] We assume that these sectors are not affected by the MW. In fact, according to the US Bureau of Labor Statistics (2020a, Table 5), in 2019 the percent of workers paid an hourly rate at or below the federal MW in those industries was 0.8, 1.5, and 0.2, respectively. In comparison, 9.5 percent of workers in "Leisure and hospitality" were paid an hourly rate at or below the federal MW.

[24] We thank an anonymous referee for pointing out this concern.

We estimate this model using an IV strategy in which the first lag of the change in rents is instrumented with the second lag.

1.4.5 Heterogeneity and Sample Selection Concerns

We explore heterogeneity of our results with respect to pre-determined variables. Given the mechanism proposed in Section 1.2, we expect the effect of the residence MW to be stronger in locations where many workers earn close to the MW. The reason is that the production of non-tradable goods presumably uses more low-wage work, and thus the increase in the MW would affect prices more. Similarly, we expect the effect of the workplace MW to be stronger in locations with lots of MW workers as residents since income would increase more strongly there. We then estimate the following model:

$$\Delta r_{it} = \Xi_t + \tilde{\gamma}_0 \Delta \underline{w}_{it}^{\text{res}} + \tilde{\gamma}_1 \iota_i \Delta \underline{w}_{it}^{\text{res}} + \tilde{\beta}_0 \Delta \underline{w}_{it}^{\text{wkp}} + \tilde{\beta}_1 \iota_i \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}' \tilde{\eta} + \Delta \tilde{\upsilon}_{it},$$

where ι_i represents the standardized share of MW workers residing in i. Because we cannot estimate the share of MW workers working in a given location, we interact both the residence and workplace MW with the estimated share of MW residents. We conduct a similar exercise using median household income and the share of public housing units.

As explained in Section 1.3.3.4, the model in equation (1.4) relies on a selected sample. In an alternative estimation exercise we use an unbalanced panel with all ZIP codes with Zillow rental data in the SFCC category from February 2010 to December 2019, controlling for time period by quarterly date of entry fixed effects. However, even all ZIP codes available in the Zillow data may be a selected sample of the set of urban ZIP codes. To approximate the average treatment effect in urban ZIP codes we follow Hainmueller (2012) and estimate our main models re-weighting observations to match key moments of the distribution of characteristics of those.

1.5 Estimation Results

In this section we present our empirical results. First, we show our baseline estimates and discuss our identifying assumptions and other robustness checks. Second, we present results of models that use alternative empirical strategies. Third, we discuss concerns that arise from the selectivity of our sample of ZIP codes and show heterogeneity analyses. Finally, we summarize our results and compare them with existing literature.

1.5.1 Baseline estimates

1.5.1.1 Main results

Table 1.2 displays our estimates using the baseline sample described in Section 1.3.3.4 and the parametric model in equation (1.4). Column (1) shows the results of a regression of the workplace MW on the residence MW, economic controls, and time fixed effects. We observe that a 10 percent increase in the residence MW is associated with an 8.63 percent increase in the workplace MW. While the measures are strongly correlated, this estimate shows that this correlation is far from exact, confirming the presence of independent variation that allows the inclusion of both MW measures in our models.

Columns (2) and (3) of Table 1.2 show estimates of models that include a single MW measure. Column (2) uses only the residence MW. In this model, only locations with a statutory MW change are assumed to experience effects, similar to the existing literature. The elasticity of rents to the MW is estimated to be 0.0372 ($t = 2.57$). Column (3) uses solely the workplace MW. The coefficient on the MW variable increases slightly to 0.0449 ($t = 2.88$), supporting the view that changes in the workplace MW are a better measure of the changes in the MW that are relevant for a ZIP code.

Table 1.2: Estimates of the effect of the minimum wage on rents, baseline sample

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}		
	(1)	(2)	(3)	(4)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	0.8627	0.0372		-0.0219
	(0.0374)	(0.0145)		(0.0175)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$			0.0449	0.0685
			(0.0156)	(0.0288)
Sum of coefficients				0.0466
				(0.0158)
Economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0514
R-squared	0.9444	0.0212	0.0213	0.0213
Observations	80,241	80,241	80,241	80,241

Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. Column (1) shows the results of a regression of the workplace MW measure on the residence MW measure. Column (2) through (4) show the results of regressions of the log of median rents per square foot on our MW-based measures. All regressions include time-period fixed effects and economic controls that vary at the county by month and county by quarter levels. The measure of rents per square foot corresponds to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the log of the average wage, the log of employment, and the log of the establishment count from the sectors "Information", "Financial activities", and "Professional and business services". Standard errors in parentheses are clustered at the state level.

Column (4) of Table 1.2 show estimates of equation (1.4) including both MW measures. The coefficient on the workplace MW (β) increases to 0.0685 and is statistically significant ($t = 2.38$). These results suggest that omitting the residence MW generates a downward bias on the coefficient of the workplace MW. Consistent with the theoretical model in Section 1.2, the coefficient on the residence MW (γ) now turns negative and equals -0.0219, although it is not statistically significant ($t = -1.25$). We reject the hypothesis that $\gamma = \beta$ at the 10% significance level ($p = 0.051$). Finally, $\gamma + \beta$ is estimated to be 0.0466, which is highly significant ($t = 2.95$). Thus, our results imply that a 10 percent increase in both MW measures will increase rents by approximately 0.47 percent. However, our results also imply substantial heterogeneity across space. If only the residence MW increases then rents are expected to decline, and if only the workplace MW goes up then the rents increase will likely be larger.

1.5.1.2 Identification assumptions

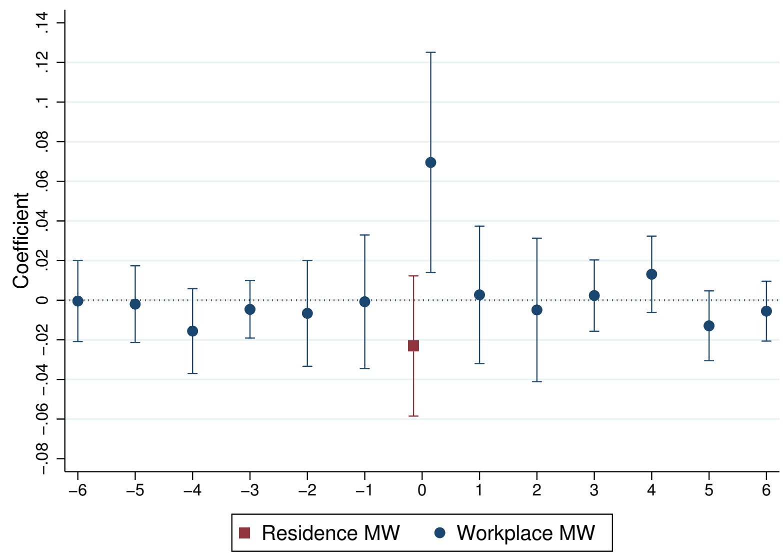
A central concern with these results is whether our identifying assumptions are likely to hold. Panel A of Figure 1.4 shows estimates of the parametric first-differences model including leads and lags of the workplace MW, so that the coefficients are $\{\{\beta_s\}_{s=-6}^{-1}, \beta, \{\beta_s\}_{s=1}^{6}, \gamma\}$. We cannot reject the hypothesis that $\beta_{-6} = ... = \beta_{-1} = 0$ ($p = 0.563$). Post-event coefficients $\{\beta_s\}_{s=1}^{6}$ are also estimated to be statistically zero. The only significant estimate is β at 0.0695 ($t = 2.45$). The estimate of γ is -0.0231 ($t = -1.28$). We now reject the hypothesis of equality of coefficients at the 5% significance level ($p = 0.045$). Our estimate of $\gamma + \beta$ is 0.0464. It is significant ($t = 2.90$) and almost identical to our baseline. Panel B shows the implied effects in levels of our first-differences models, assuming that pre- and post-coefficients are zero. Appendix Figure 1.I shows that a similar story obtains when we add leads and lags of the residence MW only. We interpret these results as evidence in favor of the parallel trends assumption.

Appendix Figure 1.J plots the relationship between log rents and each of the MW measures for ZIP codes in CBSAs and months in which at least one residence MW changed. Panel A displays the raw data, which shows a positive correlation between log rents and both MW measures. Panel B displays the same relationships after residualizing each variable on ZIP code fixed effects and indicators for different values of the other MW measure. We observe a positive slope for the workplace MW, and a negative one for the residence MW. This provides evidence in favor of the assumption of no selection on gains, and also of the linear functional form assumed in equation (1.3). Furthermore, the slopes in these figures show a similar magnitude to our baseline estimates of γ and β.

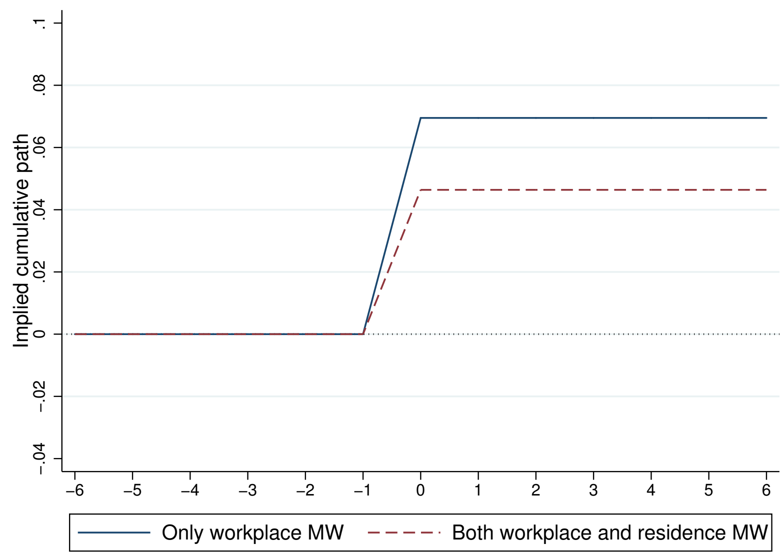
Appendix Figure 1.K illustrates the identifying variation we use by mapping the residualized change in

Figure 1.4: Estimates of the effect of the minimum wage on rents, baseline sample including leads and lags

(a) Estimates in first differences



(b) Implied path in levels



Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. The top panel shows coefficients from regressions of the change in log of rents per square foot on leads and lags of the change in the workplace MW and the change in the residence MW. The bottom panel shows the implied paths in levels given the estimated coefficients assuming pre- and post-coefficients are equal to zero. The regression includes time-period fixed effects and economic controls that vary at the county by month and county by quarter levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the log statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors "Information," "Financial activities," and "Professional and business services." 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

workplace MW and the residualized change in log rents.[25] Panel A of Appendix Figure 1.K, to be contrasted with the left panel of Figure 1.1, shows that the residualized change in the workplace MW is high outside of Cook County, where the statutory MW increased. For completeness, Panel B of Appendix Figure 1.K shows residualized rents.

1.5.1.3 Zillow's repeat rental index

Appendix Figure 1.L shows regression results using the ZORI index. Since the index has more missing values than our baseline before 2020, we use the entire sample of ZIP codes and set the window to 4.[26] Panel A controls for CBSA by year-month fixed effects, and shows a strong increase in rents following a change in the workplace MW. We observe that the effects lasts for a few months, which is to be expected as this variable is computed as an average over 3 months. The residence MW has a negative coefficient, although it is not statistically significant. We observe precisely estimated null pre-trends. Panel B controls for year-month fixed effects only, and shows similar though weaker patterns. We see this evidence as supportive of the view that the estimated effects using our main rental variable are not driven by changes in the composition of listings that coincide with changes in the MW.

1.5.1.4 Robustness checks

Table 2.B shows how our results change when we vary the specification of the regression model and the commuting shares used to construct the workplace MW measure. Each row of the table shows estimates analogous to those of columns (1) and (4) of Table 1.2.

Panel A of Table 2.B groups the results when varying the regression equation. Row (b) shows that our results are very similar when we exclude the economic controls from the QCEW. Rows (c) and (d) show that interacting our time fixed effects with indicators for county or CBSA yields similar conclusions. In all these cases our baseline point estimates are contained in relevant confidence intervals and, in the case of CBSA by month fixed effects, the results seem even larger. This supports the view that our results are not caused by regional trends in housing markets correlated with our MW variables. Row (e) shows that the results are non-significant when using state by monthly date fixed effects. While our baseline estimates are within relevant confidence intervals, the signs of the point estimates are flipped. However, these results are much noisier. In fact, a formal test does not reject the hypothesis that the coefficients on the workplace MW

[25] To maximize the number of ZIP codes with valid data on this map we use the results of the unbalanced panel discussed in Section 1.5.3.

[26] While our baseline analysis in Table 1.2 uses 80,241 observations, the models that are estimated using the ZORI index use only 22,984.

($p = 0.2168$) and the residence MW ($p = 0.2668$) are equal to our baseline. Row (f) includes ZIP code fixed effects in the first differences model, which is equivalent to allowing for a ZIP code-specific linear trend in the model in levels. These results are also very similar to our baseline.

Panel B of Table 2.B estimates the baseline model but computing the workplace MW using alternative commuting structures. Rows (g) and (h) use commuting shares from 2014 or 2018 instead of 2017. Row (i) allows the commuting shares to vary by year, introducing additional cross-year variation in the workplace MW measure that does not arise from changes in the statutory MW. The fact that these specifications yield very similar results suggests that changes in commuting correlated with MW changes are unlikely to be the driver of the results. Furthermore, as discussed in the last subsection of Section 1.2.3, changing commuting shares would load on the workplace MW. However, we see no effects of the workplace MW beyond the month of the change, suggesting that commuting shares are empirically quite stable. Finally, rows (j) and (k) use 2017 commuting shares for workers earning less than $1,251 per month and workers that are less than 29 years old, respectively. If anything, the results seem stronger and more significant in this case, consistent with the idea that these workers are more likely to earn close to the minimum wage.

1.5.2 Alternative Strategies

Appendix Table 1.C estimates our main models using a "stacked" sample, as discussed in Section 1.4.4. Our sample contains 184 "events," that is, CBSA-month pairs that had some strict subset of ZIP codes increasing the residence MW and had at least 10 ZIP codes. These estimates interact the year-month fixed effects with event ID indicators, limiting comparisons to ZIP codes in the same event. This is in line with recent literature that focuses on carefully selecting the comparison groups in difference-in-differences settings (de Chaisemartin & D'Haultfoeuille, 2022; Roth et al., 2022). We find that our key MW-based measures have little predictive power on their own, but the model including both measures yields similar patterns as our baseline. If anything, results are stronger in this case. Now, both MW measures are strongly significant. A 10 percent increase in both MW measures is estimated to increase rents by 0.463 percent. Appendix Figure 1.M shows the results of a similar model that includes leads and lags of the workplace MW. Estimates of leads and lags are statistically non-distinguishable from zero. However, they are noisier than in our baseline.

Table 1.3: Robustness of estimates of the effect of the minimum wage on rents, baseline sample

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}			
	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Sum of coefficients	N
(a) Baseline	0.8627	-0.0219	0.0685	0.0466	80,241
	(0.0374)	(0.0175)	(0.0288)	(0.0158)	
Panel A: Vary specification					
(b) No controls	0.8632	-0.0200	0.0668	0.0468	80,692
	(0.0374)	(0.0180)	(0.0291)	(0.0162)	
(c) County by time FE	0.2857	-0.0606	0.1559	0.0953	75,593
	(0.0399)	(0.0511)	(0.1116)	(0.0811)	
(d) CBSA by time FE	0.5081	-0.0358	0.0944	0.0587	78,293
	(0.0387)	(0.0295)	(0.0610)	(0.0343)	
(e) State by time FE	0.5405	0.0142	-0.0076	0.0066	80,393
	(0.0629)	(0.0239)	(0.0526)	(0.0320)	
(f) ZIP code-specific linear trend	0.8596	-0.0217	0.0711	0.0494	80,241
	(0.0390)	(0.0167)	(0.0264)	(0.0132)	
Panel B: Vary workplace MW measure					
(g) 2014 commuting shares	0.8625	-0.0199	0.0662	0.0463	80,241
	(0.0377)	(0.0193)	(0.0299)	(0.0158)	
(h) 2018 commuting shares	0.8626	-0.0217	0.0683	0.0466	80,241
	(0.0372)	(0.0177)	(0.0292)	(0.0159)	
(i) Time-varying commuting shares	0.8806	-0.0292	0.0792	0.0500	64,236
	(0.0372)	(0.0207)	(0.0309)	(0.0166)	
(j) 2017 commuting shares, low-income workers	0.8566	-0.0348	0.0841	0.0493	80,241
	(0.0371)	(0.0221)	(0.0341)	(0.0160)	
(k) 2017 commuting shares, young workers	0.8569	-0.0332	0.0822	0.0490	80,241
	(0.0390)	(0.0180)	(0.0294)	(0.0156)	

Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. Each row of the table shows two estimations on the same sample of ZIP codes and months. The first column shows the results of a regression of the change in the workplace MW on the change in the residence MW. The second through fourth columns show the results of a regression of the change in log rents on the change in the residence MW and the workplace MW, with the fifth column showing the sum of the coefficients on the MW measures. The rents variable corresponds to the median rent per square foot in the SFCC category in Zillow. Row (a) repeats the results of Table 1.2, including fixed effects for each year month and economic controls from the QCEW. Specifications in Panel A vary the set of fixed effects included in the regression relative to row (a). Row (f) includes ZIP code fixed effects in the first-differenced model, which in the level model can be interpreted as a ZIP-code specific linear trend. Specifications in Panel B vary the commuting shares used to construct the workplace MW measure relative to row (a). Row (i) uses data from 2015 to 2018 only. Standard errors in parentheses are clustered at the state level.

Appendix Table 1.D shows estimates of a model that includes the lagged difference in log rents as a covariate. This specification relaxes the strict exogeneity assumption and allows for feedback effects of rent increases on the MW measures. To avoid the well-known endogeneity problem of including this covariate, the models are estimated using an IV strategy where we instrument the first lag of the change in rents with the second lag of this variable (Arellano & Bond, 1991; Arellano & Honoré, 2001). Columns (1) and (2) show estimates of models in levels, both of which imply confidence intervals for the coefficients that include our preferred estimates. Columns (3) and (4) show preferred models in first differences, where results are very similar across strategies.

1.5.3 Heterogeneity and Sample Selection Concerns

Table 1.4 explores heterogeneity of our estimates. Column (1) reproduces the baseline results. Column (2) presents estimates interacting the MW measures with an estimated share of MW workers residing in each ZIP code. At the mean share of MW workers, our estimates indicate that the coefficient on the workplace MW is 0.097 (SE = 0.030). For a ZIP code that is one standard deviation above the average share of MW workers, the effect of the workplace MW is stronger at 0.181 (SE= 0.065). Housing more MW workers in a ZIP code implies that income is likely to be more sensitive to the MW and so, consistent with our model, the effect of the MW on rents is larger. The coefficient on the residence MW also presents significant heterogeneity.

Column (3) of Table 1.4 interacts both MW measures with the standardized median household income from the ACS. We find analogous patterns to Column (2), as a higher median income is correlated with a lower share of MW workers. Column (4) interacts the MW measures with the standardized share of public housing units. The effects for ZIP codes with more public housing seems larger, although the coefficient on the interaction is not statistically significant. This result suggests that public housing does not necessarily diminish the scope for landlords to increase rents. However, it is possible that this variable is capturing a high presence of low-wage residents and workers who, per our previous discussion, are more affected by the MW.

Appendix Table 1.E explores the sensitivity of our estimates to the sample of ZIP codes used in estimation. Column (1) replicates our baseline estimates. In column (2) we estimate the same model but re-weighting observations to match pre-treatment characteristics of the sample of urban ZIP codes, defined in Table 1.1.[27] The coefficient on the workplace MW is somewhat smaller, but it remains strongly significant. Column (3)

[27] Our weights follow Hainmueller (2012) and are designed to match the averages of three variables from the 2010 US Census: the share of urban households, the share of renter-occupied households, and the share of white households.

Table 1.4: Heterogeneity of estimates of the effect of the minimum wage on rents, baseline sample

	Change log rents Δr_{it}			
	(1)	(2)	(3)	(4)
Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0219	-0.0483	-0.0377	-0.0158
	(0.0175)	(0.0196)	(0.0254)	(0.0150)
Change res. MW × Std. share of MW workers		-0.0801		
		(0.0392)		
Change res. MW × Std. median household income			0.0506	
			(0.0266)	
Change res. MW × Std. share of public housing				-0.0336
				(0.0330)
Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0685	0.0969	0.0862	0.0645
	(0.0288)	(0.0300)	(0.0356)	(0.0258)
Change wkp. MW × Std. share of MW workers		0.0841		
		(0.0445)		
Change wkp. MW × Std. median household income			-0.0608	
			(0.0344)	
Change wkp. MW × Std. share of public housing				0.0306
				(0.0374)
Mean heterogeneity variable		0.1497	60,457	0.0044
Std. dev heterogeneity variable		0.0468	22,923	0.0173
R-squared	0.0213	0.0212	0.0211	0.0213
Observations	80,241	75,329	77,197	79,701

Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. In all columns we report the results of regressions of the log of median rents per square foot on our MW-based measures. Column (1) reproduces estimates our baseline results from Table 1.2. In column (2) the changes in residence and workplace MW levels are interacted with the standardized share of MW workers residing in the ZIP code, estimated as in Online Appendix 1.B.2. In column (3) they are interacted with standardized median household income from the ACS (US Census Bureau, 2022a). In column (4) they are interacted with the standardized share of public housing units. To construct this share we use total units of public housing in 2017 (US Department of Housing and Urban Development, 2022a), and the number of households in the 2010 US Census (US Census Bureau, 2022b). Standard errors in parentheses are clustered at the state level.

uses an unbalanced sample of ZIP codes and controls for quarter-of-entry by year-month fixed effects. The coefficient on the workplace MW is again somewhat smaller than our baseline, and now it is significant only at the 10% level.

1.5.4 Alternative rental categories

Appendix Table 1.F shows how our results change when we use other rental categories available in the Zillow data. For each rental variable we use an unbalanced panel that controls for year-month fixed effects interacted with indicators for the quarter of entry to the data in the given rental category. We note that the number of observations varies widely across housing categories, and is always much lower than for our baseline SFCC variable.

Given the reduced precision of these estimates is hard to obtain strong conclusions on what type of housing is reacting more strongly to MW changes. We observe that the sum of the coefficients on our MW variables is statistically significant at conventional levels in the categories "Single Family" (SF), "Condominium and Cooperative Houses" (CC), and "Multifamily 5+ units." Appendix Figure 1.D shows that low-wage households are likely to reside in these type of housing units. However, the coefficients on each of the MW measures are typically much noisier than baseline. We observe inconsistent results for the category "1 bedroom," for which the sign of the coefficients is flipped relative to baseline. However, these estimates are not statistically significant.

1.5.5 Summary and Discussion

We find strong evidence that increases in the MW at workplace locations increase rents, supporting the view that MW policies spill over across local housing markets through commuting. Our baseline estimated elasticity of rents to the MW is 0.0685. The magnitude of our estimates is similar to estimates of the elasticity of restaurant prices to the MW (Allegretto and Reich, 2018 finds an average restaurant price elasticity of 0.058), and the elasticity of grocery store prices to the MW (Leung, 2021 finds an elasticity ranging from 0.06 to 0.08; see also Renkin, Montialoux, and Siegenthaler, 2020).

Our results are also consistent with existing research on the elasticity of wages to the MW. For instance, estimates in Cengiz et al. (2019) (obtained using state MW events), and an assumed share of MW workers of 0.14 (as the average in Table 1.4), imply an elasticity of income to the MW of 0.094.[28] Hughes (2020, Table 1) finds an elasticity of household income of affected households to the MW of 0.189. Dube (2019b) finds minimum wage elasticities of household income between 0.152 and 0.430 for households at the lower end of the distribution. Wiltshire, McPherson, and Reich (2023) explores recent large MW increases in the US and finds an earnings elasticity of 0.18 for fast food workers. For a given elasticity of wages to the MW ε, and a share of housing expenditure s, our elasticity of rents to the workplace MW implies that $\gamma = 100 \times (s0.0685/\varepsilon)$ percent of the income generated by the MW is spent in housing. For instance, if we assume $s = 1/3$ and $\varepsilon = 0.1$, then $\gamma = 22.8$.

We find that our estimates of the elasticity of the MW to rents are on the lower end of the range of estimates in the literature. Relying on repeated cross-sectional survey data from the ACS, Hughes (2020, Table 1) finds an elasticity of rents to the MW for affected households of 0.0543, similar to ours. Our estimates are significantly smaller than those in Yamagishi (2021) and Agarwal, Ambrose, and Diop (2022).

[28] Cengiz et al. (2019, Table I) find that a "MW event" increases wages by 6.8 percent, and their average MW event represents an increase of 10.1 percent. Assuming that 14 percent of workers in a location earn the MW results in an elasticity of $(6.8/10.1) \times 0.14 \approx 0.0943$. In their data, the authors find that "8.6% of workers were below the minimum wage." Our estimates of this share is likely larger because we account for local MW policies.

Exploiting heterogeneity in MW levels across Japanese prefectures, Yamagishi (2021) estimates an elasticity of 0.25–0.45. Agarwal, Ambrose, and Diop (2022) estimate the effect of the MW on rents using state-level MW changes between 2000 and 2009, and their findings imply an elasticity of 0.73. There are many factors that could account for the differential results. For instance, while these authors use micro-data on total rental payments, we rely on the median per-square foot rent in a ZIP code. Additionally, Yamagishi (2021) focuses on low-quality apartments. While precisely pinning down the source of the differences is hard, given the existing evidence of the effect of the MW on income and consumption prices we see our results as plausible.

1.6 Counterfactual Analysis

We use our empirical results to explore the incidence of counterfactual MW policies across space. We evaluate two policies: an increase in the federal MW from \$7.25 to \$9, and an increase in the local MW of the city of Chicago from \$13 to \$14. To measure incidence, we compute the share of the extra income generated by the policy that is pocketed by landlords.

1.6.1 Empirical Approach

Following the notation in Section 1.2, define the ZIP code-specific share pocketed by landlords as

$$\rho_i = \frac{\Delta H_i R_i}{\Delta Y_i} = \frac{H_i^{\text{Post}} R_i^{\text{Post}} - H_i^{\text{Pre}} R_i^{\text{Pre}}}{\Delta Y_i}$$

where "Pre" and "Post" denote moments before and after the MW change, $H_i R_i = \sum_{z\in\mathcal{Z}(i)} H_{iz} R_{iz}$ denotes total housing expenditure in i, and $Y_i = \sum_{z\in\mathcal{Z}(i)} Y_{iz}$ denotes total wage income in i.

Changes in rented square footage (if any) are unobserved. Therefore, we assume $H_i^{\text{Pre}} = H_i^{\text{Post}} = H_i$ and the share becomes

$$\rho_i = \frac{H_i^{\text{Post}} R_i^{\text{Post}} - H_i^{\text{Pre}} R_i^{\text{Pre}}}{\Delta Y_i} = H_i \frac{\Delta R_i}{\Delta Y_i}. \tag{1.5}$$

If $\Delta H_i > 0$ instead, then our estimates of ρ_i will be a lower bound.

We predict rent changes for all ZIP codes using the model in equation (1.4). Because we are interested in the partial effect of the policy, we hold constant common shocks affecting all ZIP codes, local economic trends reflected in the controls, and idiosyncratic shocks that show up in the error term. Then,

$$\Delta r_i = \beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}}. \tag{1.6}$$

We define the change in log total wages using a first-differenced model as well:

$$\Delta y_i = \varepsilon \Delta \underline{w}_i^{\text{wkp}}, \tag{1.7}$$

where $y_i = \ln Y_i$. The residence MW is excluded because we are considering the effect of the MW on nominal wages. Estimates of the income elasticity ε are not readily available in the literature. Given the discussion in Section 1.5.5, we set a baseline value of $\varepsilon = 0.1$. However, we show how our results change for different values of ε.

Assuming that we know the value of ε, we can substitute (1.6) and (1.7) into equation (1.5) to obtain

$$\begin{aligned}\rho_i &= H_i \left[\frac{\exp\left(\Delta r_i + r_i\right) - R_i}{\exp\left(\Delta y_i + y_i\right) - Y_i}\right] \\ &= s_i \left[\frac{\exp\left(\beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}}\right) - 1}{\exp\left(\varepsilon \Delta \underline{w}_i^{\text{wkp}}\right) - 1}\right]\end{aligned}$$

where $s_i = \left(H_i R_i\right)/Y_i$ is the share of i's expenditure in housing. As discussed in Section 1.3.2, we estimate this share as the ratio of the 2-bedroom SAFMR rental value, $\tilde{R}_i$, and monthly average wage per household, $\tilde{Y}_i$.

We also compute the total incidence of the policy on ZIP codes $i \in \mathcal{Z}_1$, for some subset $\mathcal{Z}_1 \subseteq \mathcal{Z}$, as follows:

$$\rho_{\mathcal{Z}_1} = \frac{\sum_{i \in \mathcal{Z}_1} \tilde{R}_i \left(\exp\left(\beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}}\right) - 1\right)}{\sum_{i \in \mathcal{Z}_1} \tilde{Y}_i \left(\exp\left(\varepsilon \Delta \underline{w}_i^{\text{wkp}}\right) - 1\right)}.$$

In words, total incidence is defined as the ratio of the total change in rents per household in $\mathcal{Z}_1$ to the total change in wage income per household in $\mathcal{Z}_1$.

1.6.2 Results

We use our estimates to compute the shares $\{\{\rho_i\}_{i \in \mathcal{Z}_1}, \rho_{\mathcal{Z}_1}\}$ for two counterfactual scenarios: an increase of the federal MW from \$7.25 to \$9 and an increase in the Chicago City MW from \$13 to \$14. In the federal case, we let $\mathcal{Z}_1$ be the set of ZIP codes located in urban CBSAs (as defined in Table 1.1) and exclude ZIP codes that are part of a CBSA where the average estimated increase in log total wages is less than 0.1%.[29]

[29] The goal of this restriction is to exclude metropolitan areas located in jurisdictions with a MW level above the new counterfactual federal level. Because all those ZIP codes experience a small and similar increase in the workplace MW, the estimated share pocketed will be equal to the estimated housing expenditure share times the constant $(\exp(\beta x) - 1) / (\exp(\varepsilon x) - 1)$, where x is the value of the workplace MW increase. These estimates, however, are not economically meaningful because the increase in income due to the policy is negligible.

In the local case, we let $\mathcal{Z}_1$ represent ZIP codes in the Chicago-Naperville-Elgin CBSA, which are the most exposed to this policy.

1.6.2.1 Counterfactual increases in residence and workplace MW levels

We compute the counterfactual statutory MW in January 2020 at a given ZIP code by taking the max between (i) the state, county, and local MW in December 2019, and (ii) the assumed value for the federal or city MW in January 2020.[30] Then, we compute the counterfactual values of the residence MW and the workplace MW following the procedure outlined in Section 1.3.3.2. Like in our baseline estimates, we use commuting shares for all workers in 2017.

Federal increase. The distributions of counterfactual increases in the MW measures are displayed in Appendix Figure 1.N. Out of the 6,784 ZIP codes that satisfy our criteria, 1,043 (or 15.4%) experience no increase in the residence MW at all. The residence MW increases in 5,741 ZIP codes (or 84.6%), 3,616 of which were bound by the previous federal MW, and so the residence MW increases by $\ln(9) - \ln(7.25) \approx 0.2162$ in them. Correspondingly, we observe mass points in the distribution of the residence MW, with the two largest ones at 0 and 0.2162. Since many people reside and work under the same statutory MW, the mass points are still visible in the histogram of the workplace MW. However, we observe more places experiencing moderate increases in this measure.

Panel A of Appendix Figure 1.O maps the changes in the residence and workplace MW in the Chicago-Naperville-Elgin CBSA. Unlike in Figure 1.1, we observe the MW increasing from the outside of Cook County and spilling over inside it.

Local increase. In our second counterfactual experiment we increase the Chicago City MW from \$13 to \$14 on January 2020, keeping constant other MW policies. Importantly, under this assumption the difference between the Chicago and Cook County MW levels increases by \$1.

In this case, there are 62 ZIP codes whose residence MW are affected by this change and 323 that remain directly unaffected. Panel B of Appendix Figure 1.O shows the changes in both MW measures after this policy. As expected, we observe large increases in the workplace MW in the city, which become smaller as one moves away from it.

[30] To be more precise, we take the maximum between the MW levels of different jurisdictions at the level of the block. Then, we aggregate up to ZIP codes using the correspondence table in Appendix 1.B.1. We do so to account for the fact that the new MW policy may be partially binding in some ZIP codes.

1.6.2.2 The share of extra wage income pocketed by landlords

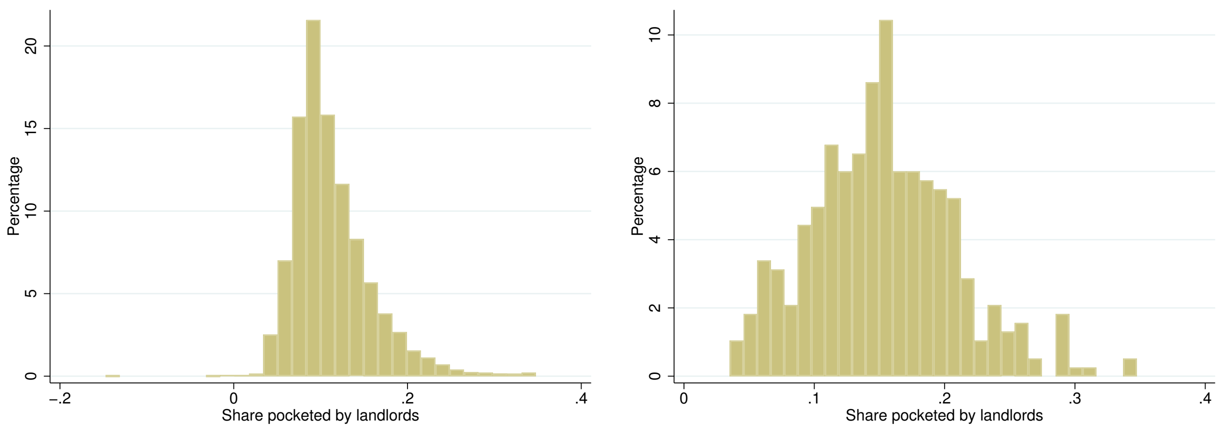
We couple the counterfactual increases in residence and workplace MW with estimates of β and γ. Following the results in Table 1.2, we take $\beta = 0.0685$ and $\gamma = -0.0219$. Based on previous discussion, we set $\varepsilon = 0.1$. We follow the procedure outlined in the previous subsection to estimate the incidence of the counterfactual policy.

Federal increase. Panel A of Figure 1.5 displays a histogram of the estimated shares $\{\rho_i\}_{i\in\mathcal{Z}_1}$. The median share is 0.103, which implies that at the median ZIP code landlords capture roughly 10 cents of each additional dollar generated by the MW change. The distribution of the shares is skewed to the right. However, we observe a long left-tail with a few negative values which arise due to declines in rents in locations where the increase in the residence MW is much larger than the increase in the workplace MW.

Figure 1.5: Estimated shares pocketed by landlords under counterfactual MW policies

(a) Increase in federal MW to $9, urban ZIP codes

(b) Increase in Chicago MW to $14, Chicago-Naperville-Elgin CBSA



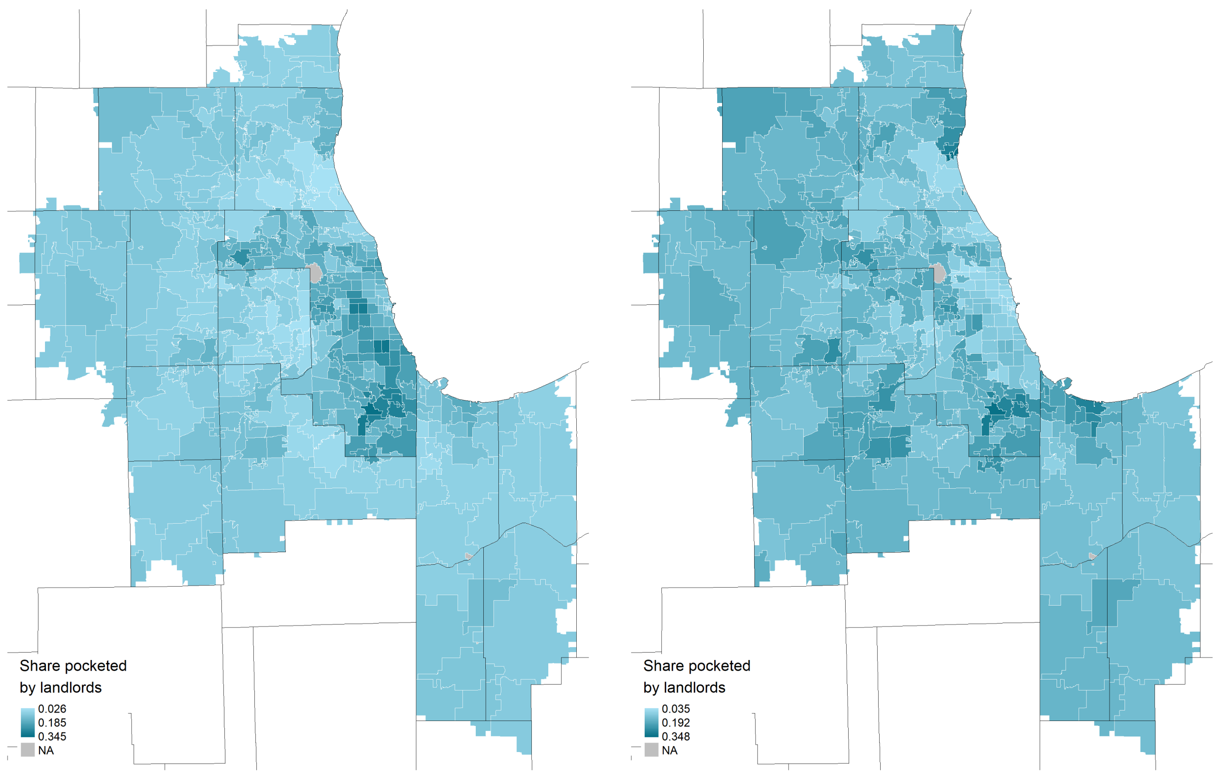
Notes: Data are from the MW panel described in section 1.3.1 and from LODES. The figures show the distribution of the estimated ZIP-code specific shares of additional income pocketed by landlords ("share pocketed") under different counterfactual policies. Panel A is based on a counterfactual increase to $9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. Panel B is based on a counterfactual increase from $13 to $14 in the Chicago City MW, also holding constant other MW policies. The unit of observation is the ZIP code. Panel A includes ZIP codes located in urban CBSAs where the estimated increase in income was higher than 0.1. Panel B includes ZIP codes in the Chicago-Naperville-Elgin CBSA. The share pocketed is defined as the ratio between the percent increase in rents and the percent increase in total wages multiplied by the share of housing expenditure in the ZIP code. To estimate it we follow the procedure described in Section 1.6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$.

Panel A of Figure 1.6 maps the estimated shares in the Chicago-Naperville-Elgin CBSA. Panel A of Appendix Figure 1.P shows estimated increases in rents and wage income. We estimate a larger share pocketed in Cook County. The reason is that these ZIP codes experience the new policy only through their workplace MW and, as a result, rents increase relatively more than wage income. We also observe a larger incidence on landlords in the south of Cook County, where the housing expenditure share is larger.

Figure 1.6: Estimated shares pocketed by landlords under counterfactual MW policies, Chicago-Naperville-Elgin CBSA

(a) Increase in federal MW from \$7.25 to \$9

(b) Increase in Chicago MW from \$13 to \$14



Notes: Data are from the MW panel described in Section 1.3.1 and from LODES. The figures map the estimated ZIP code-specific shares of additional income generated by the MW that are pocketed by landlords, for different counterfactual MW policies. Panel A is based on a counterfactual increase from \$7.25 to \$9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. Panel B is based on a counterfactual increase from \$13 to \$14 in the Chicago City MW, also holding constant other MW policies. The share pocketed is defined as the ratio between the percent increase in rents and the percent increase in total wages multiplied by the share of housing expenditure in the ZIP code. To estimate it we follow the procedure described in Section 1.6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$.

The top rows of Panel A in Table 1.5 show the medians of the key estimated objects for two groups: ZIP codes where the residence MW did not change, and ZIP codes where it did. ZIP codes in the first group see rent increases that are moderated by the negative effect of the residence MW. The median incidence on landlords for this group is 9.7 cents of each dollar. Locations in the second group are only affected through changes in the workplace MW, so median incidence for this group is larger at 15.9 cents of each dollar. The bottom row of Panel A in Table 1.5 shows our estimate of total incidence of the policy, which is given by 0.093. The share is lower than the median values reported earlier because landlords capture more in locations with lower rent increases.

More generally, one can think of the average share for different values of the gap between the residence MW and the workplace MW, i.e., $\Delta \underline{w}_i^{\text{wkp}} - \Delta \underline{w}_i^{\text{res}}$. Figure 1.7 displays the average estimated share for each decile of that gap. We observe a positive and nearly monotonic relation. The share is lower in ZIP codes that had a low increase in the workplace MW relative to the residence MW, highlighting how the share pocketed depends on the incidence of the federal MW increase on the MW measures.

Table 1.5: Median effect of counterfactual minimum wage policies by treatment status

Panel A: Increase in federal MW to \$9, urban ZIP codes

	N	Change in res. MW	Change in wkp. MW	Share of housing exp.	Share Pocketed
Effect in ZIP codes with...					
previous MW $\leq$ \$9	5,741	0.216	0.204	0.214	0.097
previous MW $>$ \$9	1,043	0.000	0.013	0.232	0.159
Total incidence	6,784				0.093

Panel B: Increase in Chicago MW to \$14, Chicago-Naperville-Elgin CBSA

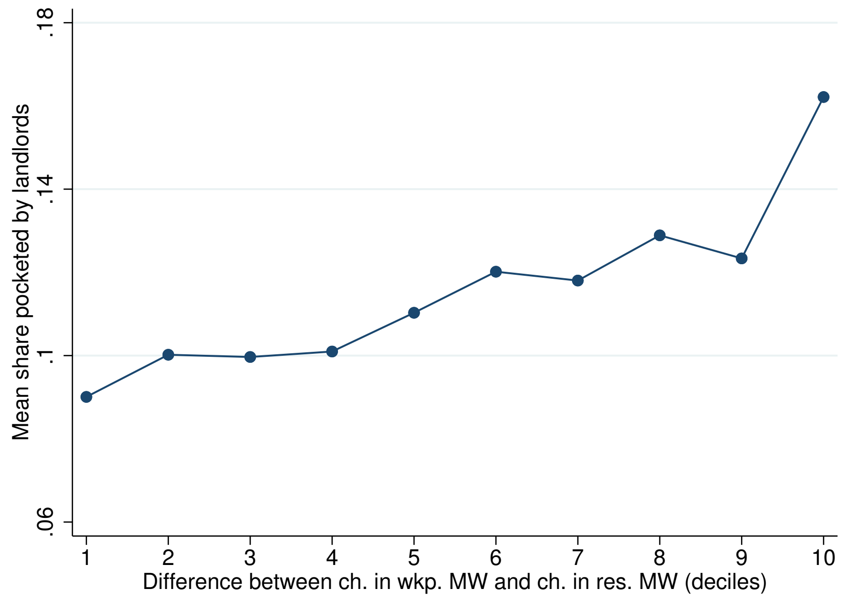
	N	Change in res. MW	Change in wkp. MW	Share of housing exp.	Share Pocketed
Effect in ZIP codes with...					
previous MW $\geq$ \$13	62	0.074	0.046	0.252	0.092
previous MW $<$ \$13	323	0.000	0.009	0.231	0.158
Total incidence	385				0.112

Notes: Data are from LODES origin-destination statistics, Small Area Fair Market Rents, IRS ZIP code aggregate statistics, and the MW panel described in Section 1.3.1. The table shows the median of the estimated ZIP code-specific shares of the additional income pocketed by landlords ("Share pocketed"), defined as the ratio of the increase in income to the increase in rents, for different groups of ZIP codes. Panel A is based on a counterfactual increase from \$7.25 to \$9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. Panel B is based on a counterfactual increase from \$13 to \$14 in the Chicago City MW, also holding constant other MW policies. In the last row of each panel, we report the total incidence of the counterfactual policy. We also report the median change in residence MW, change in workplace MW, and share of ZIP code-specific housing expenditure "Share of housing exp.") defined in Online Appendix 1.B.3. Increases in income and rents are simulated following the procedure described in Section 1.6. We assume the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$. Panel A includes urban ZIP codes only and excludes ZIP codes located in 61 CBAs for which the average estimated change in log total wage income was below 0.1. Panel B includes all ZIP codes with valid data in the Chicago-Naperville-Elgin CBSA.

Local increase. Panel B of Figure 1.5 shows the distribution of the estimated shares in the Chicago-Naperville-Elgin CBSA. Panel B of Table 1.5 displays median values for ZIP codes inside the city and outside it. The incidence on landlords is of 9.2 cents of each dollar for the median directly treated ZIP code and of 15.8 cents for the median not directly treated one.

Panel B of Figure 1.6 maps the shares. Panel B of Appendix Figure 1.P shows the estimated changes in rents and total wages. Unlike the previous exercise, the share pocketed by landlords is now higher right outside of Chicago City. Many commuters reside there, and thus the workplace MW changes the most. This translates into higher rent increases, implying a large share pocketed.[31]

Figure 1.7: Share pocketed by landlords by intensity of treatment, urban ZIP codes under federal MW increase to $9



Notes: Data are from the MW panel described in Section 1.3.1 and from LODES. The figure shows the average estimate of the shares of additional income pocketed by landlords ρ_i for each decile of the difference $\Delta\underline{w}_i^{\text{wkp}} - \Delta\underline{w}_i^{\text{res}}$. Estimates for lower deciles correspond to ZIP codes where the increase in residence MW was relatively large. The unit of observation is the urban ZIP code, where we define a ZIP code as urban if it belongs to a CBSA with at least 80% of its population classified as urban by the 2010 Census. The share pocketed is defined as the ratio between the percent increase in rents and the percent increase in total wages multiplied by the share of housing expenditure in the ZIP code. To estimate it we follow the procedure described in Section 1.6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1013$. The figure excludes ZIP codes located in the 61 CBAs for which the average estimated change in log total wages was below 0.1.

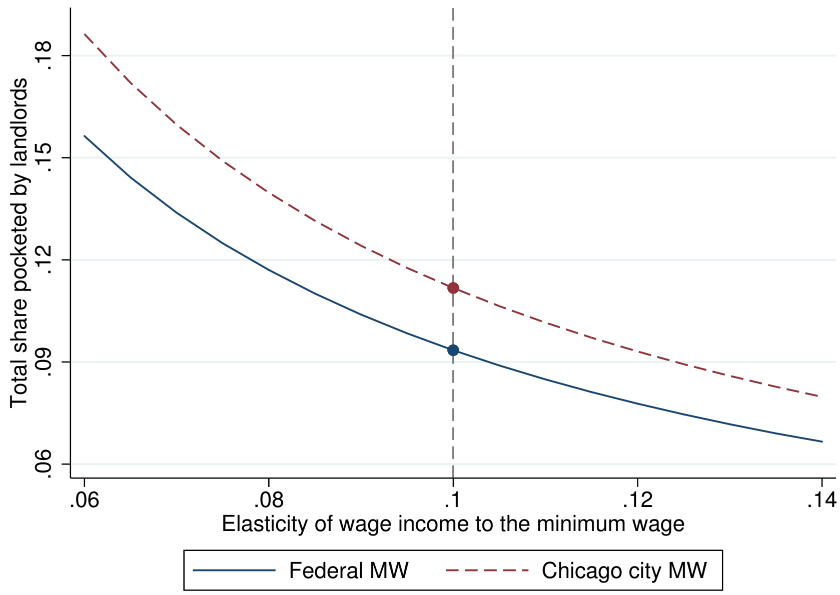
Sensitivity to ε. Our estimates of the incidence of the policy depend on the value of the income elasticity ε. Figure 1.8 displays total share pocketed by landlords for different values of ε, both for the federal and the local counterfactual policies. As expected, the share pocketed is decreasing in ε. For instance, if we assume $\varepsilon = 0.06$ instead of 0.1, then for the federal policy the share pocketed would be about 0.16 cents and for the local policy it would be about 0.19 cents.

1.6.3 Discussion

Overall, we observe that landlords capture a significant portion of the income generated by MW policies. We also found strong spatial heterogeneity in incidence depending on commuting patterns. The share pocketed

[31] It is worth emphasizing that we estimate large increases in wage income inside the city due to the fact that our model in (1.7) excludes heterogeneity based on the share of MW workers. In a setting where this equation accounts for the share of MW workers we would not expect a strong effect on wages inside the city.

Figure 1.8: Estimated shares pocketed by landlords for different values of the elasticity of wage income to the MW



Notes: Data are from the MW panel described in section 1.3.1 and from LODES. The figures show the estimated ZIP-code specific share of additional income pocketed by landlords ("share pocketed") under different counterfactual policies: an increase to \$9 in the federal MW in January 2020, and an increase from \$13 to \$14 in the Chicago City MW, both holding constant other MW policies. The unit of observation is the ZIP code. To estimate it we follow the procedure described in Section 1.6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$. The x-axis shows a range of values for the elasticity of wage income to the minimum wage ε. The line at $\epsilon = 0.1$ corresponds to the estimates reported in Table 1.5.

by landlords tends to be larger in ZIP codes located in jurisdictions where the MW policy did not change, particularly those located close to the MW change as many of their residents work under the new MW level and experience no change in the residence MW. According to the model in Section 1.2, the mechanism behind this result is the offsetting effect of increases in prices of non-tradable consumption in the same location.

Because of the housing market, the impact of the MW will be less equalizing in terms of the distribution of real incomes than nominal incomes. There are many reasons for this. First, poorer areas tend to have a higher share of expenditure in housing. Second, as we discussed in Section 1.3.2, low-wage households are more likely to rent. Finally, in the case of high-income cities enacting MW policies, affected low-wage workers are more likely to live outside the city where rent increases will be larger.

1.7 Conclusions

We explore whether minimum wage changes affect housing rental prices, and whether MW shocks propagate spatially through commuting. To answer this question we develop a theoretical approach that accounts for the fact that MW workers typically reside and work in different locations. Our model suggests that MW changes at workplaces will tend to increase rents, and highlights the importance of accounting for the MW at the residence location when estimating the effect of the workplace MW on rents.

We collect data on rents, statutory MW levels, and commuting flows, and estimate the effect of the residence and workplace MW on rents. We find evidence supporting the main conclusions of our model: the workplace MW increases rents, and thus MW policies spill over spatially through commuting. Our conclusions are robust to a variety of robustness checks, and suggest stronger effects in locations that are residence to more MW workers. Our two-parameter model is able to capture rich heterogeneity in the effect of the MW on rents depending on the prevailing commuting structure.

To explore the incidence of the MW on landlords, we explore two counterfactual MW policies. Our results suggest that landlords pocket a non-negligible portion of the newly generated wage income, and that this share varies spatially. Because low-wage households are more affected by MW policies and more likely to be renters, the omission of the housing market channel would lead to an overstatement of the equalizing effects of the MW on disposable income.

Our analysis takes a partial equilibrium perspective, exploring the incidence of small increases in the MW within metropolitan areas. However, one would expect general equilibrium adjustments to large changes in MW levels, such as worker mobility and changes in housing supply. Exploring these issues in the context of a spatial model with worker mobility that distinguishes between renters and homeowners appears as a fruitful avenue for future work.

APPENDIX

1.A Model Appendix

1.A.1 Proofs

Proof of Proposition 1. Fully differentiate the market clearing condition with respect to $\ln R_i$ and $\ln \underline{W}_z$ for all $z \in \mathcal{Z}(i)$. Using (1.1) and appropriate algebraic manipulations, one can show that

$$\Big(\eta_i - \sum_z \pi_{iz}\xi^R_{iz}\Big) d\ln R_i = \sum_z \pi_{iz}\left(\xi^P_{iz}\epsilon^P_i d\ln \underline{W}_i + \xi^Y_{iz}\epsilon^Y_{iz} d\ln \underline{W}_z\right), \tag{1.8}$$

where $\xi_{iz}^x = \frac{dh_{iz}}{dx_i} \frac{x_i}{\sum_z \pi_{iz} h_{iz}}$ for $x \in \{R, P\}$ is the elasticity of the per-capita housing demand with respect to x evaluated at the average per-capita demand of ZIP code i, $\xi_{iz}^Y = \frac{dh_{iz}}{dY_z} \frac{Y_z}{\sum_z \pi_{iz} h_{iz}}$ represents the analogous elasticity with respect to income Y from each workplace z, $\epsilon_i^P = \frac{dP_i}{d\underline{W}_i} \frac{\underline{W}_i}{P_i}$ and $\epsilon_{iz}^Y = \frac{dY_z}{d\underline{W}_z} \frac{\underline{W}_z}{Y_z}$ are elasticities of prices and income to the MW, and $\eta_i = \frac{dS_i}{dR_i} \frac{R_i}{S_i}$ is the elasticity of housing supply in ZIP code i.

For any $z' \in \mathcal{Z}_0 \setminus \{i\}$ the partial effect on rents of the policy is given by

$$\frac{d \ln R_i}{d \ln \underline{W}_{z'}} = \left(\eta_i - \sum_z \pi_{iz} \xi_{iz}^R \right)^{-1} \pi_{iz'} \xi_{iz}^Y \epsilon_{iz'}^Y.$$

Because $\eta_i \geq 0$ and $\xi_{iz}^R < 0$ for all $z \in \mathcal{Z}(i)$, the first factor is positive. From Assumptions 1 and 2, $\epsilon_{iz}^Y \geq 0$ and $\xi_{iz}^Y > 0$. Therefore, the effect is positive if for z' we have $\frac{dY_{z'}}{d\underline{W}_{z'}} > 0$ (or $\epsilon_{iz'}^Y > 0$), and the effect is zero otherwise.

For ZIP code i the partial effect is given by

$$\frac{d \ln R_i}{d \ln \underline{W}_i} = \left(\eta_i - \sum_z \pi_{iz} \xi_{iz}^R \right)^{-1} \left(\epsilon_i^P \sum_z \pi_{iz} \xi_{iz}^P + \pi_{ii} \xi_{ii}^Y \epsilon_{ii}^Y \right).$$

By Assumption 1 we have that $\epsilon_i^P > 0$ and that $\epsilon_{ii}^Y \geq 0$. By Assumption 2 we have that $\xi_{ii}^Y > 0$ and that, for all $z \in \mathcal{Z}(i)$, $\xi_{iz}^P < 0$. Then, the second parenthesis has an ambiguous sign. The third statement of the Proposition follows directly. $\square$

Proof of Proposition 2. Under the stated assumptions we can manipulate (1.8) to write

$$dr_i = \beta_i d\underline{w}_i^{\text{wkp}} + \gamma_i d\underline{w}_i^{\text{res}}$$

where $\beta_i = \frac{\xi_i^Y \epsilon_i^Y}{\eta_i - \sum_z \pi_{iz} \xi_{iz}^R} > 0$ and $\gamma_i = \frac{\sum_{z \in \mathcal{Z}(i)} \pi_{iz} \xi_{iz}^P \epsilon_i^P}{\eta_i - \sum_z \pi_{iz} \xi_{iz}^R} < 0$ are parameters, which signs can be verified using Assumptions 1 and 2. $\square$

1.A.2 A dynamic supply and demand model

The geography is represented by a set of ZIP codes $\mathcal{Z}$. There is an exogenously given distribution of workers with differing residence i and workplace z locations across these ZIP codes which, as in the main body of the paper, we denote by $\{L_{iz}\}_{i,z \in \mathcal{Z} \times \mathcal{Z}}$.

Let H_{it} be the stock of square feet rented in period t. We assume that all contracts last for one year, so that the stock is composed of contracts starting at different calendar months. We impose that $H_{it} \leq S_i$ for all t, where S_i denotes the total number of available square feet in i.

We further decompose H_{it} as follows. Let $h_{izt} = h_{iz}\left(R_{it}, \underline{W}_{it}, \underline{W}_{zt}\right)$ be the per-capita demand of housing of group (i, z) in period t, which depends on the prevailing MW levels at the time of contract sign-up. We assume that this demand function is decreasing in the residence MW and increasing in the workplace MW, just as in Section 1.2. For simplicity, we omitted the mediation channels of prices and income. Let λ_{it} denote the share of i's residents who started their contracts in period t.[32] Then, we can write the stock of contracted square feet during period t as

$$H_{it} = \sum_{\tau=t-11}^{t} \lambda_{i\tau} \sum_{z\in\mathcal{Z}} L_{iz} h_{iz\tau}(r_{i\tau}, \underline{W}_{i\tau}, \underline{W}_{z\tau})$$

where $r_{i\tau}$ represents rents per square foot in period τ, and by assumption $\sum_{\tau=t-11}^{t} \lambda_{i\tau} = 1$. It is convenient to define the stock of contracted square feet excluding the ones that were signed 12 months ago:

$$\tilde{H}_{it} = \sum_{\tau=t-10}^{t} \lambda_{i\tau} \sum_{z\in\mathcal{Z}} L_{iz} h_{iz\tau}(r_{i\tau}, \underline{W}_{i\tau}, \underline{W}_{z\tau}).$$

We assume that all square feet are homogeneous, and so they have the same price in the market.

Within-period equilibrium

We assume the following timing: (1) At the beginning of period t, a share λ_{it} of contracts expire (the ones that started on $t-12$); (2) The square feet from expiring contracts are added to the pool of available rental space for new renters; (3) Renters in t and a flow supply of rental space in t determine equilibrium rents R_{it}. Next, we develop each of these steps more formally.

At the start of every period t, $\lambda_{i,t-12} \sum_z L_{iz} h_{iz,t-12}$ square feet become available for rent from each group of workers (i, z). The square feet available to rent in period t (vacant) are then

$$\lambda_{i,t-12} \sum_z L_{iz} h_{iz,t-12} + (S_i - H_{i,t-1}) = S_i - \tilde{H}_{i,t-1}.$$

Note that this differs from $S_i - H_{i,t-1}$, the non-rented square feet as of $t-1$. We denote by $V_{it}(R_{it}, \lambda_t)$ the supply of housing, increasing in R_{it}. A feasibility constraint is that

$$V_{it}(R_{it}, \lambda_t) \leq S_i - \tilde{H}_{i,t-1}. \tag{1.9}$$

[32] We assume that these shares do not vary by workplace.

The flow demand for new rentals in t by those whose contract expired is given by

$$\lambda_{it} \sum_{z} L_{iz} h_{izt} \left(R_{it}, \underline{W}_{it}, \underline{W}_{zt} \right).$$

This demand arises because a share of the ZIP code's contracts expired. Those workers go to the market and may desire to rent more square feet given changes in their income.

The market in period t clears if

$$\lambda_t \sum_{z} L_{iz} h_{iz} \left(R_{it}, \underline{W}_{it}, \underline{W}_{zt} \right) = V_{it}(R_{it}, \lambda_t). \tag{1.10}$$

Given statutory MW levels in t, $\{\underline{W}_{it}\}_{i \in \mathcal{Z}}$, the share of workers looking to rent in period t, λ_t, and a number of vacancies that satisfies (1.9), equation (1.10) determines equilibrium rents in period t. Because the properties of housing demand and housing supply are the same as in the model in Section 1.2, the equilibrium condition (1.10) implies an analogue of Propositions 1 and 2. The results in Section 1.2 can be extended to a dynamic setting if the demand and supply functions in t only depend on MW levels in t.

1.A.3 A static model with flexible commuting shares

In this section we use a simplified version of the partial-equilibrium model in Section 1.2. We do so to focus on the implications of relaxing the assumption of fixed commuting shares.

Assume that housing demand depends directly on the workplace and residence MW, abstracting away from the mediation channels of prices and income. Let commuting shares depend on the MW at the respective workplace location. Then, the housing market equilibrium can be written as

$$L_i \sum_{z \in \mathcal{Z}(i)} \pi_{iz}(\underline{W}_z) h_{iz} \left(R_i, \underline{W}_i, \underline{W}_z \right) = S_i(R_i). \tag{1.11}$$

Following empirical results in Pérez Pérez (2021), we assume that an increase in the workplace MW may decrease the share of workers traveling to a given destination.

Assumption 3 (Endogenous commuting shares)**.** *Commuting shares of location i's residents are given by $\{\pi_{iz}(\underline{W}_z)\}_{z \in \mathcal{Z}(i)}$, where $d\pi_{iz}/d\underline{W}_z \leq 0$.*

With this assumption we are ready to prove the following result.

Proposition 3 (Representation with endogenous shares)**.** *Assume that for all ZIP codes $z \in \mathcal{Z}(i)$ we* *have (a) homogeneous elasticity of per-capita housing demand to the MW at workplace locations,* $\xi_{iz}^{wkp} = \xi_i^{wkp}$*, and (b) homogeneous elasticity of commuting shares to the MW,* $\zeta_{iz} = \zeta_i$*. Then, approximating* $\pi_{iz}h_{iz}/\sum_{z'}\pi_{iz'}h_{iz'} \approx \pi_{iz}$*, we can write*

$$dr_i = (\beta_i + \zeta_i)\, d\underline{w}_i^{wkp} + \gamma_i d\underline{w}_i^{res}$$

where $r_i = \ln R_i$*,* $\underline{w}_i^{wkp} = \sum_{z\in\mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_z$ *is ZIP code* i*'s* ***workplace MW****,* $\underline{w}_i^{res} = \ln \underline{W}_i$ *is ZIP code* i*'s* ***residence MW****, and* $\beta_i > 0$*,* $\zeta_i < 0$*, and* $\gamma_i < 0$ *are parameters.*

Proof. Fully differentiate the market clearing condition with respect to $\ln R_i$ and $\ln \underline{W}_z$ for all $z \in \mathcal{Z}(i)$. Using (1.11) and appropriate algebraic manipulations, one can show that

$$\left(\eta_i - \sum_z \pi_{iz}\xi_{iz}^R\right) d\ln R_i = \left(\sum_z \pi_{iz}\xi_{iz}^{\text{res}}\right) d\ln \underline{W}_i + \sum_z \pi_{iz}(\xi_{iz}^{\text{wkp}} + \zeta_{iz}) d\ln \underline{W}_z, \tag{1.12}$$

where we also impose the approximation that $\pi_{iz}h_{iz}/\sum_{z'}\pi_{iz'}h_{iz'} \approx \pi_{iz}$. In this expression $\xi_{iz}^R = \frac{dh_{iz}}{dR_i}\frac{R_i}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of housing demand to rents, $\xi_{iz}^{\text{res}} = \frac{dh_{iz}}{d\underline{W}_i}\frac{\underline{W}_i}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of housing demand to the MW at i, $\xi_{iz}^{\text{wkp}} = \frac{dh_{iz}}{d\underline{W}_z}\frac{\underline{W}_z}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of housing demand to the MW at workplace z, and $\zeta_{iz} = \frac{d\pi_{iz}}{d\underline{W}_z}\frac{\underline{W}_z}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of commuting shares to the MW at z.

Under the stated assumptions we can manipulate (1.12) to write

$$dr_i = (\beta_i + \zeta_i) d\underline{w}_i^{\text{wkp}} + \gamma_i d\underline{w}_i^{\text{res}} \tag{1.13}$$

where $\beta_i = \frac{\xi_i^{\text{wkp}}}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} > 0$, $\zeta_i = \frac{\zeta_i}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} \le 0$, and $\gamma_i = \frac{\sum_z \pi_{iz}\xi_{iz}^{\text{res}}}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} < 0$ are parameters. The sign of ζ_i is given by Assumption 3. □

This result implies that, up to an approximation, the response of rents to the workplace MW includes the negative effect of the MW on commuting shares.

1.B Data Appendix

1.B.1 Matching census blocks to USPS ZIP codes

One challenge of this project is that LODES data on commuting patterns are aggregated at the level of the *census block*. However, Zillow data are aggregated at the level of *USPS ZIP codes*, and blocks and ZIP codes are not nested. In this appendix section we describe the steps we took to construct a correspondence table between these geographies.

First, we collected the GIS map of 11,053,116 blocks from US Census Bureau (2012) and computed their centroids. Second, we assigned each block to a unique ZIP code using the GIS map from ESRI (2020) based on assigning to each block the ZIP code that contains its centroid. If the centroid falls outside the block, we pick a random point inside it. We assigned 11,013,203 blocks using this spatial match (99.64 percent of the total).[33] Third, for the blocks that remain unassigned we used the tract-to-ZIP-code correspondence from US Department of Housing and Urban Development (2022b). For each tract we keep the ZIP code where the largest number of houses of the tract fall, and we assign it to each block using the tract identifier. We assigned 22,819 blocks using this approach (0.21 percent). There remain 17,094 unassigned blocks (0.15 percent), which we drop from the analysis. This creates a unique mapping from census blocks to ZIP codes.

In the end, there are 11,036,022 blocks which are assigned to 31,754 ZIP codes, implying an average of 347.55 blocks per ZIP code. Thus, even though there may be blocks that go beyond one ZIP code, we expect the error introduced by this process to be very small.

1.B.2 Assigning minimum wage levels to USPS ZIP codes

Our main rents data is aggregated at the level of the USPS ZIP code. To match this geographical level, we assign statutory MW levels to ZIP codes. ZIP codes usually cross jurisdictions, and as a result parts of them are subject to different statutory MW levels. Trying to overcome this problem, we assign averages of the relevant MW levels to each ZIP code.

We proceed as follows. First, we collect a census crosswalk constructed by US Census Bureau (2021) that contains, for each block, identifiers for block group, tract, county, CBSA (i.e., Core-Based Statistical Area), place (i.e., Census-Designated Place), and state. Second, we assign the MW level of each jurisdiction to the relevant block. We use the state code for state MW policies, and we match local MW policies based on the names of the county and the place. We define the statutory MW at each census block as the maximum of the federal, state, county, and place levels. Then, based on the original correspondence table described in Online Appendix 1.B.1, we assign a ZIP code to each block. Finally, we define *the statutory MW* for ZIP code i and month t, $\underline{W}_{it}$, as the weighted average of the statutory MW levels in its constituent blocks, where the weights are given by the number of housing units.[34] For ZIP codes that have no housing units in them, such as those corresponding to universities or airports, we use a simple average instead.

[33] 545,566 of ZIP codes assigned via spatial match use a point of the census block picked at random (4.94 percent of the total).
[34] ZIP codes between 00001 and 00199 correspond to federal territories. Thus, we assign as statutory MW the federal level.

Locating minimum wage earners. We approximate the share of people that earn at or below the MW as follows. First, we collect data on the number of workers in each tract from the 5-year 2010-2014 American Community Survey (US Census Bureau, 2022a). Using our assignment of hourly statutory MW levels in January 2014 we compute the total yearly wage of a full-time worker earning the MW in each tract, which we denote by $\underline{YW}$.[35] We keep track of what wage bin $\underline{YW}$ falls into. We estimate the number of MW earners in a tract as the total number of workers in all bins below the one where $\underline{YW}$ falls plus a fraction of the total number of workers in the bin $\underline{YW}$ falls given by $(\underline{YW} - b_\ell) / (b_h - b_\ell)$, where b_h and b_ℓ represent the upper and lower limits of the bin. We impute the tract estimates to ZIP codes proportionally to the share of houses in each tract that fall in every ZIP code the tract overlaps with.[36] Finally, we compute the share of MW workers who reside in each ZIP code dividing our estimate of the number of MW workers by the total number of workers in the data. Due to limitations in the ACS data, it is not possible to use the MW at workplace locations in the computation, nor to estimate the share of MW workers by workplace.

1.B.3 Measuring housing expenditure at the ZIP code level

For our counterfactual exercises we require several pieces of information. First, to estimate the overall incidence of a MW policy we need the levels of total wages and total housing expenditure in each location. Second, to estimate the ZIP code-specific incidence, we require a housing expenditure share that varies by ZIP code. We construct these measures for 2018 using data from the Internal Revenue System (2022b) and the US Department of Housing and Urban Development (2020b).

To construct these data we proceed as follows. We approximate the levels of total wages and housing expenditure using per household variables. From the IRS we obtain annual wage per household, which we divide by 12 to obtain a monthly measure. From the HUD, we use the 2-bedroom SAFMR series as our monthly housing expenditure variable.[37] We define the ZIP code-specific housing share as the ratio of these two variables. The computed variables have several missing values across the entire US, and small percentage of missing values within urban CBSAs (as defined in Table 1.1). We impute missing values independently for each variable using an OLS regression based on sociodemographic characteristics of each ZIP code (including data from the US Census and LODES) and CBSA by county fixed effects. To limit the

[35] We use the definition of full-time workers from Internal Revenue System (2022a). Specifically, we assume that a full-time employee works for 130 hours per week for 12 months.

[36] More precisely, we compute a tract-to-ZIP-code correspondence from the LODES correspondence between blocks and tracts, available in US Census Bureau (2021), and the geographical match between blocks and ZIP codes from Online Appendix 1.B.1. For each tract, we compute the share of houses that fall in each ZIP code, and we assume that the share in the tract-ZIP code combination equals the share of houses times the estimated number of MW workers in the tract.

[37] Average rents in a location would be better approximated as a weighted average of rents for houses with different number of bedrooms, weighted by the share of households that rent each type of housing. However, these data are not publicly available.

influence of outliers, we winsorize the results at the 0.5th and 99.5th percentiles. The percentage of urban ZIP codes with non-imputed housing expenditure shares is 93.2.

1.B.4 Posted rents and contract rents: Quora question

We asked the following question: "How different is the rent paid by a tenant and the rent posted online of the same housing unit? Do tenants have space to bargain the posted price, or is it common for tenants to just accept the posted price?" The online version of the question can be accessed at this link.

Landlords

- *As a landlord for over 40 years, I have never agreed to negotiate the rental price. I would rather lose that renter and later lower the list price and rent to someone else. ...*
- *... The rent posted should be the actual rent available. As far as comparing the rent to a newly available listed property and one that is being occupied by another existing tenant, they will likely differ as the market continues to evolve and inflation has an impact on everything as well as the law of supply and demand. ...*
- *I've been a landlord for over 35 years, in three states and in three countries. I do NOT "bargain," and anyone trying would find their application in the round file, instantly.*

Tenants

- *Where I am, you accept the posted price unless there is something very odd about the unit or you have a needed skill to offer. Both are very rare.*

  The payoff is that landlords often increase rent for renewing tenants at a slightly lower rate than they set for a similar empty unit. If you stay many years, you may have noticeably lower rent. ...
- *Presumably, the landlord has done at least minimal research to figure out how much the market is charging. That market price should bring in multiple potential tenants wanting to rent. Given that, it would be very rare for a landlord to accept lower offers. ...*

From a Real Estate Transaction Coordinator (boldface added by us):

- *That's entirely up to the landlord.*

  When dealing with a property management company or the manager of an apartment complex, they may have limits on what they can do as far as negotiations. I'm not familiar with any in my city who negotiate rent. ***What's posted is the price. Period.***

We mainly deal with private landlords who, of course, have 100% control over whether they are willing to negotiate the rent. ***In 15 years, I've only seen it happen twice.*** *But our clients post the rent at a fair price—generally right in the center of the "fair market value range"—so they have no reason to negotiate.*

What they WILL negotiate, and I've seen it done many times, is how the deposits are collected. ...

1.C Identification in a Potential Outcomes Framework

Following Section 1.2, we assume that the effect of MW policies across locations can be summarized in the residence and workplace MW measures. Thus, we consider the following causal model

$$r_{it} = r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}). \tag{1.14}$$

For this section we represent our dataset as $\left\{\{r_{it}, \underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}\}_{t=\underline{T}}^{\overline{T}}\right\}_{i\in\mathcal{Z}}$. Monthly dates run from $\underline{T}$ to $\overline{T}$ for every unit, and $\mathcal{Z}$ is the set of ZIP codes. We assume that the data are *iid*. We impose no anticipation, so units do not change their pretreatment outcome given future changes in the MW measures.

Every month in which some jurisdiction changes the level of the MW there will be units that are treated directly and units that are treated indirectly. We follow Angrist and Imbens (1995) and Callaway, Goodman-Bacon, and Sant'Anna (2021) to define the treatment effects of interest. We denote a unit's causal response to the residence MW as $\partial r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}})/\partial \underline{w}_{it}^{\text{res}}$, and to the workplace MW as $\partial r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}})/\partial \underline{w}_{it}^{\text{wkp}}$. Let the federal MW level be $\underline{w}^{\text{fed}}$.

Definition 1 (Treatment Effects)**.** *Consider a group with a residence MW level of w^{res} and a workplace MW level of w^{wkp}. Focus on the effect of the workplace MW. The average treatment effect on that group is*

$$ATT^{wkp}(w^{wkp}|w^{res}, w^{wkp}) = \mathrm{E}\left[r_{it}(w^{res}, w^{wkp}) - r_{it}(w^{res}, \underline{w}^{fed})\middle|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = w^{wkp}\right].$$

The average causal response of the same group to the workplace MW is given by

$$ACRT^{wkp}(w^{wkp}|w^{res}, w^{wkp}) = \left.\frac{\partial\,\mathrm{E}\left[r_{it}(w^{res}, l)|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = w^{wkp}\right]}{\partial \underline{w}^{wkp}}\right|_{l=w^{wkp}}.$$

These treatment effects may be heterogeneous across the distribution of $(\underline{w}_{it}^{res}, \underline{w}_{it}^{wkp})$. The average causal *response across all groups treated with different levels of the workplace and residence MW is*

$$ACR^{wkp}(w^{wkp}) = \frac{\partial \, \mathrm{E}\left[r_{it}(w^{res}, w^{wkp})\right]}{\partial w^{wkp}}.$$

Analogously, for the residence MW we define: ATT^{res}, $ACRT^{res}(w^{res}|w^{res}, w^{wkp})$, *and* $ACR^{res}(w^{res})$.

Our main interest lies in the rent gradient to the MW, i.e., the average causal response of rents to each of the MW measures. For that, we make a parallel trends assumption.

Assumption 4 (Parallel trends)**.** *We assume that, for all levels of* w^{res} *and* w^{wkp},

$$\begin{aligned}
&\mathrm{E}\left[r_{it}(\underline{w}^{fed}, \underline{w}^{fed}) - r_{i,t-1}(\underline{w}^{fed}, \underline{w}^{fed})\big|\underline{w}^{res}_{it} = w^{res}, \underline{w}^{wkp}_{it} = w^{wkp}\right] \\
&\qquad = \mathrm{E}\left[r_{it}(\underline{w}^{fed}, \underline{w}^{fed}) - r_{i,t-1}(\underline{w}^{fed}, \underline{w}^{fed})\big|\underline{w}^{res}_{it} = w^{res}, \underline{w}^{wkp}_{it} = \underline{w}^{fed}\right] \\
&\qquad = \mathrm{E}\Big[r_{it}(\underline{w}^{fed}, \underline{w}^{fed}) - r_{i,t-1}(\underline{w}^{fed}, \underline{w}^{fed})\big|\underline{w}^{res}_{it} = \underline{w}^{fed}, \underline{w}^{wkp}_{it} = w^{wkp}\Big].
\end{aligned}$$

Assumption 4 states that the untreated outcomes evolve in parallel between ZIP codes experiencing treatment levels $(w^{\text{res}}, w^{\text{wkp}})$ and (a) ZIP codes with the same level of the residence MW but unchanged workplace MW and (b) ZIP codes with the same level of the workplace MW but unchanged residence MW. We further maintain a second assumption.

Assumption 5 (No selection on gains)**.** *We assume that*

$$\frac{\partial ATT^{wkp}(w^{wkp}|w^{res}, l)}{\partial \underline{w}^{wkp}}\Big|_{l=w^{wkp}} = 0 \quad \textit{and} \quad \frac{\partial ATT^{res}(w^{res}|l, w^{wkp})}{\partial \underline{w}^{res}}\Big|_{l=w^{res}} = 0.$$

To identify $ACRT^{\text{wkp}}$ we will compare ZIP codes that received similar levels of the residence MW and different levels of the workplace MW. Analogous comparisons of ZIP codes with different residence MW and similar workplace MW will idenfity $ACRT^{\text{res}}$.

Proposition 4 (Identification)**.** *Under Assumption 4 we have that*

$$\begin{aligned}
\frac{\partial \, \mathrm{E}\Big[r_{it}(w^{res}, w^{wkp})|\underline{w}^{res}_{it} = w^{res}, \underline{w}^{wkp}_{it} = w^{wkp}\Big]}{\partial \underline{w}^{wkp}} &= ACRT^{wkp}(w^{wkp}|w^{res}, w^{wkp}) \\
&\quad + \frac{\partial ATT^{wkp}(w^{wkp}|w^{res}, l)}{\partial \underline{w}^{wkp}}\Big|_{l=w^{res}}.
\end{aligned}$$

Furthermore, if Assumption 5 holds, then

$$\frac{\partial \, \mathrm{E}\left[r_{it}(w^{res}, w^{wkp})|w^{res}, w^{wkp}\right]}{\partial w^{wkp}} = ACRT^{wkp}(w|w^{res}, w).$$

Analogous expressions hold for the residence MW.

Proof. The setting is analogous to Callaway, Goodman-Bacon, and Sant'Anna (2021) but with two treatment variables. The proof is analogous as well, with the only difference being that one must condition on the residence MW when deriving the expression for the workplace MW, and viceversa. □

As extensively discussed by Callaway, Goodman-Bacon, and Sant'Anna (2021), Assumption 4 is not enough to identify the average causal response in the context of continuous treatments. The gradient of our rents function for the group $(w^{\text{res}}, w^{\text{wkp}})$ is a mix of the average causal response of interest and a "selection bias" term that captures the fact that the treatment for the particular group that received $(w^{\text{res}}, w^{\text{wkp}})$ may be different for other groups at that level of treatment. Assumption 5 imposes that those selection bias terms are zero.[38] We discuss the plausibility of these assumptions in Section 1.4.

Consider now a functional form for (1.14) like the one used in the main analysis:

$$r_{it} = \alpha_i + \tilde{\delta}_t + \gamma \underline{w}_{it}^{\text{res}} + \beta \underline{w}_{it}^{\text{wkp}} + \epsilon_{it}$$

where we exclude the controls for simplicity. It is easy to see, if $\mathrm{E}[\epsilon_{it} | \underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}] = 0$, then both Assumptions 4 and 5 hold under this linear functional form with constant effects. Furthermore, in this case we have that

$$ACRT^{\text{wkp}}(w^{\text{wkp}} | w^{\text{res}}, w^{\text{wkp}}) = ACR^{\text{wkp}}(w^{\text{wkp}} | w^{\text{res}}, w^{\text{wkp}}) = \beta$$

and that

$$ACRT^{\text{res}}(w^{\text{res}} | w^{\text{res}}, w^{\text{wkp}}) = ACR^{\text{res}}(w^{\text{res}} | w^{\text{res}}, w^{\text{wkp}}) = \gamma$$

for any $w^{\text{res}} \geq \underline{w}^{\text{fed}}$ and $w^{\text{wkp}} \geq \underline{w}^{\text{fed}}$.

[38] There are several alternatives to this assumption. See Callaway, Goodman-Bacon, and Sant'Anna (2021, Section 3.3) and discussion therein.

1.D Additional Tables and Figures

Table 1.A: Summary statistics of baseline panel

	N	Mean	St. Dev.	Min	Max
Minimum wage variables					
Statutory MW $\underline{W}_{it}$	80,700	8.56	1.58	7.25	16.00
Residence MW $\underline{w}_{it}^{\text{res}}$	80,700	2.132	0.168	1.981	2.773
Workplace MW $\underline{w}_{it}^{\text{wkp}}$	80,700	2.136	0.163	1.981	2.694
Workplace MW, low-income workers	80,700	2.134	0.161	1.981	2.681
Workplace MW, young workers	80,700	2.135	0.163	1.981	2.707
Median Rents					
SFCC	74,012	1,757.89	901.50	625.00	30,000.00
SFCC per sqft.	80,700	1.32	1.01	0.47	22.20
Log(SFCC per sqft.)	80,700	0.14	0.47	-0.76	3.10
Economic controls					
Avg. wage Business services	80,700	11.19	1.38	6.02	13.39
Employment Business services	80,700	8.71	1.25	4.36	10.96
Estab. count Business services	80,700	7.14	0.31	5.73	8.18
Avg. wage Financial services	80,352	9.01	1.57	2.40	12.39
Employment Financial services	80,700	6.13	1.35	1.61	9.53
Estab. count Financial services	80,352	7.33	0.36	5.89	8.91
Avg. wage Information services	80,688	10.23	1.43	4.75	12.90
Employment Information services	80,700	8.01	1.21	3.66	10.34
Estab. count Information services	80,688	7.31	0.37	6.33	9.16

Notes: This table shows summary statistics of the panel of ZIP codes used in our baseline results, constructed as explained in Section 1.3.3.4. All workplace MW variables use 2017 commuting data from LODES. The workplace MW variables "Workplace MW, low-income workers" and "Workplace MW, young workers" are constructed using data for workers who earn less $1,251 and are aged less than 29, respectively.

Table 1.B: Estimates of the effect of the minimum wage on rents in levels and first differences, baseline sample

	Log rents	
	Levels (1)	First Differences (2)
Residence MW	-0.0432	-0.0199
	(0.1751)	(0.0195)
Workplace MW	0.0376	0.0687
	(0.2033)	(0.0298)
Economic controls	Yes	Yes
P-value autocorrelation test		< 0.0001
R-squared	0.9924	0.0216
Observations	80,340	78,912

Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. Both columns report the results of regressions of the log of median rents per square foot on our MW-based measures. Column (1) presents estimates of a model in levels, including ZIP code and year-month fixed effects. Column (2), presents estimates of a model in first differences, including year-month fixed effects (note that the ZIP code fixed effect drops out). For the model in first differences, we also report the results of an AR(1) auto-correlation test. We proceed as in Wooldridge (2010, Section 10.6.3). First, we compute the residuals of the model estimated in column (2), and we regress those residuals on their lag. Let the auto-correlation coefficient of this model be ϕ. The model in levels is efficient assuming no auto-correlation in the error term, which would imply that the residuals of the first-differenced model are auto-correlated with $\phi = -0.5$. The row "P-value autocorrelation test" reports the p-value of a Wald test of that hypothesis. Standard errors in parentheses are clustered at the state level.

Table 1.C: Estimates of the effect of the minimum wage on rents, stacked sample

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}		
	(1)	(2)	(3)	(4)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	0.5461	0.0051		-0.0444
	(0.0316)	(0.0109)		(0.0174)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$			0.0242	0.0906
			(0.0216)	(0.0391)
Sum of coefficients				0.0463
				(0.0266)
Economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0208
R-squared	0.9763	0.0539	0.0540	0.0540
Observations	98,326	98,326	98,326	98,326

Notes: Data are from Zillow, the MW panel described in Section 1.3.1, LODES origin-destination statistics, and the QCEW. The table mimics the estimates in Table 1.2 using a "stacked" sample. To construct the sample we proceed as follows. First, we define a CBSA-month as treated if in that month there is at least one ZIP code that had a change in the binding MW. We drop events that have less than 10 ZIP codes. For each of the selected CBSA-months we assign a unique event ID. Second, for each event ID we take a window of 6 months, and we keep all months within that window for the ZIP codes that belong to the treated CBSA. If a ZIP code has missing data for some month within the window, we drop the entire ZIP code from the respective event. For each column, we estimate the same regression as the analogous column in Table 1.2 but include event ID by year-month fixed effects.

Table 1.D: Estimates of the effect of the minimum wage on rents including one lag of the dependent variable, baseline sample

	Log rents			
	Levels		First Differences	
	Baseline (1)	Arellano Bond (2)	Baseline (3)	Arellano Bond (4)
Residence MW	-0.0432	-0.0055	-0.0219	-0.0221
	(0.1751)	(0.0298)	(0.0175)	(0.0234)
Workplace MW	0.0376	0.0065	0.0685	0.0702
	(0.2033)	(0.0346)	(0.0288)	(0.0390)
Lagged log rents		0.8421		0.3299
		(0.0179)		(0.0177)
Economic controls	Yes	Yes	Yes	Yes
P-value equality	0.8264	0.8481	0.0514	0.1378
Observations	80,340	80,321	80,241	80,217

Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. All columns show the results of regressions of the log of median rents per square foot on the residence and workplace MW measures. Columns (1) and (2) estimate two-way fixed-effects regressions in levels that include ZIP code and year-month fixed effects. Columns (3) and (4) estimate models in first differences that include year-month fixed effects. All regressions include economic controls (in levels or first differences, respectively) that vary at the county by month and county by quarter levels. Odd-numbered columns are estimated under OLS. Even-numbered columns include the lagged variable of the dependent variable as control, and are estimated using an IV strategy where the first lag is instrumented with the second lag, following Arellano and Bond (1991). The measure of rents per square foot corresponds to the SFCC category from Zillow. Economic controls from the QCEW include the log of the average wage, the log of employment, and the log of the establishment count from the sectors "Information", "Financial activities", and "Professional and business services". Standard errors in parentheses are clustered at the state level.

Table 1.E: Estimates of the effect of the minimum wage on rents, different samples

	Change log rents Δr_{it}		
	Baseline (1)	Reweighted (2)	Unbalanced (3)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0219	-0.0039	-0.0274
	(0.0175)	(0.0128)	(0.0237)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0685	0.0558	0.0528
	(0.0288)	(0.0240)	(0.0299)
P-value equality	0.0514	0.1026	0.1325
R-squared	0.0213	0.0209	0.0309
Observations	80,241	79,701	193,239

Notes: Data are from Zillow, the statutory MW panel described in Section 1.3.1, LODES origin-destination statistics, and the QCEW. Every column shows the results of regressions of the log of median rents per square foot on our MW-based measures. All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county by month and county by quarter levels. The measure of rents per square foot corresponds to the Single Family, Condominium and Cooperative houses from Zillow. Columns (1) and (2) use our baseline sample defined in Section 1.3.3.4. Column (3) uses the unbalanced sample of all ZIP codes with Zillow rents data at any point in time, and controls for quarter-year of entry to the panel by year-month fixed effects. Column (2) re-weights observations so that the sample of ZIP codes in the data (column 3 of Table 1.1) matches the averages of the set of ZIP codes located in urban CBSAs (column 2 of Table 1.1) in the following census variables: share of urban population share of renter-occupied households, and share of white population. Weights for each sample are computed following Hainmueller (2012). Standard errors in parentheses are clustered at the state level.

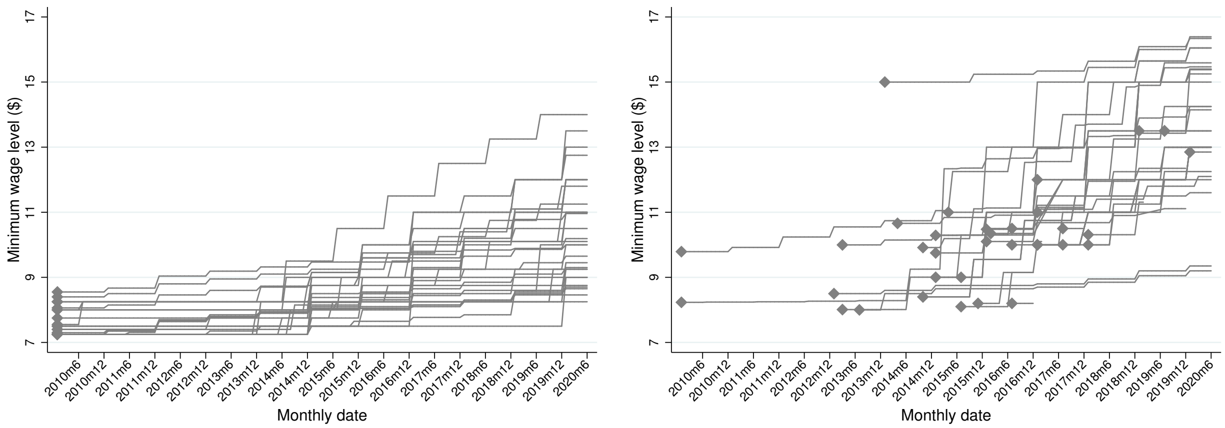
Table 1.F: Comparison of estimates of the effect of the minimum wage on rents across Zillow categories, unbalanced samples

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}			
	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Sum of coefficients	N
(a) Unbalanced (SFCC)	0.8476	-0.0263	0.0479	0.0216	193,292
	(0.0297)	(0.0213)	(0.0302)	(0.0157)	
(b) Single family (SF)	0.8588	-0.0169	0.0429	0.0260	140,750
	(0.0315)	(0.0399)	(0.0477)	(0.0138)	
(c) Condo/Cooperatives (CC)	0.8019	-0.0648	0.0968	0.0320	29,817
	(0.0288)	(0.0266)	(0.0417)	(0.0199)	
(d) Studio	0.8330	-0.0669	0.0776	0.0107	22,746
	(0.0287)	(0.0520)	(0.0570)	(0.0206)	
(d) 1 Bedroom	0.7879	0.0287	-0.0327	-0.0039	53,538
	(0.0300)	(0.0269)	(0.0456)	(0.0208)	
(e) 2 Bedroom	0.8022	-0.0069	0.0063	-0.0006	89,635
	(0.0296)	(0.0232)	(0.0285)	(0.0114)	
(f) 3 Bedroom	0.8113	-0.0645	0.0920	0.0275	64,916
	(0.0322)	(0.0475)	(0.0682)	(0.0328)	
(g) Multifamily 5+ units	0.8072	-0.0133	0.0369	0.0236	142,759
	(0.0314)	(0.0260)	(0.0362)	(0.0115)	

Notes: Data are from Zillow, the statutory MW panel described in Section 1.3.1, LODES origin-destination statistics, and the QCEW. Each row of the table shows two estimations on the same sample of ZIP codes and months. The first column shows the results of a regression of the change in the workplace MW measure on the change in the residence MW measure. The second through fourth columns show the results of a regression of the change in log rents on the change in the residence MW and the workplace MW, with the fifth column showing the sum of the coefficients on the MW measures. All rent variables correspond to the median per square foot rent in a Zillow category. All estimated regressions include quarter of entry to Zillow by year-month fixed effects and economic controls from the QCEW. Row (a) repeats the results of column (5) of Table 1.E, using the Single Family, Condominium and Cooperative Houses category. Rows (b) through (g) estimate the same regression for different Zillow categories. We exclude the rental categories "4 bedroom," "5 bedroom,", and "Duplex and triplex," all of which contain less than 15 thousand ZIP code by month observations. Standard errors in parentheses are clustered at the state level.

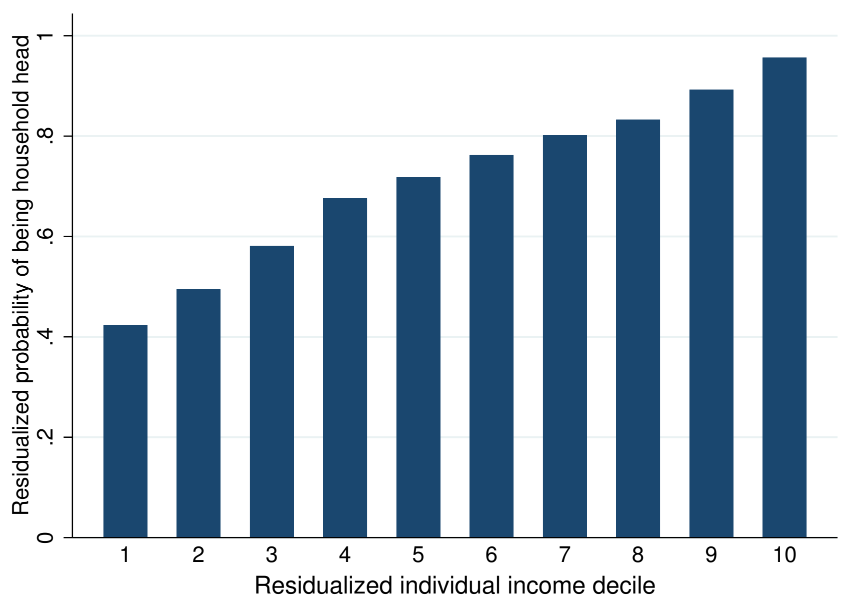
Figure 1.A: Minimum wage levels in the US by jurisdiction between January 2010 and June 2020

(a) State policies (b) Sub-state policies



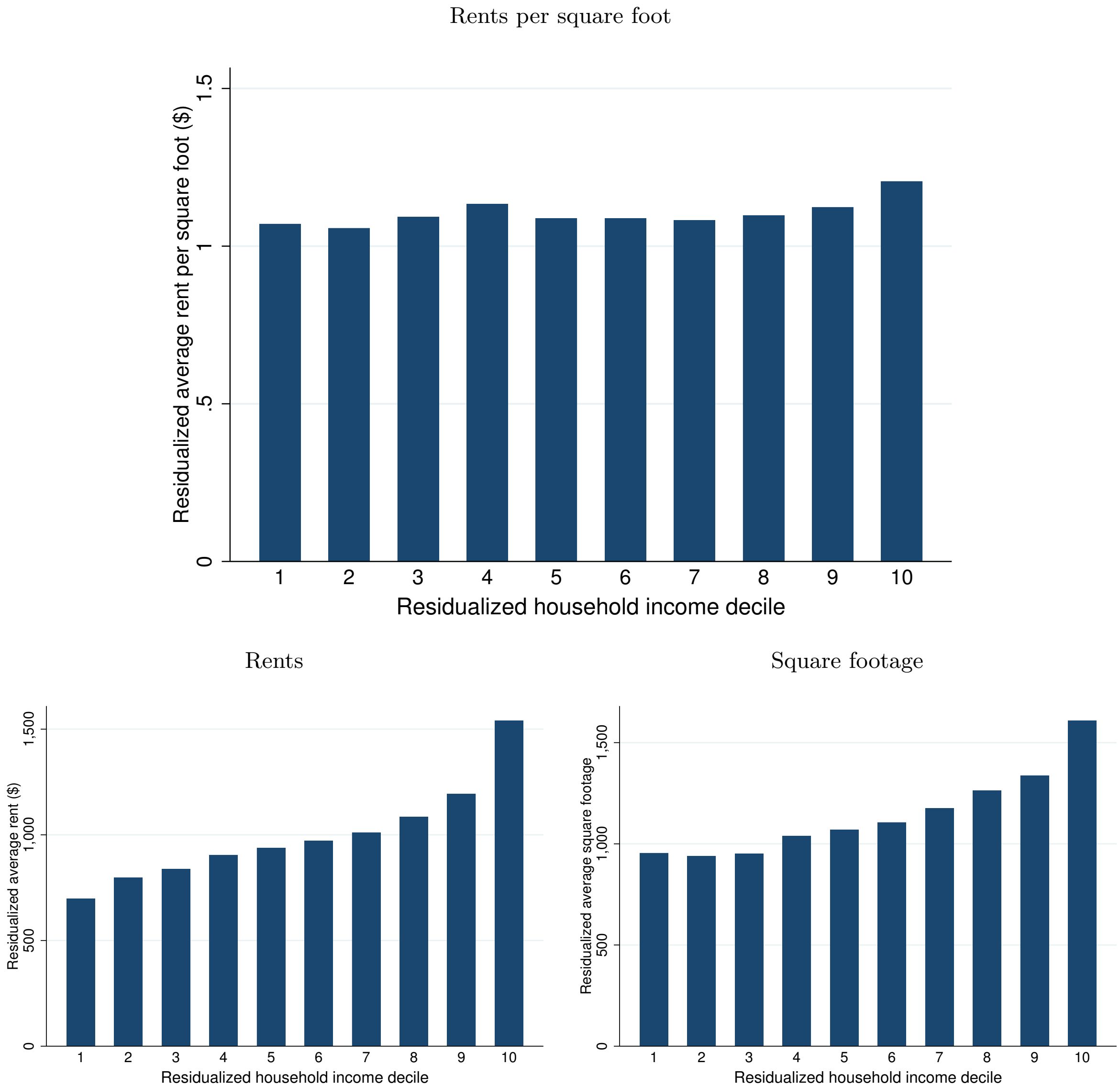
Notes: Data are from the MW panel described in Section 1.3.1. Lines show the levels of the MW for jurisdictional policies that were binding for at least one ZIP code inside them in some month between January 2010 and June 2020. Diamonds indicate the first month the MW policy became operational within the same period. Panel A reports state level policies. Panel B reports sub-state level policies.

Figure 1.B: Probability of being a head of household, by income decile



Notes: Data are from the 2011 and 2013 American Housing Surveys. The figure shows the probability that an individual is a head of household, by individual income decile. We construct the figure as follows. First, we residualize the variable in the y-axis and individual income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized individual income variable. Finally, we take the average of the residualized y-variable within each decile. Individuals that do not work are excluded from the figure.

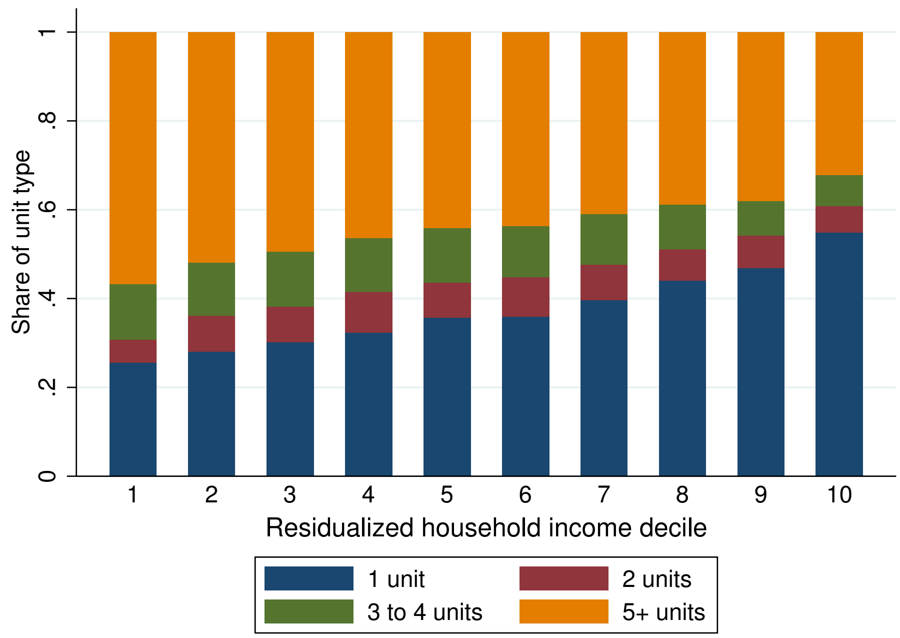
Figure 1.C: Average rent, square footage, and rent per square foot by household income decile, renters sample



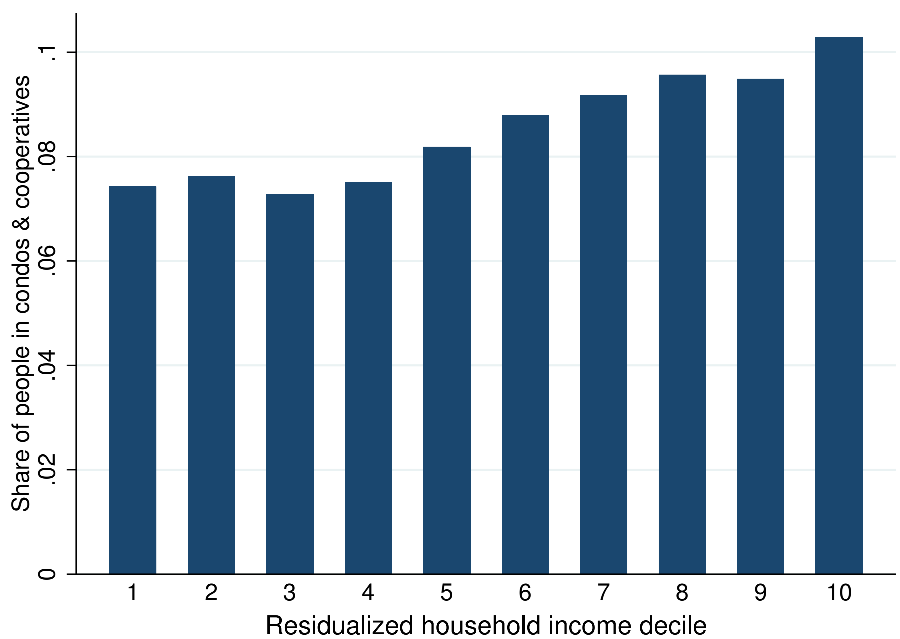
Notes: Data are from the 2011 and 2013 American Housing Surveys. The top figure shows average rents per square foot by household income. The bottom left figure shows average rents by household income. The bottom right figure shows average square feet by household income. The variable rent per square foot is defined as total rental payments divided by total square feet. We construct the figure as follows. First, we residualize the variable in the y-axis and household income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized household income variable. Finally, we take the average of the residualized y-variable within each decile. The sample includes households with non-missing values for square footage and rental payments. We exclude from the calculation non-conventional housing units, such as mobile homes, hotels, and others.

Figure 1.D: Properties of building where household unit is located by household income decile, full sample

Distribution of the number of units in building

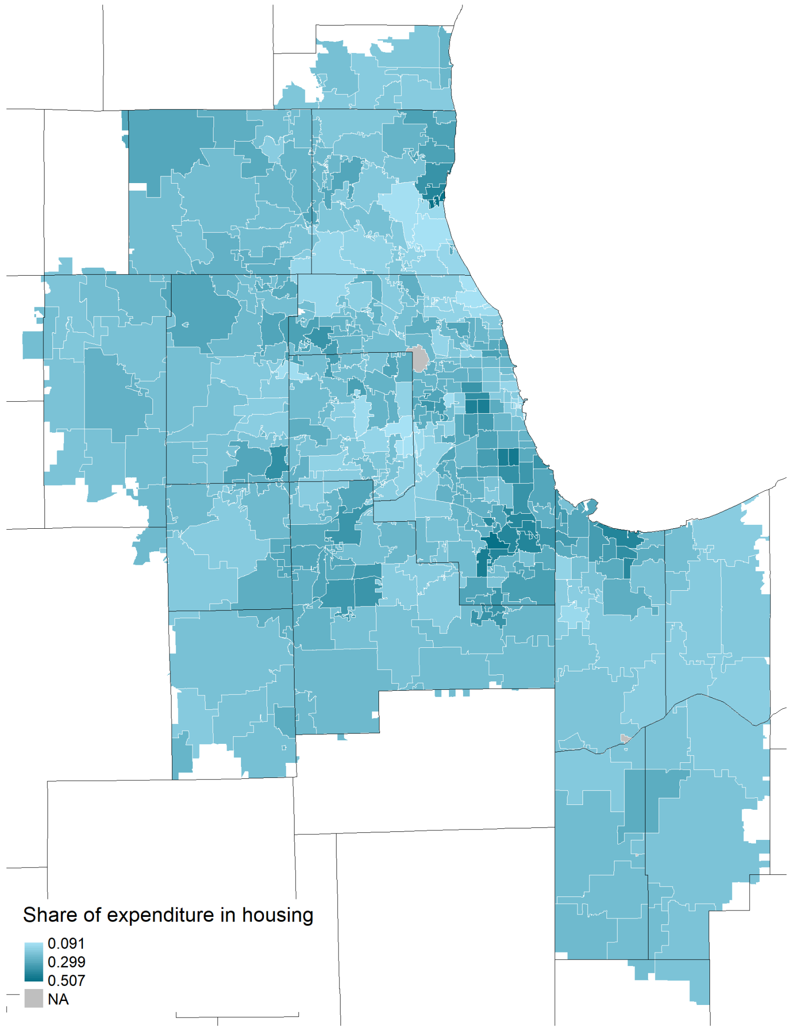


Probability building is a condominium or cooperative housing



Notes: Data are from the 2011 and 2013 American Housing Surveys. The top figure shows the number of housing units in the building where the household is located, and the bottom figure shows the share of housing units located in condominiums or cooperative housing, both by household income. We construct the figure as follows. First, we residualize the variable in the y-axis and household income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized household income variable. Finally, we take the average of the residualized y-variable within each decile. We exclude from the calculation non-conventional housing units, such as mobile homes, hotels, and others.

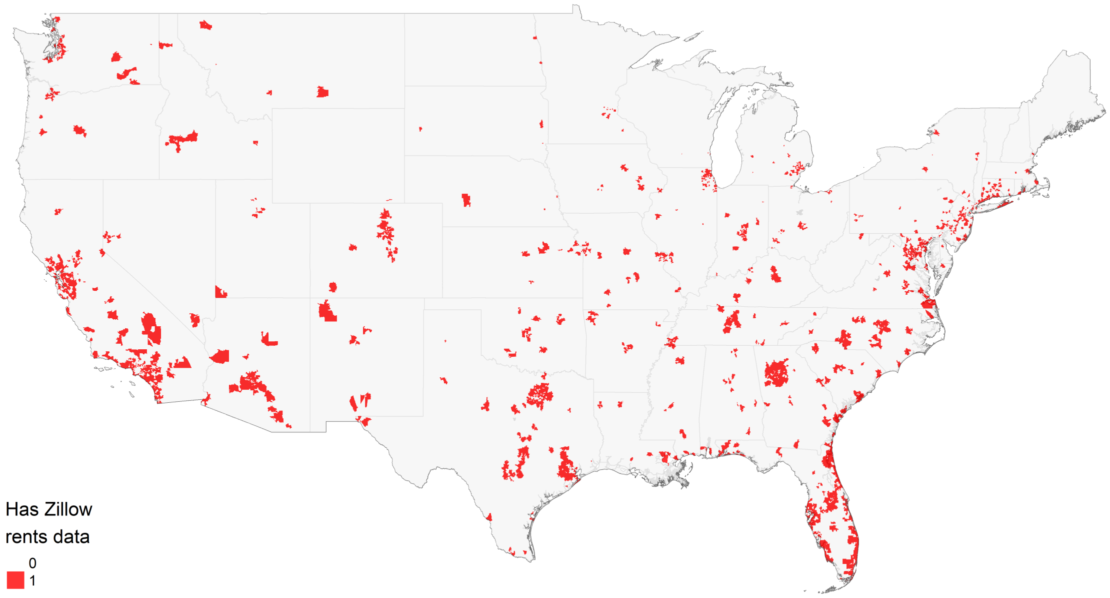
Figure 1.E: Estimated housing expenditure shares in 2018, Chicago-Naperville-Elgin CBSA



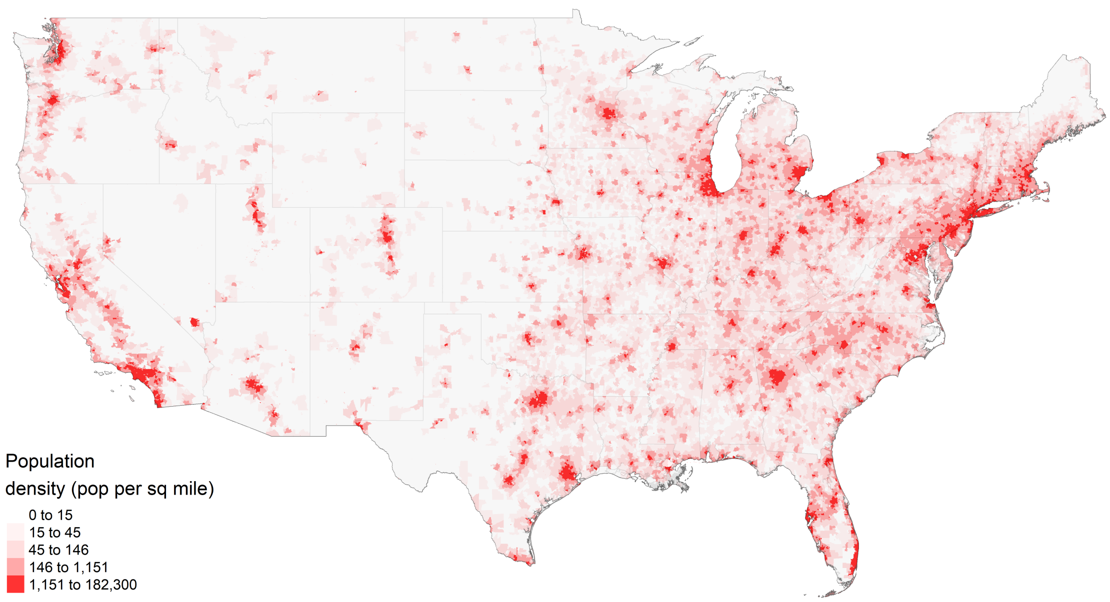
Notes: Data are from the Internal Revenue System (2022b) and the US Department of Housing and Urban Development (2020b). The figure shows housing expenditure shares as computed in Online Appendix 1.B.3, namely, by dividing the SAFMR 40th percentile rental value for a 2-bedroom apartment by average monthly wage per household divided, both for 2018. We include ZIP codes located in the Chicago-Naperville-Elgin CBSA.

Figure 1.F: Sample of ZIP codes in Zillow data and population density, mainland US

Zillow ZIP codes

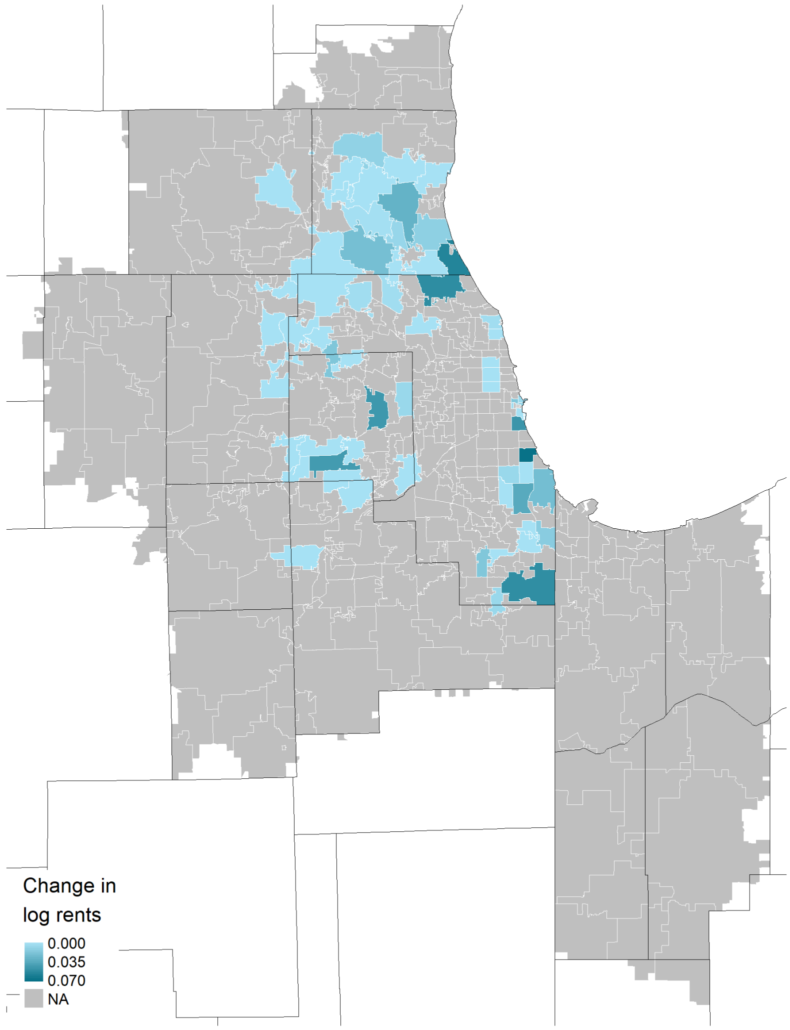


Population Density



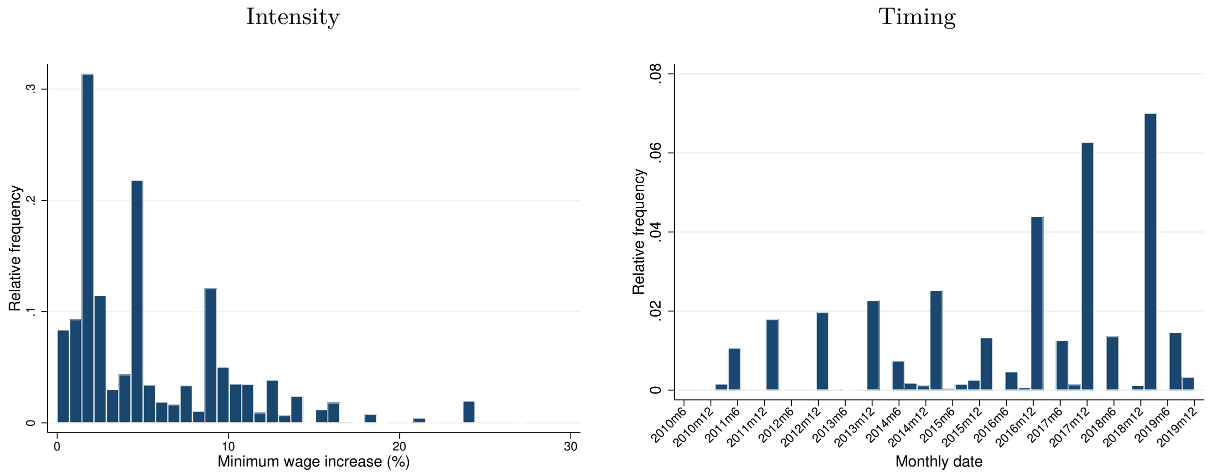
Notes: Data are from Zillow (2020b) and ESRI (2020). The figure compares the sample of ZIP codes available in Zillow to population density at the ZIP code level. The top figure shows the sample of the ZIP codes that have rents data in the SFCC category at any point in the period 2010–2019. The bottom figure shows quintiles of population density according to the 2010 US Census, and measured in population per square mile.

Figure 1.G: Changes in log rents in the Chicago-Naperville-Elgin CBSA, July 2019



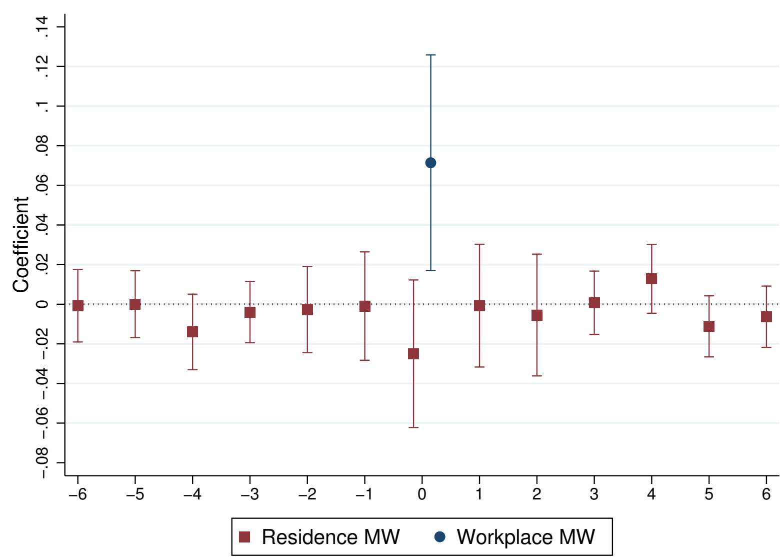
Notes: Data are from Zillow (2020b). The figure shows the change in the log of median rents per square foot in the SFCC category in the month of June 2019 in ZIP codes located in the Chicago-Naperville-Elgin CBSA.

Figure 1.H: Distribution of statutory minimum wage changes, Zillow sample



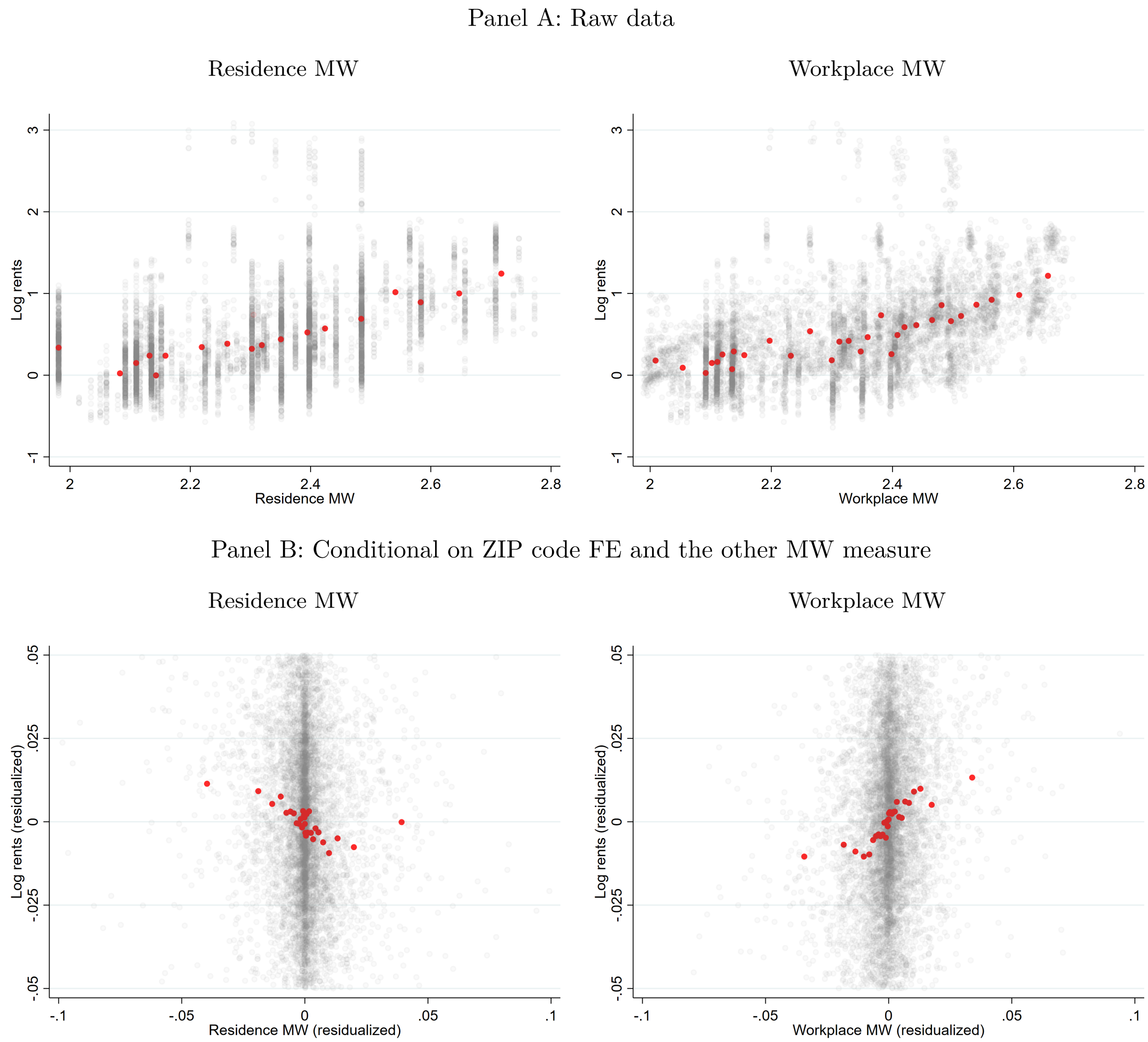
Notes: Data are from the MW panel described in Section 1.3.1. The histograms show the distribution of positive MW changes in the sample of ZIP codes available in the Zillow data. We exclude a few negative changes for expository purposes. The top figure ("Intensity") reports the intensity of the changes in percentage terms. The bottom figure ("Timing") reports the distribution of such changes over time.

Figure 1.I: Estimates of the effect of the minimum wage on rents, baseline sample including leads and lags of the residence MW



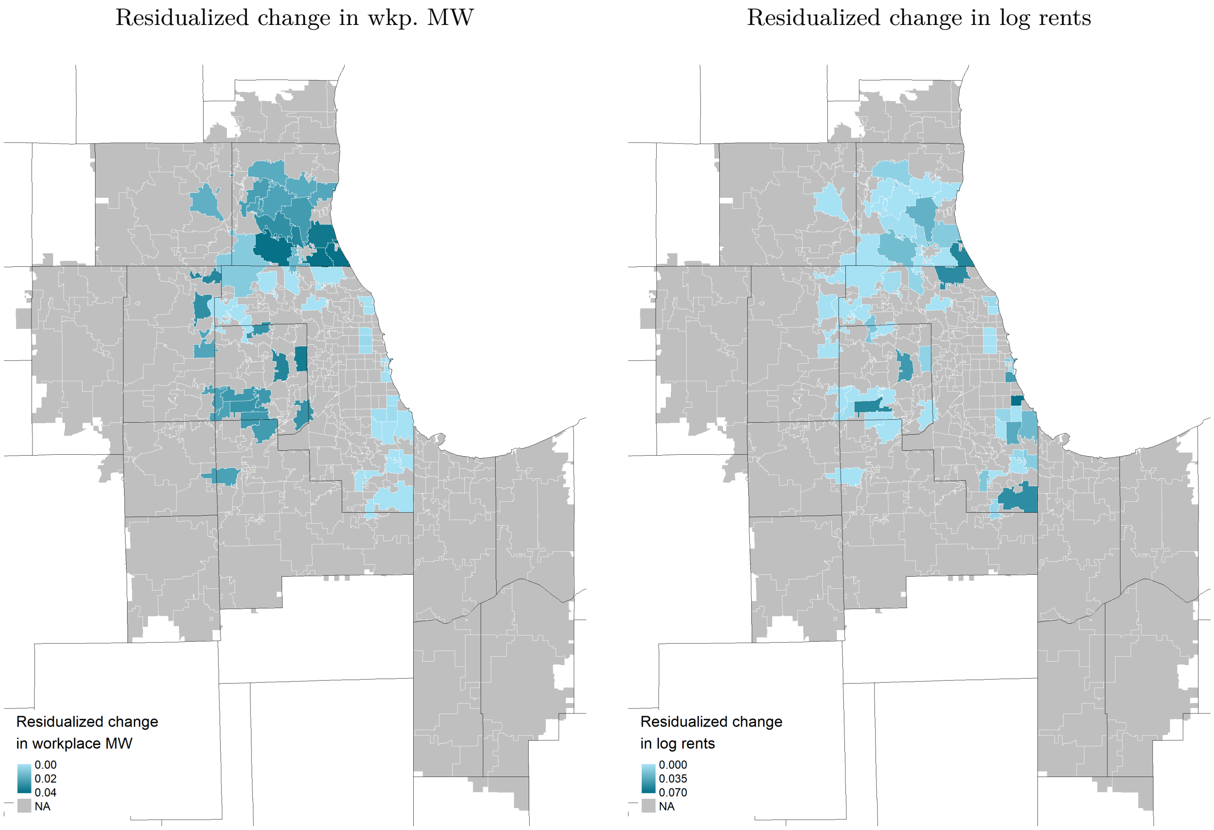
Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. The figure shows coefficients from regressions of the log of rents per square foot on the residence and workplace MW measures, including six leads and lags of the residence MW. All regressions include time-period fixed effects and economic controls that vary at the county by month and county by quarter levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the log statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors "Information," "Financial activities," and "Professional and business services." 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

Figure 1.J: Relationship between log rents and the minimum wage measures, sample of affected ZIP code-months



Notes: Data are from Zillow and LODES. The plot shows the unconditional and conditional relationship between log rents and the MW measures. The sample is composed of ZIP code-month observations located in CBSAs where there was some statutory MW increase in the month of interest. The rents variable correspond to log rents per square foot in the SFCC category in Zillow. The workplace MW measure is constructed using commuting data from the closest prior year. Panel A shows the raw relationship between log rents and workplace and residence MW levels. Panel B shows the same relationship using residuals from regressions on ZIP code indicators and 100 indicators of the other MW measure. Red dots correspond to 30 equally-sized bins of the x-axis variable. Gray dots correspond to all data points in Panel A, and only those data points that fall within the range of the plot axes in Panel B.

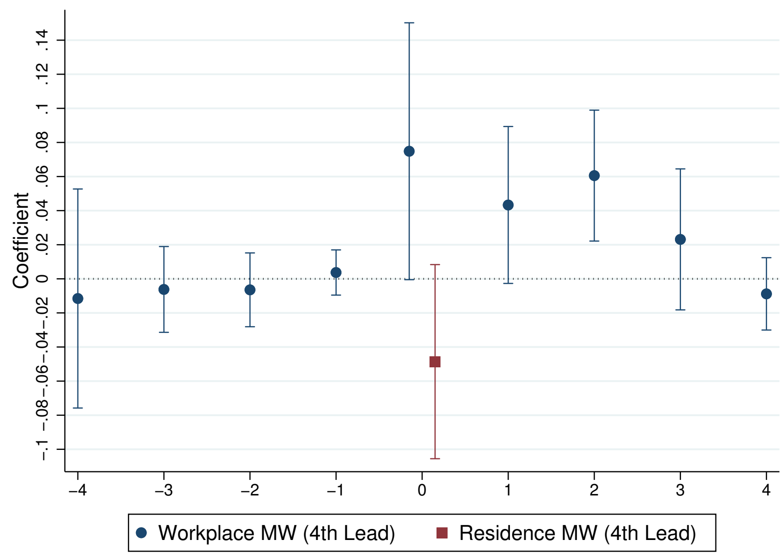
Figure 1.K: Residualized changes in the workplace minimum wage and log rents, Chicago-Naperville-Elgin CBSA on July 2019



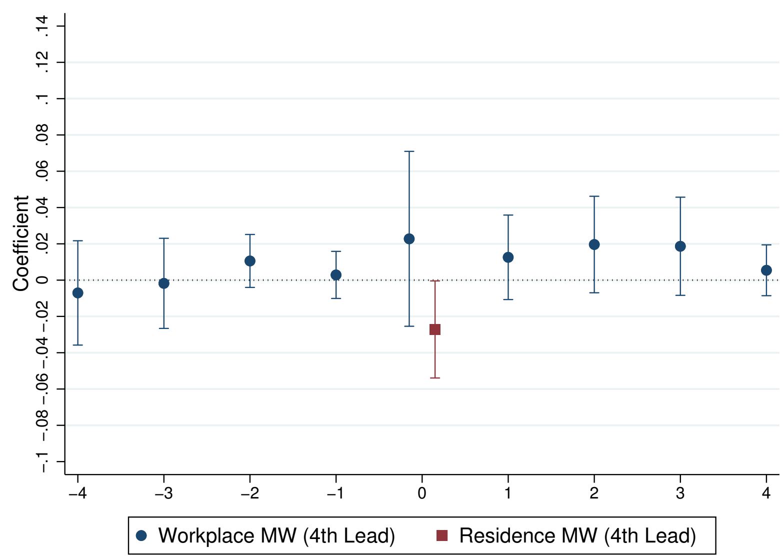
Notes: Data are from the unbalanced estimation panel described in Section 1.3.3.4. The left figure maps the residuals of a regression of the change in the workplace MW measure on the change in the residence MW measure, including economic controls and year-month fixed effects. The right figure maps the residuals of a regression of the change in log rents on economic controls and year-month fixed effects. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors "Information," "Financial activities," and "Professional and business services."

Figure 1.L: Estimates of the effect of the minimum wage on rents, Zillow rental index

(a) Control for year-month by CBSA fixed effects

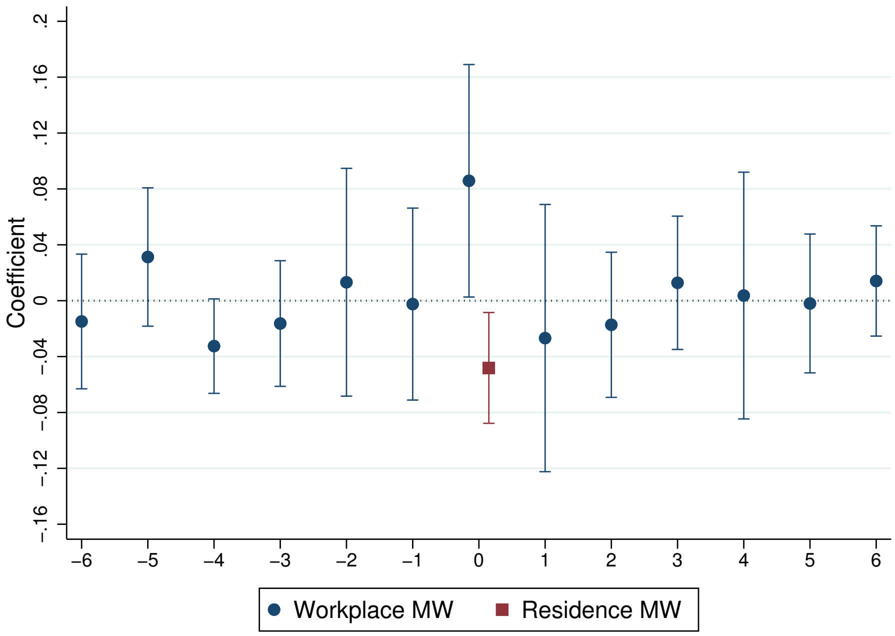


(b) Control for year-month fixed effects



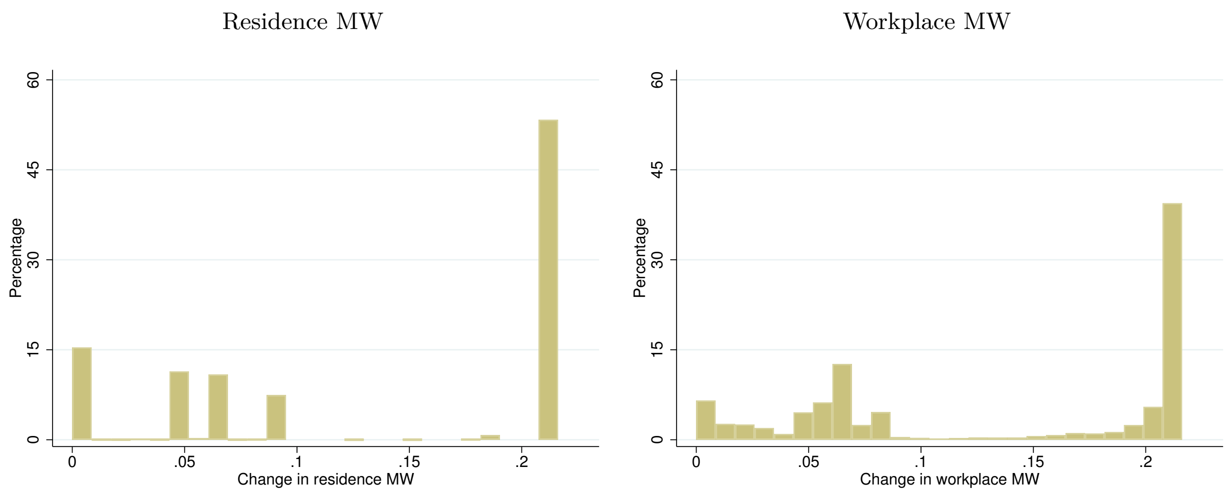
Notes: Data are from the baseline estimation sample described in Section 1.3.3.4. The figures show coefficients from regressions of the change in log of Zillow rental index on leads and lags of the change in the workplace MW and the change in the residence MW, using the 4th lead of the MW-based measures. The top panel includes CBSA by year-month fixed effects, whereas the bottom panel includes year-month fixed effects. Both regressions include economic controls that vary at the county by month and county by quarter levels, which include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors “Information,” “Financial activities,” and “Professional and business services.” 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

Figure 1.M: Estimates of the effect of the minimum wage on rents, stacked sample including leads and lags



Notes: Data are from Zillow, the MW panel described in Section 1.3.1, LODES origin-destination statistics, and the QCEW. The figure mimics estimates in Figure 1.4 using a "stacked" sample. We construct the sample as explained in Online Appendix Table 1.C. 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

Figure 1.N: Distribution of changes in minimum wage measures under a counterfactual federal minimum wage of $9, urban ZIP codes

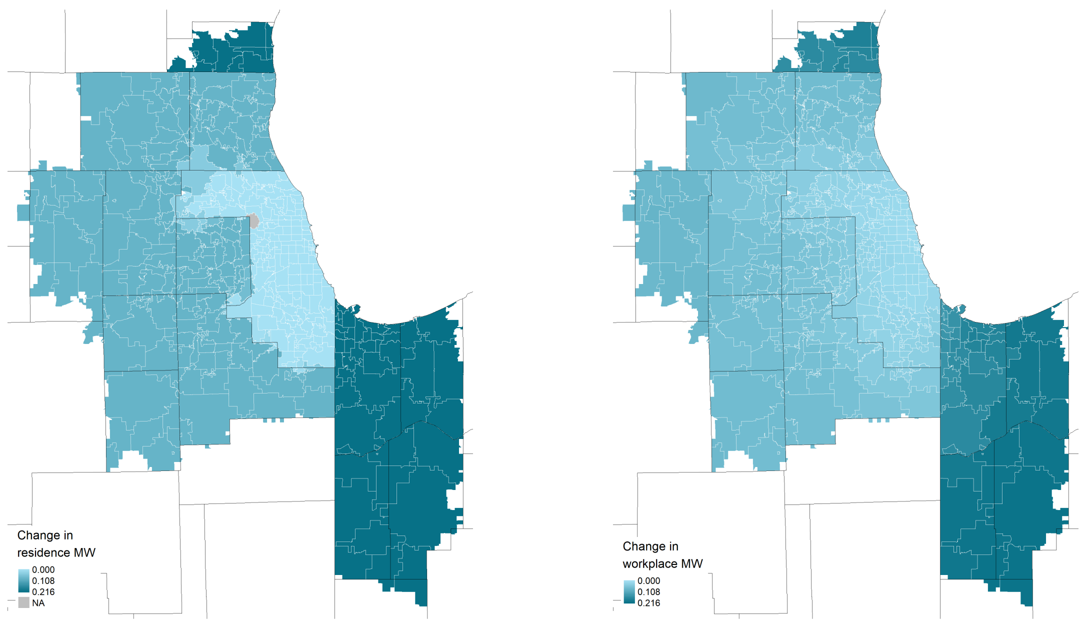


Notes: Data are from LODES and the MW panel described in Section 1.3.1. The figures show the distribution of changes in the residence and workplace MW measures generated by a counterfactual increase to $9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. The unit of observation is the urban ZIP code, where we define a ZIP code as urban if it belongs to a CBSA with at least 80% of its population classified as urban by the 2010 Census. We exclude ZIP codes located in CBSAs where the estimated increase in income was higher than 0.1.

Figure 1.O: Changes in the minimum wage measures under counterfactual minimum wage policies, Chicago-Naperville-Elgin CBSA

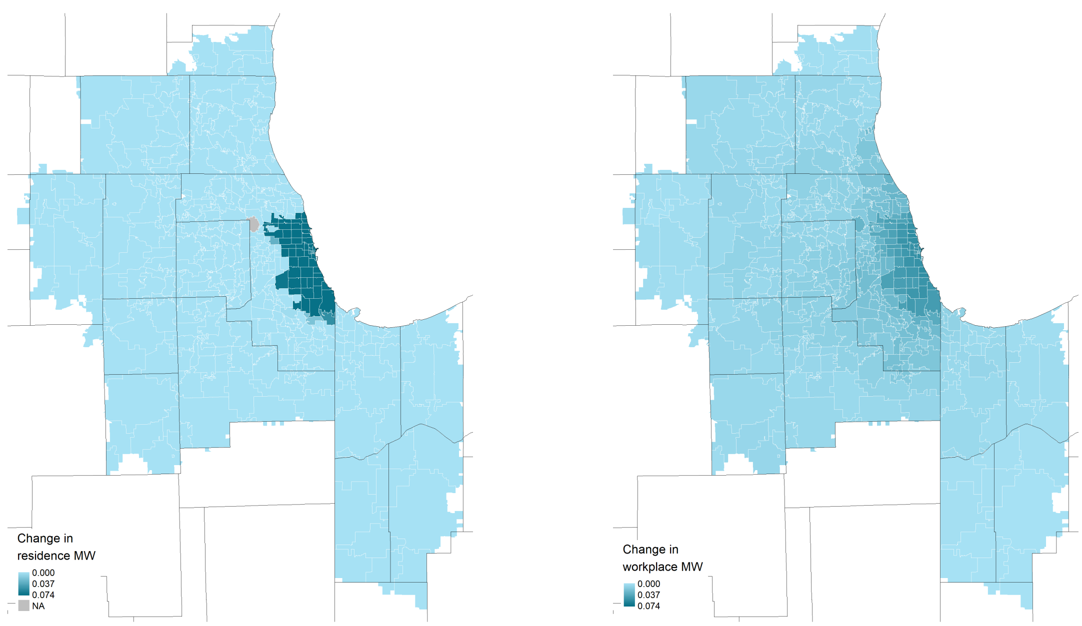
Panel A: Increase in federal MW from $7.25 to $9

Changes in residence MW | Changes in workplace MW



Panel B: Increase in Chicago MW from $13 to $14

Changes in residence MW | Changes in workplace MW

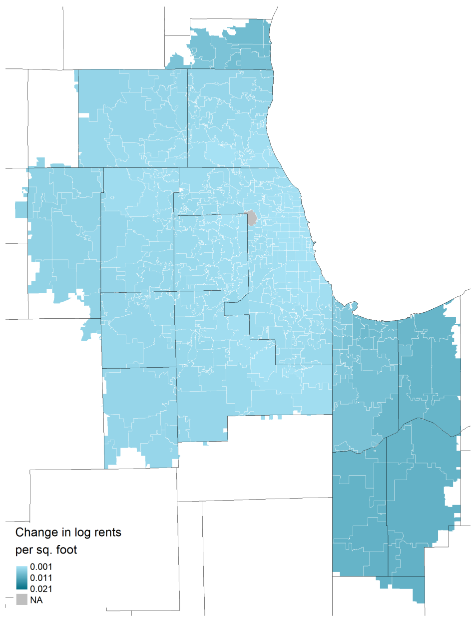


Notes: Data are from the MW panel described in Section 1.3.1 and from LODES. The figures map changes in the residence and workplace MW measures by counterfactual MW policies in the Chicago-Naperville-Elgin CBSA. Panel A shows a policy where the federal MW increases from $7.25 to $9 in January 2020, holding constant other MW policies at their December 2019 levels. Panel B shows a policy where the city of Chicago increases its MW from $13 to $14 in January 2020, holding constant other MW policies at their December 2019 levels as well.

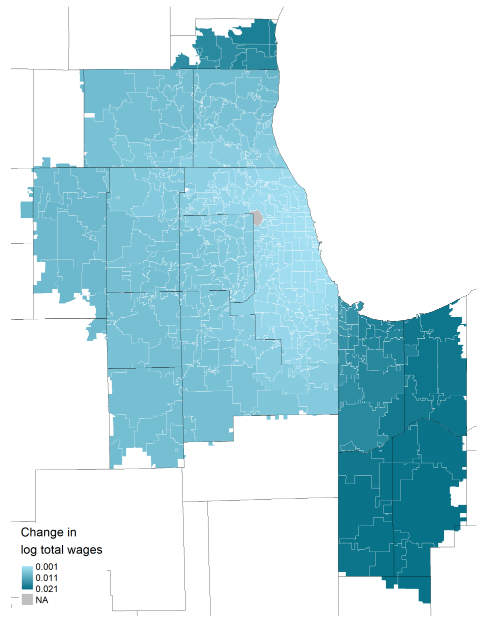
Figure 1.P: Changes in log rents and log total wages under counterfactual minimum wage policies, Chicago-Naperville-Elgin CBSA

Panel A: Increase in federal MW from \$7.25 to \$9

Changes in log rents

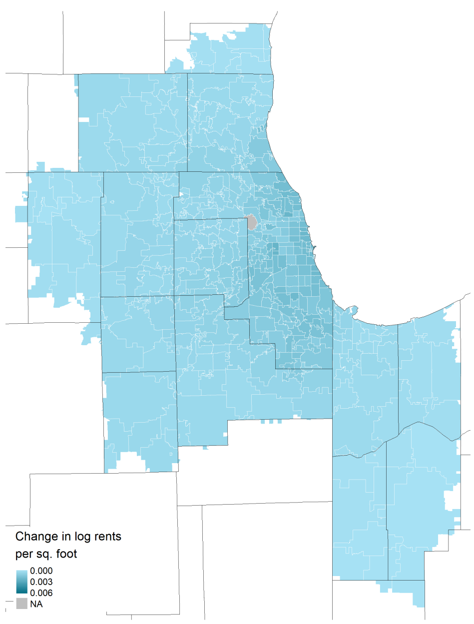


Changes in log total wages

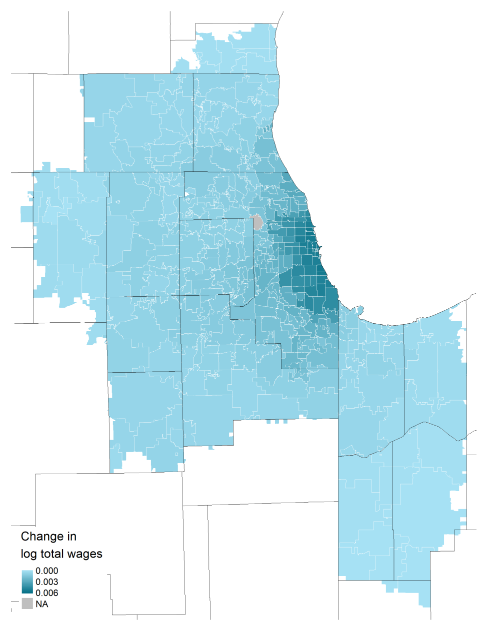


Panel B: Increase in Chicago MW from \$13 to \$14

Changes in log rents



Changes in log total wages



Notes: Data are from the MW panel described in section 1.3.1 and from LODES. The figures map the estimated changes in log total rents per square foot and log total wage income under different counterfactual MW policies in the Chicago-Naperville-Elgin CBSA. Panel A is based on a counterfactual increase to \$9 in the federal MW in January 2020, and Panel B on a counterfactual increase from \$13 to \$14 in the Chicago City MW, both holding constant other MW policies. The color scale has been standardized within each panel. To estimate the changes we follow the procedure described in Section 1.6 assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$.

CHAPTER 2

Labor Market Returns and the Evolution of Cognitive Skills: Theory and Evidence

2.1 Introduction

A large and important literature in cognitive science documents substantial gains in intelligence (IQ) scores across successive cohorts in developed countries, sometimes called the "Flynn effect" (Flynn, 2007, 2012; Flynn & Shayer, 2018; Pietschnig & Voracek, 2015; Schaie, Willis, & Pennak, 2005; Trahan et al., 2014, see, for example,).[1] These gains are especially pronounced for *fluid intelligence*, a notion of general reasoning ability often measured with abstract reasoning tasks (Pietschnig & Voracek, 2015). There are less pronounced gains, or even declines, in *crystallized intelligence*, a notion of domain knowledge often measured with knowledge assessments such as vocabulary tests (Pietschnig & Voracek, 2015; Schaie, Willis, & Pennak, 2005).[2] Understanding the causes of these trends is important in part because of evidence that a population's level of cognitive skills influences its economic productivity, economic growth, and distribution of income (e.g., Bishop Bishop 1989; Hanushek and Woessmann 2008, Section 5).[3]

There is no consensus on the precise causes of cohort trends in cognitive performance, which some consider to be an important puzzle.[4] Research in cognitive science emphasizes factors, such as improvements in health and nutrition, that expand the supply of skill (e.g., Pietschnig & Voracek, 2015; Rindermann, Becker, & Coyle, 2017). But the incentive to invest in particular dimensions of skill may also evolve over time in response to the demands of the economy.

In this paper, we study the role of labor market returns in determining cohort trends in skill levels and skill composition. We focus on Sweden, where an administrative data join between standardized test scores

[1] Rindermann, Becker, and Coyle (2017) write, "Among the most discussed topics in intelligence research is the rise of average IQ test results across generations in the 20th century" (p. 242).

[2] Cattell (1943) writes, "Fluid ability has the character of a purely general ability to discriminate and perceive relations between any fundaments, new or old... Crystallized ability consists of discriminatory habits long established in a particular field." (p. 178).

[3] There is also evidence that a population's level of cognitive skills is related to its levels of patience and risk aversion (Falk et al., 2018; Potrafke, 2019).

[4] Deary (2020) writes, "If there were a prize in the field of human intelligence research, it might be for the person who can explain the 'Flynn effect'..." (also quoted in Wai & Putallaz, 2011).

(collected for military conscription typically at age 18 or 19) and earnings (collected by the tax agency over the lifecycle) allows us to measure the level of and return to skill in a consistent way across cohorts for the near-population of men.

We develop a model of an economy whose aggregate output is determined by the aggregate skills of workers. Skills, which can be multidimensional, are determined both by an exogenous endowment (e.g., health) and an investment decision made early in life (by parents, children, and schools). The investment decision is in turn influenced by the lifetime labor market returns to different skills. We identify the relative returns to different skills by assuming that unobserved determinants of an individual's earnings are correlated with the individual's skill endowment only through its market value. Under this assumption, the relative returns to different skills can be recovered from a Mincerian regression of the log of earnings on skills in a cross-section of individuals.

We parameterize the model so that a single unknown parameter governs the degree to which individuals can substitute investment across skill dimensions. We identify this parameter by assuming that long-run average shocks to the technology for producing skills are proportional across fluid and crystallized intelligence.

We take the model to the data. Across the birth cohorts 1962–1975, we find that performance on a logical reasoning task—our proxy for fluid intelligence—improved by 4.4 percentile points, measured in terms of the distribution in the 1967 cohort. The estimated lifetime earnings premium to an additional percentile point of logical reasoning performance fell by 0.08 log points, from a base of 0.48 log points. Turning to performance on a vocabulary knowledge test—our proxy for crystallized intelligence—we find that performance declined by 2.9 percentile points. The estimated lifetime premium to an additional percentile point of vocabulary knowledge fell by 0.07 log points, but from a much lower base of 0.16 log points.

Because logical reasoning performance rose while its market return fell, a model in which logical reasoning is the only skill dimension would imply that there must have been an increase in the supply of skill, consistent with the hypothesis of a growth in the endowment of fluid intelligence of the sort emphasized in the cognitive science literature. A richer picture emerges when incorporating the second skill dimension. Vocabulary knowledge performance fell along with its market return, suggesting a decline in the demand for this skill dimension. Moreover, the premium to vocabulary knowledge relative to logical reasoning fell by 38 log points. Seen through the lens of our model, the declining relative premium to crystallized intelligence drove a reallocation of effort towards developing abstract reasoning and away from acquiring knowledge.

We use the model to decompose the observed trends in skills into a portion driven by changing labor market returns and a portion driven by other factors. According to the estimated model, if the market returns to different skills had remained constant at their 1962 level, logical reasoning and vocabulary knowledge performance would have increased by 2.8 and 3.0 percentile points, respectively. The estimated model thus implies that trends in labor market returns explain 37 percent of the growth in logical reasoning performance (roughly, $100 \times \frac{4.4-2.8}{4.4}$) and more than fully explain the decline in vocabulary knowledge.

We extend our baseline analysis in a few directions. First, we use a nationally representative survey linked to earnings records to expand our analysis to a broader set of birth cohorts, from 1948 to 1977, and to skills measured at a younger age, around age 13. We find that the relative level of and return to logical reasoning performance rose across these cohorts, though our estimates are less precise than those from the (much larger) enlistment sample. Second, we adjust the estimated trends in skill levels and skill returns to account for the role of covariates such as height and secondary school completion. Although adjusting for covariates is conceptually delicate, as some covariates may themselves respond to labor market returns, we find broadly similar conclusions across a variety of sensitivity analyses. Third, we extend our model to incorporate non-cognitive skills. We estimate a smaller, but still important, role for changes in labor market returns in explaining the evolution of cognitive skills, and we highlight limitations of the analysis that arise because the measure of non-cognitive skills in our data is not directly comparable across cohorts.

We also explore whether the main actors in skill investment—parents and schools—place increasing emphasis on reasoning relative to knowledge. In an original survey, we find that parents of more recent cohorts tend to regard reasoning ability as more important for their children than knowledge of facts. In a review of pedagogical scholarship, and an original quantitative text analysis, we find evidence of a trend towards increasing emphasis on reasoning relative to knowledge in primary school curricula in Sweden. Turning to the demand for skills, we show evidence of relative growth in occupations that place more emphasis on reasoning as opposed to knowledge. We view this evidence as consistent with the mechanism underlying our estimated model.

Our analysis has some important limitations. A first limitation is that we treat the skill demand portion of the model fairly abstractly and do not offer a precise account of why some skills have become relatively more valuable in the labor market over time, though we show some suggestive evidence based on occupational characteristics. A second limitation is that our conclusions require assumptions on unmeasured determinants of earnings and skills. We specify and discuss these assumptions, their plausibility, and their importance in more detail in the body of the paper, where we also discuss evidence on sensitivity to departures from key assumptions. A third limitation is that we focus on the labor market returns to skills and do not measure their nonmarket returns, though we show that our conclusions are preserved if market and non-market returns to skill move in proportion across cohorts. A final limitation is that, due to the nature of the military enlistment data that we use, our main analyses are limited to men only, though in an Appendix we show results for women in the survey sample.

The main contribution of this paper is to develop and apply an economic model to quantify the role of labor market returns in determining cohort trends in multidimensional cognitive skills. We are not aware of prior work that does this. A large literature in economics studies the determinants and market value of (possibly multidimensional) cognitive and non-cognitive skills (see, for example, the review by Sanders and Taber 2012 and recent papers by Roys and Taber 2020 and Agostinelli and Wiswall (2020)). Our analysis of the market for skills is closely related to the work of Katz and Murphy (1992) and the large literature that follows (see, e.g., Deming 2017 and the review by Acemoglu and Autor 2011), but differs in focusing on explaining trends across cohorts (rather than time periods) and in offering an explicit quantitative model of the supply of (rather than demand for) skills. As we do, Heckman, Lochner, and Taber (1998) develop a general-equilibrium model of the supply and demand for skill. Their model is richer than ours in its treatment of labor demand but does not incorporate multiple dimensions of skill.[5]

A large literature in cognitive science (reviewed, for example, in Pietschnig & Voracek, 2015) studies causes of trends in various measures of ability or intelligence. Although some work in this literature considers the possibility that social demands affect the development of skills, we are not aware of work in this literature that quantifies trends in the economic returns to different types of skills, or that uses an estimated model to link trends in skills to trends in their returns.[6] We are also not aware of prior work that quantifies long-term trends in parents' and schools' emphasis on reasoning vs. knowledge.[7]

An additional contribution of this paper is to document trends in the relative labor market returns to different dimensions of cognitive skill. Much prior work in economics and other fields studies trends in the level of and returns to skills,[8] including some work using linked administrative data from elsewhere in

[5]Our model of the supply of skill, which focuses on cohort-level trends, is more stylized than in work that focuses on the skill formation process itself (see, e.g., Cunha, Heckman, & Schennach, 2010; Cunha et al., 2006; Doepke, Sorrenti, & Zilibotti, 2019). In particular, unlike much of the work reviewed in, e.g., Heckman and Mosso (2014), we treat the skill investment decision as static and do not model the dynamics of skill formation during childhood.

[6]Dickens and Flynn (2001) specify and simulate a quantitative model in which genetic endowments and environmental factors interact to produce measured intelligence. They discuss the role of occupational demands in driving cohort differences in skills, but do not incorporate labor market returns into their quantitative model, and do not estimate the model's parameters. Flynn (2018, p. 79) notes that "When society asks us to increase our use of any skill over time, the brain responds," and cites research by Maguire, Woollett, and Spiers (2006) on the effect of occupational demands on brain structure in the context of London taxi and bus drivers.

[7]Okagaki and Sternberg (1993) study group differences in parents' conceptions of intelligence. Bietenbeck (2014) studies the effects on reasoning and knowledge skills of traditional and modern teaching practices. Cunha, Elo, and Culhane (forthcoming), among others, study the relationship between parents' beliefs about the technology of skill formation and parents' investments in children's skills.

[8]For example, Castex and Dechter (2014) use survey data to document falling returns to cognitive skills as measured by Armed Forces Qualification Test scores in the US between the 1980s and 2000s.

Europe,[9] as well as some work using the same data from Sweden that we use.[10] Rönnlund et al. (2013) report trends in test scores in Sweden from 1970–1993. Lindqvist and Vestman (2011) study the labor market return to cognitive and non-cognitive skills in Sweden. Especially related, Edin et al. (2022) estimate trends in the returns to cognitive and non-cognitive skills in Sweden. None of these papers documents trends in the relative lifetime labor market returns to different dimensions of cognitive skill, or quantifies the role of labor market returns in driving cohort trends in skill levels in a model with multidimensional skills.[11]

The remainder of the paper is organized as follows. Section 2.2 presents our model and approach to identification. Section 2.3 describes the data we use. Section 2.4 presents our main findings. Section 2.5 discusses additional evidence related to the mechanisms in the model. Section 2.6 extends our analysis to incorporate non-cognitive skills. Section 2.7 concludes.

2.2 Model

2.2.1 Production and Earnings

There is a finite population of workers $i \in \mathcal{N}$, each of which is associated with a cohort $c(i) \in \{\underline{c}, ..., \overline{c}\}$. Each worker is characterized by a skill level $\mathbf{x}_i \in \mathbb{R}^J_{\geq 0}$ for $J \geq 2$.

In each time period t, each worker i has an experience level $a(i,t) = t - c(i)$ and supplies efficiency units $z_{it} \in \mathbb{R}_{\geq 0}$, where $z_{it} > 0$ if $a(i,t) \in \{1, ..., A\}$ and $z_{it} = 0$ otherwise. Thus, members of cohort c enter the labor force in period $c + 1$ and exit the labor force after period $c + A$, and we identify the cohort c with the period immediately before workers in the cohort enter the labor force.

Let $\mathbf{X}_t$ be the $J \times A$ matrix whose a^{th} column is given by the sum of $z_{it}\mathbf{x}_i$ over all workers i with experience level $a(i,t) = a$. This matrix collects the total supply of skill in period t for each dimension j and experience level a. Let $\mathbf{X}_t^{-i}$ be the analogue of $\mathbf{X}_t$ excluding worker i.[12]

[9] For example, Jokela et al. (2017) document cohort trends in personality traits using scores from military conscripts in Finland, and argue based on estimated labor market returns that the economic significance of cohort trends in personality traits is similar to that of cohort trends in cognitive abilities. Markussen and Røed (2020, Section 4.2) document declining labor market returns to men's cognitive skills using test scores from enrollment in military service in Norway.

[10] These data have also been used to study, among other topics, the effect of schooling on measured skills (Carlsson et al., 2015) and the effect of officer training on occupational outcomes later in life (Grönqvist & Lindqvist, 2016).

[11] Jokela et al. (2017) document trends in the within-cohort rank correlation between three different dimensions of cognitive skill and earnings at age 30 (Figure 2, panel B) or ages 30-34 (Figure S1, panel B), but do not report trends in lifetime labor market returns from a model of earnings that accounts for multiple skill dimensions simultaneously. Lindquist (2005) models trends in the demand for skill in Sweden arising from capital-skill complementarity.

[12] That is, the a^{th} column of $\mathbf{X}_t$ is

$$\sum_{\{l \in \mathcal{N}:\ a(l,t)=a\}} z_{lt}\mathbf{x}_l$$

Total output Y_t at time t is given by

$$Y_t = F_t\left(\mathbf{X}_t\right)$$

where $F_t\left(\cdot\right)$ is a scalar-valued differentiable function that may vary over time, for example due to changes in production technology.

In each period t, a worker i earns his marginal product w_{it}, which is given by

$$w_{it} = F_t\left(\mathbf{X}_t\right) - F_t\left(\mathbf{X}_t^{-i}\right) \approx z_{it}\nabla F^{'}_{t,a(i,t)}\mathbf{x}_i$$

where $\nabla F_{t,a}$ is the gradient of $F_t\left(\mathbf{X}_t\right)$ at $\mathbf{X}_t$ with respect to the a^{th} column of $\mathbf{X}_t$. We will assume that $\nabla F^{'}_{t,a(i,t)}\mathbf{x}_i > 0$ for all workers i in all periods t of working life. Motivated by a large-population setting, we will treat $\mathbf{X}_t$ as fixed from the perspective of any individual worker i.

Pick a period t of worker i's working life, so that $z_{it} > 0$, and rewrite the earnings equation as

$$\ln\left(w_{it}\right) \approx \ln\left(z_{it}\right) + \ln\left(\nabla F^{'}_{t,a(i,t)}\mathbf{x}_i\right).$$

Now take a first-order approximation around the mean skill level $\mathbf{x}_{t,a(i,t)}$ of individuals who share worker i's experience level at time t to get

$$\ln\left(w_{it}\right) \approx \ln\left(z_{it}\right) + \ln\left(\nabla F^{'}_{t,a(i,t)}\mathbf{x}_{t,a(i,t)}\right) + \frac{\nabla F^{'}_{t,a(i,t)}}{\nabla F^{'}_{t,a(i,t)}\mathbf{x}_{t,a(i,t)}}\left(\mathbf{x}_i - \mathbf{x}_{t,a(i,t)}\right),$$

where we will again treat $\mathbf{x}_{t,a(i,t)}$ as fixed from the perspective of any individual worker i. We can write the preceding as

$$\ln\left(w_{it}\right) \approx B_{t,a(i,t)} + \mathbf{p}^{'}_{t,a(i,t)}\mathbf{x}_i + \ln\left(z_{it}\right) \tag{2.1}$$

where $B_{t,a}$ is a scalar, $\mathbf{p}_{t,a}$ is a vector of skill premia, and both of these are specific to a time period and experience level.[13]

We will proceed taking equation (2.1) to be exact. Although we have derived (2.1) from a particular model of the labor market, any model in which earnings take the form in (2.1) will be equivalent for the

and that of $\mathbf{X}_t^{-i}$ is

$$\sum_{\{l \in \mathcal{N}\setminus\{i\}:\ a(l,t)=a\}} z_{lt}\mathbf{x}_l.$$

[13]Specifically,

$$B_{t,a} = \ln\left(\nabla F^{'}_{t,a}\mathbf{x}_{t,a}\right) - 1, \qquad \mathbf{p}_{t,a} = \frac{\nabla F_{t,a}}{\nabla F^{'}_{t,a}\mathbf{x}_{t,a}}.$$

purposes of our subsequent analysis. Moreover, although for concreteness we refer to z_{it} as efficiency units, (2.1) makes clear that z_{it} captures any individual-and-period-specific determinants of earnings that are not included in $\mathbf{x}_i$.

2.2.2 Skill Investment

At the beginning of life, each worker i chooses his skills $\mathbf{x}_i$ subject to the constraints

$$\begin{aligned} \mathbf{x}_i &\geq \mu_i \\ S_{c(i)}\left(\mathbf{x}_i - \mu_i\right) &\leq \overline{S}_{c(i)} \end{aligned} \tag{2.2}$$

where $\mu_i \in \mathbb{R}^J$ is an individual skill endowment, $\overline{S}_c \in \mathbb{R}_{>0}$ is a cohort-specific skill budget, and $S_c(\cdot)$ is a cohort-specific transformation function.

We can think of $\mathbf{x}_i - \mu_i \in \mathbb{R}^J_{\geq 0}$ as the skill investment of individual i, i.e., the increment in skills over and above the individual's endowment μ_i. The endowment μ_i represents cross-sectional differences within a cohort, say in ability or access to schooling. The budget $\overline{S}_c$ can be seen as representing the total time and effort available for skill investment. The transformation function $S_c(\cdot)$ may be thought of as governing the ease of skill investment and of substituting investment across skill dimensions. The budget $\overline{S}_c$ and the function $S_c(\cdot)$ may differ across cohorts because of trends in the technology of skill formation, say because of improvements in health or nutrition. Although for simplicity we refer to the decision-maker as the worker, we may alternatively think of the skill investment decision as being made by the worker's parents, or by a collective decision-making process involving the worker, his parents, and the schooling system.[14] Because we take the timing of entry into the labor market as given, we do not account for any foregone earnings due to time spent acquiring skills.

Each worker consumes his earnings in each period and has time-separable preferences with a felicity function given by the log of consumption. Each worker discounts future felicity by a discount factor $\delta \in (0, 1]$. At the time of choosing the skill investment, worker i has full knowledge of the path of skill premia over his lifecycle, $\left\{\mathbf{p}_{c(i)+a,a}\right\}_{a=1}^{A}$. We further assume that worker i's skill investment does not influence the path of z_{it}.

[14] For example, we may think of the skill budget $\overline{S}_c$ as reflecting the sum of the effective time and effort available from the worker, his parents, and his teachers.

It follows that the worker's problem is equivalent to maximizing $\mathbf{P}^{'}_{c(i)}\mathbf{x}_i$ subject to (2.2), where

$$\mathbf{P}_{c(i)} = \frac{\sum_{a=1}^{A} \delta^a \mathbf{p}_{c(i)+a,a}}{\sum_{a=1}^{A} \delta^a} \tag{2.3}$$

is the net present value of the skill premia $\mathbf{p}_{c(i)+a,a}$ at different experience levels a, normalized by the constant $\sum_{a=1}^{A} \delta^a$ to have a convenient interpretation as a weighted average. We refer to $\mathbf{P}_c$ as the *lifetime skill premia* faced by cohort c. Although we have assumed for concreteness that workers have full knowledge of the path of skill premia, the linearity of equation (2.1) in $\mathbf{x}_i$ means that we can alternatively allow for uncertainty in skill premia by replacing $\mathbf{p}_{c(i)+a,a}$ in (2.3) with its expectation.[15] Likewise, although we have assumed that skills $\mathbf{x}_i$ are fixed throughout working life, it is possible to accommodate a linear, deterministic evolution of skills over the lifetime under a suitable reinterpretation of $\mathbf{p}_{c(i)+a,a}$ in (2.3).[16]

The worker's problem is also equivalent to maximizing $\mathbf{P}^{'}_{c(i)}\tilde{\mathbf{x}}_i$ subject to $\tilde{\mathbf{x}}_i \geq 0$ and $S_{c(i)}\left(\tilde{\mathbf{x}}_i\right) \leq \overline{S}_{c(i)}$, where $\tilde{\mathbf{x}}_i = \mathbf{x}_i - \mu_i$. The solutions to this problem depend only on the cohort $c\left(i\right)$ of the worker and not on the worker's identity. In this sense, within-cohort variation in skill levels arises only due to variation in the individual skill endowment μ_i. We assume that μ_i has mean zero within each cohort. This assumption is without loss of generality since we can always define $\mathbf{x}_i$ and μ_i relative to a cohort-specific mean endowment.[17]

2.2.3 Parameterization and Identification

We will assume that the transformation function $S_c\left(\cdot\right)$ takes the constant elasticity form

$$S_c\left(\tilde{\mathbf{x}}\right) = \left(\sum_{j=1}^{J} K_{cj}^{\rho-1} \tilde{x}_j^{\rho}\right)^{\frac{1}{\rho}} \tag{2.4}$$

[15] That is, taking $\mathrm{E}_c\left[\cdot\right]$ to be an expectation with respect to the information set of workers in cohort c at the time that skill investments are made, we can take the worker's expected discounted utility to be

$$\frac{\sum_{a=1}^{A} \delta^a \,\mathrm{E}_{c(i)}\left[\mathbf{p}^{'}_{c(i)+a,a}\right]}{\sum_{a=1}^{A} \delta^a}\mathbf{x}_i.$$

[16] Specifically, suppose that each worker enters working life with chosen skills $\mathbf{x}_{i,0} = \mathbf{x}_i$, which then evolve with experience according to $\mathbf{x}_{i,a} - \mathbf{x}_{i,a-1} = \Lambda_{c(i),a}\mathbf{x}_{i,a-1}$ for $a \in \{1, ..., A\}$, with $\Lambda_{c,a} > -I_J$ elementwise for all c, a. Then we can take $\mathbf{p}^{'}_{c(i)+a,a} = \tilde{\mathbf{p}}^{'}_{c(i)+a,a} \prod_{a'=1}^{a} \left(\Lambda_{c(i),a'} + I_J\right)$ where $\tilde{\mathbf{p}}_{c(i)+a,a}$ are the (contemporaneous) premia to the worker's skills $\mathbf{x}_{i,a}$ at experience level a.

[17] To see this, start with an endowment $\mathring{\mu}_i$ with mean $\mathring{\mu}_c = \frac{\sum_{\{i:c(i)=c\}} \mathring{\mu}_i}{|\{i:c(i)=c\}|}$ in cohort c, where $\mathring{\mu}_c$ need not be zero. The problem of maximizing $\mathbf{P}^{'}_{c(i)}\mathring{\mathbf{x}}_i$ subject to $\mathring{\mathbf{x}}_i \geq \mathring{\mu}_i$ and $S_{c(i)}\left(\mathring{\mathbf{x}}_i - \mathring{\mu}_i\right) \leq \overline{S}_{c(i)}$ is equivalent to the problem of maximizing $\mathbf{P}^{'}_{c(i)}\mathbf{x}_i$ subject to (2.2) where $\mathbf{x}_i = \mathring{\mathbf{x}}_i - \mathring{\mu}_{c(i)}$ and $\mu_i = \mathring{\mu}_i - \mathring{\mu}_{c(i)}$. Here μ_i has mean zero within each cohort by construction.

where $\mathbf{K}_c \in \mathbb{R}^J_{>0}$ is a vector-valued parameter that we may think of as describing the cost of increasing skill along each of the J dimensions for cohort c, and $\rho > 1$ is a scalar parameter that determines the substitutability of effort across different skill dimensions.

Worker i's problem has a unique solution, with $\tilde{\mathbf{x}}_i = \tilde{\mathbf{x}}_{i'}$ if $c(i) = c(i')$. Therefore write $\tilde{\mathbf{x}}_c = \tilde{\mathbf{x}}_c(\mathbf{P}_c)$ as the optimal $\tilde{\mathbf{x}}_i$ for all workers i in cohort c. Here $\tilde{\mathbf{x}}_c(\cdot)$ is a *skill supply function* that returns the optimal skill investment for members of cohort c given the lifetime skill premia $\mathbf{P}_c$.[18] We assume that $\mathbf{P}_c > 0$ for all c.

Imagine an econometrician who has data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$ and wishes to learn the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$. Focus on the first two dimensions, where we may think of fluid intelligence as dimension $j = 1$ and crystallized intelligence as dimension $j = 2$. Under the model, the relative supply of fluid intelligence obeys

$$\ln\left(\frac{\tilde{x}_{c1}}{\tilde{x}_{c2}}\right) = \frac{1}{\rho - 1}\ln\left(\frac{P_{c1}}{P_{c2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right). \tag{2.5}$$

A standard difficulty in learning the elasticity of substitution $\frac{1}{\rho-1}$ is that the unobserved costs $\mathbf{K}_c$ may affect both skill investments (via the workers' incentives) and skill premia (via the labor market). We assume that, on average, there is no trend in the relative costs of the two skill dimensions.

Assumption 1. *(Zero average relative supply shock.) We assume that*

$$\frac{1}{\overline{c} - \underline{c}}\sum_{c=\underline{c}}^{\overline{c}-1}\left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right)\right] = 0.$$

Under Assumption 1, long-run improvements in the technology for producing skills are not systematically biased towards either fluid or crystallized intelligence.

Assumption 1 is sufficient for the identification of $\tilde{\mathbf{x}}_c(\cdot)$ under a regularity condition on $\mathbf{P}_c$.

Proposition 5. *Under Assumption 1, if $\frac{P_{\overline{c}1}}{P_{\overline{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$, then the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$ for each cohort c is identified from data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$.*

All proofs are in Appendix 2.A. The proof of Proposition 5 is constructive. Under Assumption 1, an explicit expression for ρ can be derived using equation (2.5). We can then learn the costs $\mathbf{K}_c$ and budget $\overline{S}_c$ up to

[18] Specifically, for each skill $j \in \{1, \ldots, J\}$, we have

$$\tilde{x}_{cj}(\mathbf{P}_c) = \frac{P_{cj}^{\frac{1}{\rho-1}} K_{cj}^{-1}}{\left(\sum_{j'=1}^{J} P_{cj'}^{\frac{\rho}{\rho-1}} K_{cj'}^{-1}\right)^{\frac{1}{\rho}}} \overline{S}_c.$$

suitable normalizations. The required regularity condition on $\mathbf{P}_c$ can in principle be checked in the data. Appendix 2.B presents conditions for the identification of $\tilde{\mathbf{x}}_c(\cdot)$ in the presence of a social multiplier in skill investment in the spirit of Dickens and Flynn (2001, equation 2”).

Proposition 5 requires that the econometrician knows $\mathbf{P}_c$. This requirement can be relaxed to require only that $\mathbf{P}_c$ is known up to scale.

Corollary 1. *Under the conditions of Proposition 5, the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$ for each cohort c is identified from data $\{(\alpha \mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$, where the scalar $\alpha > 0$ may be unknown.*

Corollary 1 allows the econometrician to understate or overstate the lifetime skill premia, provided the error is proportional across dimensions j and the constant of proportionality does not differ across cohorts. An immediate implication is that if there are non-market returns to skill that evolve in proportion to market returns—say, because skills earn a premium on the marriage market only to the extent they improve a person's earning potential—then measurement of market returns is sufficient for identification of the skill supply function.

What remains is to establish conditions for the identification of $\tilde{\mathbf{x}}_c$ and $\mathbf{P}_c$. Recall that we assume that μ_i has mean zero within each cohort, implying that $\tilde{\mathbf{x}}_c = \overline{\mathbf{x}}_c$ for $\overline{\mathbf{x}}_c$ the mean skill of individuals in cohort c. Identification of $\tilde{\mathbf{x}}_c$ from the distribution of $\mathbf{x}_i$ is therefore trivial.

Recall also that $\mathbf{P}_c$ is the net present value of cohort-and-period-specific skill premia $\mathbf{p}_{t,a} = \mathbf{p}_{t,t-c}$. We identify $\mathbf{p}_{t,t-c}$, up to scale, from a Mincerian regression of the log of earnings on measured skills. To do this, we restrict the relationship between the unobserved determinants of earnings z_{it} and skill endowments μ_i, allowing that the econometrician may also observe a vector of covariates $\mathbf{d}_{it}$.

Assumption 2. *The values of z_{it} in each period t obey*

$$\mathrm{E}\left(\ln\left(z_{it}\right)|\mu_i = \mu,\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right) = \zeta_{t,t-c} + \tilde{\alpha}\mathbf{p}_{t,t-c}^{'}\mu + \mathbf{d}^{'}\beta_{t,t-c}$$

where $\zeta_{t,t-c}$ and $\beta_{t,t-c}$ are unknown parameters, and the scalar $\tilde{\alpha} \geq 0$ may also be unknown.

Assumption 2 allows that the unobserved determinants of earnings are linearly related both to the observed covariates $\mathbf{d}_{it}$ and to the market value of the skill endowment $\mathbf{p}_{t,t-c}^{'}\mu_i$. Such a relationship can arise if the market supplies inputs complementary to the worker's endowment.[19]

[19]Suppose, for example, that the efficiency units z_{it} of worker i at time t are given by $z_{it} = \tilde{z}_{it}\underline{z}_{t,a(i,t)}$ where $\tilde{z}_{it} \geq 1$ is the amount of some input and $\underline{z}_{t,a} = (\nabla F_{t,a}^{'}\mathbf{x}_{t,a})^{-1}$ is a scale factor that ensures that mean earnings in each period and experience level are unity if the minimum input is always supplied. Say that the input for worker i at time t is supplied competitively, with

Assumption 2 is sufficient to identify the cohort-and-period-specific skill premia $\mathbf{p}_{t,t-c}$, and hence the lifetime skill premia $\mathbf{P}_c$, up to scale, from the conditional expectation function of the log of earnings.

Proposition 6. *Under Assumption 2, for some scalar $\alpha > 0$, a multiple $\alpha \mathbf{P}_c$ of the lifetime skill premia for each cohort c is identified from the conditional expectation function of the log of earnings,*

$$\mathrm{E}\left(\ln\left(w_{it}\right) | \mathbf{x}_i = \mathbf{x},\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right),$$

for each time period $t \in \{c+1, ..., c+A\}$.

Importantly, Proposition 6 does not require that all determinants of earnings are observed, or that unobserved determinants of earnings are independent of skills. Instead, Proposition 6 requires that unobserved determinants of earnings are related to the skill endowment only through its market value, with a coefficient that does not vary across cohorts or periods. Appendix 2.C presents alternative conditions for identification of $\mathbf{P}_c$ up to scale when skills are measured with error.

Although we identify $\mathbf{P}_c$ only up to an unknown multiple $\alpha > 0$, going forward we will for simplicity write as if $\alpha = 1$. Moreover, although for concreteness Assumption 2 requires that $\tilde{\alpha} \geq 0$, and hence that a regression of the log of earnings on skills will tend to overstate skill premia, the proofs of Corollary 1 and Proposition 6 make clear that $\tilde{\alpha} \neq -1$ is sufficient.

2.2.4 Discussion

Assumption 1 is violated if long-run improvements in skill production technology favor one skill dimension over the other. Testing this assumption is difficult because it imposes a restriction only on those changes in relative skill levels that would have occurred in the absence of changes in relative skill premia.[20]

However, it is possible to obtain some clues about the plausibility of this assumption from prior research in cognitive science and economics. Improvements in schooling are one potentially important cause of changes

marginal product $\underline{z}_{t,a(i,t)} \nabla F'_{t,a(i,t)} \mu_i$ given by the effect of an increase in $\tilde{z}_{it}$ on total output from the worker's skill endowment, and marginal cost $\tilde{\alpha}^{-1}\left(\ln\left(\tilde{z}_{it}\right) - \eta_{it}\right)$ for η_{it} a shock. From equating marginal product and marginal cost, it follows that

$$\ln\left(\tilde{z}_{it}\right) = \tilde{\alpha} \mathbf{p}'_{t,t-c} \mu_i + \eta_{it}$$

and therefore that Assumption 2 holds if

$$\mathrm{E}\left(\eta_{it} | \mu_i = \mu,\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right) = \tilde{\zeta}_{t,t-c} + \mathbf{d}' \beta_{t,t-c}$$

in each period t for some $\tilde{\zeta}_{t,t-c}$.

[20] Following the proof of Proposition 5, any data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$ such that $\mathbf{P}_c, \tilde{\mathbf{x}}_c > 0$ for all c, with $\mathrm{sgn}\left(\ln\left(\frac{\tilde{x}_{\overline{c}1}\tilde{x}_{\underline{c}2}}{\tilde{x}_{\overline{c}2}\tilde{x}_{\underline{c}1}}\right)\right) = \mathrm{sgn}\left(\ln\left(\frac{P_{\overline{c}1}P_{\underline{c}2}}{P_{\overline{c}2}P_{\underline{c}1}}\right)\right) \neq 0$, are compatible with our model and with Assumption 1.

in skill production technology. Pietschnig and Voracek (2015, Table 2) argue that higher levels of education are linked especially to greater crystallized intelligence.[21] Improvements in health and nutrition are another potentially important cause of changes in skill production technology. Pietschnig and Voracek (2015, Table 2) argue that some factors in this category (e.g., blood lead levels) do not affect fluid and crystallized intelligence differently,but that some (e.g., nutrition) have larger effects on fluid than crystallized intelligence.[22] Other changes that may have improved skill production technology include increased availability of personal technology (e.g., video games) and a reduction in disease burden (Pietschnig and Voracek 2015, Table 2).[23]

Thus there are factors that favor crystallized intelligence, factors that favor fluid intelligence, and factors that do not favor one or the other. We may think of Assumption 1 as describing a situation where the opposing factors wash out. To the extent that they do not, and that changes in skill production technology favor crystallized intelligence, we expect to understate the role of labor market returns in explaining trends in skills. To the extent that changes instead favor fluid intelligence, we expect to overstate the role of labor market returns.[24]

In our empirical analysis, we explore the sensitivity of our findings to departures from Assumption 1 and to accounting for measurable changes in schooling and health occurring at or before the ages at which we measure skills. We also study skills measured at various ages and therefore at different points in a person's schooling.

Assumption 2 is violated if there are unmeasured factors that directly affect earnings and whose correlation with a person's skill endowment is not proportional to the endowment's market value. In our empirical analysis, we explore the sensitivity of our findings to including proxies for candidate factors in the covariate set $\mathbf{d}_{it}$.

[21] Cliffordson and Gustafsson (2008) and Carlsson et al. (2015) document stronger effects of schooling on crystallized than fluid intelligence using data from the same military enlistment battery that we study.

[22] In a review of the literature, Lam and Lawlis (2017) identify randomized trials showing evidence of effects of micronutrient interventions on both fluid and crystallized intelligence, though with larger effect sizes for fluid intelligence. See also Lynn (2009, pp. 253-254).

[23] Pietschnig and Voracek (2015, pp. 290-291) note that increased access to technology may have improved fluid more than crystallized intelligence, but also that gains in fluid intelligence have been observed in countries and time periods with lower levels of access to modern technology (see also Baker et al., 2015, p. 146). Simons et al. (2016) argue that there is limited evidence of effects of interventions such as video game playing on broader cognitive performance.

[24] Say that $\frac{P_{\overline{c}1}}{P_{\overline{c}2}} > \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$. If $\frac{1}{\overline{c}-\underline{c}} \sum_{c=\underline{c}}^{\overline{c}-1} \left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right)\right] > 0$, then our construction will understate the elasticity of substitution $\frac{1}{\rho-1}$. If $\frac{1}{\overline{c}-\underline{c}} \sum_{c=\underline{c}}^{\overline{c}-1} \left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right)\right] < 0$, then our construction will overstate it.

2.3 Data

2.3.1 Linked Data on Test Scores and Earnings

Our main analysis uses data on scores from tests administered at military enlistment, typically at age 18 or 19, for the near-population of Swedish men born between 1962 and 1975 and who enlisted between 1980 and 1993 (War Archives, 2016). Across all cohorts, these men took identical tests that were part of a group of tests called Enlistment Battery 80. Carlstedt (2000), Gyllenram, Hellström, and Hanes (2015), and Rönnlund et al. (2013) describe the tests in more detail.

To extend our analysis to a broader set of birth cohorts and earlier testing ages, we also use data on scores from tests administered, typically at age 13, as part of the Evaluation Through Follow-up, a large survey of Swedish families (Härnqvist, 2000). These data cover around 10 percent of the birth cohorts 1948, 1953, 1967, 1972, and 1977. Härnqvist (1998) and Svensson (2011) describe the tests, which were unchanged across the cohorts, and the survey in more detail.[25] We focus on males to parallel the military enlistment sample. Appendix 2.D presents supplementary findings for females.

Both data sources include tests for logical reasoning and vocabulary knowledge. In the enlistment data, the logical reasoning test consisted of drawing correct conclusions based on statements that are made complex by distracting negations or conditional clauses and numerical operations (Carlstedt & Mårdberg, 1993; Gyllenram, Hellström, & Hanes, 2015). The vocabulary knowledge test consisted of correctly identifying synonyms to a set of words (Gyllenram, Hellström, & Hanes, 2015). In the survey data, the logical reasoning test consisted of guessing the next in a sequence of numbers, and the vocabulary knowledge test consisted of recognizing antonyms (Svensson, 2011, Chapter 1). In both data sources, we observe the number of questions (out of a total of 40) that each person answered correctly on each test.[26]

We treat performance on the logical reasoning test as our main measure of fluid intelligence ($j = 1$). We treat performance on the vocabulary knowledge test as our main measure of crystallized intelligence ($j = 2$). Pietschnig and Voracek (2015, Table 1) list guessing the next number in a sequence as an example of a task that measures fluid intelligence, and a vocabulary test as an example of a task that measures crystallized

[25] Extensions of our analysis in Appendix 2.D include data for birth cohorts 1982 and 1992, for which we can measure skill levels but have more limited information on earnings. The test administered to the 1982 and 1992 cohorts differs slightly from the test administered to earlier cohorts in aspects such as the order of possible answers.

[26] Both data sources also include a test of spatial reasoning, which we use in sensitivity analysis. Appendix Figure 2.A shows trends in the level of and premium for technical skills, which are measured in the military enlistment data but not in the survey data. Appendix Figure 2.B shows trends in the levels of and premia for skills in the military enlistment data for men born between 1954 and 1961, for which the format of the tests was different (War Archives, 2016).

intelligence.[27]

Enlistees were assigned to military positions in part based on a composite cognitive score that depended on the logical reasoning test, the vocabulary knowledge test, and other tests (Grönqvist & Lindqvist, 2016, pp. 873-874, 877, 880). We are not aware of any incentives attached to the individual cognitive test components (e.g., logical reasoning, vocabulary knowledge), as opposed to the composite cognitive score, or any reason why incentives to perform well on the tests would have differed by birth cohort. The test questions are classified so could not be practiced in advance, and the exact mapping from individual cognitive test components to the composite cognitive score was not publicly known at the time of the tests. We are not aware of any incentives attached to performance on the survey tests, which are not publicly available.

We include in our analysis only those individuals for whom we observe valid logical reasoning and vocabulary knowledge scores. For each data source and each dimension j, we let x_{ij} denote the percentile rank of individual i's score within the distribution of scores of those born in 1967.[28] The skill vector $\mathbf{x}_i = (x_{i1}, x_{i2})$ then measures the performance of individual i on each dimension j relative to the set of individuals born in 1967. Appendix Table 2.A shows the number of individuals in each birth cohort for each data source.

We join both sources of test scores to information on labor market earnings for the universe of Swedish residents from the Income and Tax Register for the years 1968–2018.[29] For each individual i in each year t, we let w_{it} be the total gross labor market earnings.

Portions of our analysis use additional variables. From the enlistment data War Archives (2016), we obtain the date on which an individual took the enlistment tests,[30] the individual's height and weight as of enlistment, and a measure of non-cognitive skill that follows a standardized distribution.[31] From other sources we obtain administrative data on each individual's employment history (Statistics Sweden, 2020b, 2021), foreign-born status (Statistics Sweden, 2014a), secondary schooling completion (Statistics Sweden, 2014c), region of birth (Statistics Sweden, 2021), family relations (Statistics Sweden, 2014a), and parental labor market earnings (Statistics Sweden, 2014b, 2021).

[27]Carroll (1993, pp. 598-899) lists induction and sequential reasoning as two of the three factors most frequently associated with fluid intelligence, and verbal ability as the factor most frequently associated with crystallized intelligence, in a tabulation based on a hierarchical factor analysis (see also Flanagan & Dixon, 2014).

[28]Specifically, x_{ij} is equal to the average rank of sample individuals born in 1967 who have the same score as individual i on dimension j, multiplied by 100, divided by the number of sample individuals born in 1967, and centered by adding a constant so that x_{ij} has an average value of 50 among those born in 1967.

[29]Data on labor market earnings for 1990–2018 are from Statistics Sweden (2021), where we define gross labor market earnings using the concept described in Statistics Sweden (2016a, pp. 137-138). Data for 1968–1989 are from Statistics Sweden (2014b), where we approximate the concept described in Statistics Sweden (2016a, pp. 137-138) using the available data fields. For sensitivity analysis we also obtain data on business income for 1990–2018 from Statistics Sweden (2021). We define a total income measure combining labor market earnings and business income using the concept described in Statistics Sweden (2016a, pp. 141-142).

[30]We match information on enlistment test date to our other data using information on parish of residence from Statistics Sweden (2016b).

[31]Non-cognitive skill is evaluated based on an interview and scored on a Stanine (1–9) scale. Lindqvist and Vestman (2011, pp. 107-109 and Appendix F) and Edin et al. (2022, p. 6) describe the measure in more detail.

Appendix Table 2.B presents sensitivity analyses with respect to many of the choices we have made in constructing the sample and variables for our analysis, including varying the set of included cohorts, measuring an individual's skill with the percent of the maximum possible score rather than with the percentile rank, combining logical and spatial reasoning skills into a single composite measure of fluid intelligence, and including business income in the measure of earnings. We summarize the quantitative implications of these choices in Section 2.4.2.

2.3.2 Original Survey of Parents' Perceptions

We conducted an original survey to assess the importance that parents place on different types of skills. We hosted the survey on a Stockholm University survey platform. We recruited participants via Facebook ads from October 17 through October 24, 2020. During this time, 1,199 respondents began the survey and 983 completed it. We asked each respondent their own year of birth as well as the range of birth years of their children, if any. We include in our analysis the 716 respondents who reported that their first child was born at least 16 years after their own birth year.

We asked these respondents the following question:

> *As a parent, how much do you encourage (or did you encourage) your children to develop the qualities below while growing up?*
>
> *To be able to think critically and solve problems logically.*
>
> *To be able to remember facts, such as the definitions of difficult words.*

We intended the first quality to approximate the concept of fluid intelligence and the second to approximate the concept of crystallized intelligence. We also asked respondents about the importance of each quality in today's society, how much their own parents emphasized each quality, and how much their own primary school emphasized each quality. There were five possible answers ranging from "Not at all" to "Very much," and we classified each response according to whether the person rated the first quality as more important, the second quality as more important, or neither.

Appendix Figure 2.C gives screenshots of the consent form and survey form. Appendix Figure 2.D shows the distribution of year of birth, and year of birth of first child, among the respondents in our sample.

2.4 Results

2.4.1 Trends in Skills and Skill Premia

We let $c(i)$ be the year that worker i turns 29 and we let $A = 26$, so that the working life is from ages 30 through 55. Appendix Figure 2.E shows that full-time work tends to be highest during these years.

We estimate the parameter $\mathbf{p}_{t,a}$ in equation (2.1) by ordinary least squares regression of the log of labor market earnings $\ln(w_{it})$ on the vector of percentile ranks $\mathbf{x}_i$, separately for each worker experience level (age) a and for each year t for which we measure earnings, excluding men with zero earnings. This yields an estimate of $\mathbf{p}_{c+a,a}$ for each c, a such that $c + a \leq T$, for T the most recent year of earnings data available. Appendix Figure 2.F illustrates the fit of the regression model for three example cohorts at three different ages.

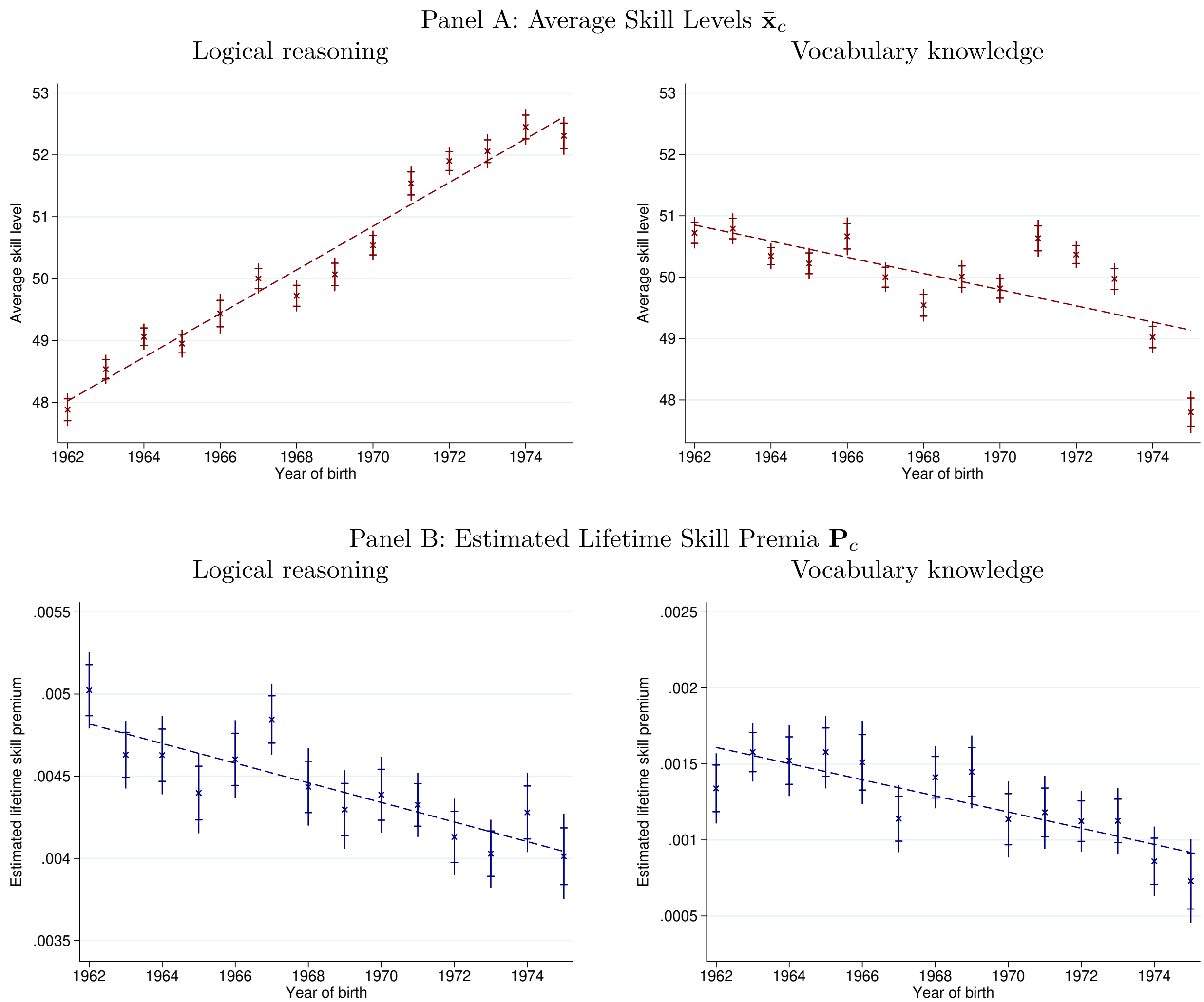
To estimate $\mathbf{p}_{c+a,a}$ for c, a such that $c + a > T$, we take the average estimate for the given cohort c for all ages $a > 10$ for which a regression estimate of $\mathbf{p}_{c+a,a}$ is available. Appendix Figure 2.G illustrates this extrapolation for three example cohorts. We plug the resulting estimates of $\mathbf{p}_{c+a,a}$ into equation (2.3), along with the value $\delta = 0.96$, to get an estimate of the lifetime skill premia $\mathbf{P}_c$ for the cohorts $c \in \{\underline{c}, ..., \overline{c}\}$. We obtain standard errors for $\mathbf{P}_c$ via a nonparametric bootstrap in which we sample individuals i with replacement.

Figure 2.1 depicts the average skill levels $\overline{\mathbf{x}}_c$ and the estimated lifetime skill premia $\mathbf{P}_c$ across cohorts in the enlistment data along with their 95 percent pointwise confidence intervals and uniform confidence bands. For convenience we label cohorts with their birth year, i.e., $c - 29$. Figure 2.1 also depicts the lines of best fit through the plotted series.

Panel A of Figure 2.1 shows that logical reasoning skill rose, on average, by 4.4 percentile points, relative to the 1967 distribution, across the birth cohorts from 1962 to 1975. By contrast, vocabulary knowledge skill fell, on average, by 2.9 percentile points. Appendix Figure 2.H depicts the cumulative distribution functions of skills in the 1962 and 1975 cohorts. Appendix Figure 2.I compares trends in skill in our data to those measured in other countries.

Panel B of Figure 2.1 shows that the lifetime skill premium fell for both logical reasoning and vocabulary knowledge. The line of best fit indicates that the lifetime premium for a percentile point of logical reasoning skill fell from 0.48 to 0.40 log points across the birth cohorts from 1962 to 1975, and the lifetime premium for a percentile point of vocabulary knowledge fell from 0.16 to 0.09 log points. Thus, the lifetime premium

Figure 2.1: Trends in Skills and Skill Premia across Birth Cohorts 1962–1975, Military Enlistment Sample



Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. Panel A depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1967 birth cohort. Panel B depicts the estimated lifetime skill premia $\mathbf{P}_c$ for each birth cohort, constructed as described in Section 2.4.1. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

for both skill dimensions fell, with a proportionately much greater decline for vocabulary knowledge.[32] Appendix Figure 2.J depicts estimated lifetime skill premia based on a generalization of equation (2.1) that allows interactions between the skill dimensions.

Panel A of Figure 2.2 depicts the evolution of the relative skill levels $\ln(\overline{x}_{c1}/\overline{x}_{c2})$ and of the relative lifetime skill premia $\ln(P_{c1}/P_{c2})$ across the two dimensions. The plot shows that both objects tend to

[32] Prior work finding evidence of declining returns to cognitive skill includes Castex and Dechter (2014) for the US, Markussen and Røed (2020) for Norway, and Edin et al. (2022) for Sweden.

increase with later birth cohorts and are fairly close to the line of best fit, evoking a movement along a relative linear supply curve as in equation (2.5). Figure 2.3 shows that a similar qualitative pattern obtains in our survey sample, which is smaller and for which estimates tend to be less precise. Appendix Figure 2.K depicts the underlying estimates of skill levels and lifetime skill premia for men in the survey sample. Appendix Figure 2.L depicts the evolution of relative skill levels and relative lifetime skill premia for women in the survey sample. Appendix Figure 2.M depicts the evolution of relative skill levels and relative lifetime skill premia in the enlistment sample by region of birth.

Under the conditions in Appendix 2.C, our approach to identification and estimation of relative skill premia remains valid even in the presence of measurement error in skills. As an alternative exploration of the role of measurement error, requiring different assumptions from those in Appendix 2.C, Panel A of Appendix Table 2.E shows estimates of the trend in skill premia computed using the individuals present in both the enlistment and survey data, instrumenting for skills measured at enlistment with skills measured in the survey. The sample is small and the instrumental variables estimates are imprecise. The confidence intervals on the estimated trends include zero and also include the slope of the linear fit from Panel B of Figure 2.1. Relative to the slope of the linear fit from Panel B of Figure 2.1, instrumental variables estimates tend to show growth in the premium to logical reasoning and more rapid decline in the premium to vocabulary knowledge, suggesting even stronger trends in labor-market incentives to invest in logical reasoning at the expense of vocabulary knowledge than in our baseline calculations. Panel B of Appendix Table 2.E reports small and statistically insignificant trends in the correlation between skills measured in the survey data and those measured in the enlistment data.

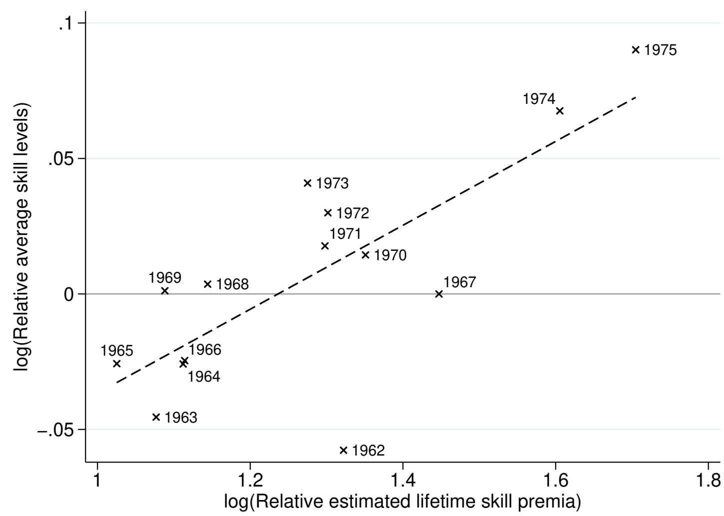
Appendix Table 2.B presents sensitivity analyses with respect to many of the choices we have made in constructing the sample and variables for our analysis, including altering the assumed ages of working life, restricting to workers who are employed year-round in a typical year, averaging over a shorter or longer span of ages to extrapolate premia to working years we do not observe in the data, and varying the assumed value of δ. We summarize the quantitative implications of these choices in Section 2.4.2.

2.4.2 Model Estimates and Counterfactuals

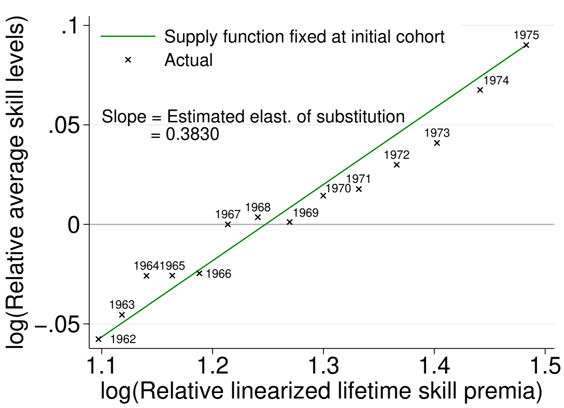
We estimate the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$ for each cohort in the enlistment sample following the construction in the proof of Proposition 5. We take $J = 2$. We take the average skill $\overline{\mathbf{x}}_c$ in each cohort as our estimate of $\tilde{\mathbf{x}}_c$. We take the linear fit in Panel B of Figure 2.1 as our estimate of the lifetime skill premia $\mathbf{P}_c$.[33] We

[33] Consistent with the regularity condition in Proposition 5, based on the linear fit we reject the null hypothesis that $\ln\left(\frac{P_{\underline{c}1}}{P_{\underline{c}2}}\right) = \ln\left(\frac{P_{\overline{c}1}}{P_{\overline{c}2}}\right)$ at conventional significance levels ($p = 0.0006$).

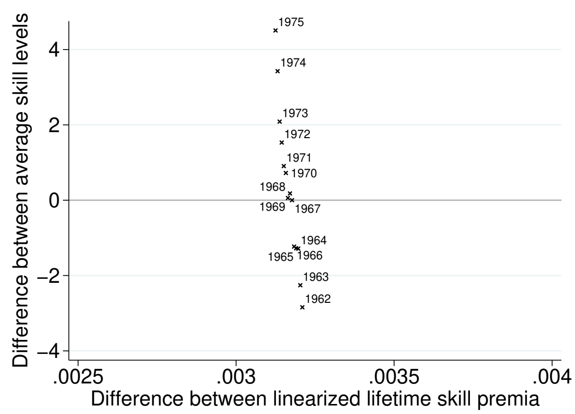
Figure 2.2: Evolution of Relative Skill Levels and Relative Skill Premia, Military Enlistment Sample



Panel A: Relative Skill Levels and Relative Skill Premia



Panel B: Illustration of Relative Supply Function



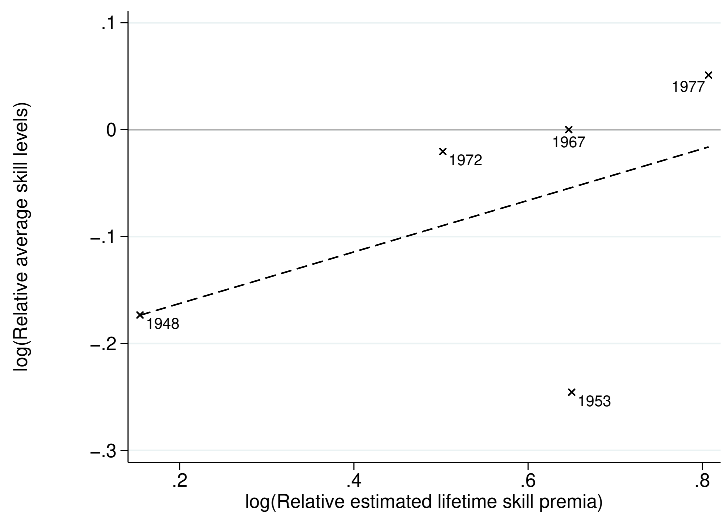
Panel C: Differences in Skills and Skill Premia

Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. Panel A shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln(\overline{x}_{c1}/\overline{x}_{c2})$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln(P_{c1}/P_{c2})$. The dashed line depicts the line of best fit. Panel B shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln(\overline{x}_{c1}/\overline{x}_{c2})$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln(P_{c1}/P_{c2})$, based on the linearized skill premia depicted in Panel B of Figure 2.1. The solid line shows the relative skill supply function estimated for the 1962 birth cohort, i.e., the relationship between $\ln\left(\frac{\tilde{x}_{\underline{c},1}(\mathbf{P}_c)}{\tilde{x}_{\underline{c},2}(\mathbf{P}_c)}\right)$ and $\ln(P_{c1}/P_{c2})$. The slope of the solid line is equal to the estimated elasticity of substitution $\frac{1}{\rho-1}$. Panel C shows a scatterplot of the difference between average skill levels, $\overline{x}_{c1} - \overline{x}_{c2}$, against the difference between estimated lifetime skill premia, $P_{c1} - P_{c2}$, based on the linearized skill premia depicted in Panel B of Figure 2.1. The ratio of the x-axis range to the x-axis value for the 1962 birth cohort is equal to the analogous ratio in Panel A.

may think of the linear fit either as a way of smoothing the sampling variation in the data, or as a way of approximating the forward-looking expectations of workers at the time the skill investment decision is made. Panel A of Table 2.1 reports estimates of key parameters.

Figure 2.4 shows the evolution of logical reasoning and vocabulary knowledge skill in the data and in

Figure 2.3: Evolution of Relative Skill Levels and Relative Skill Premia, Survey Sample



Notes: Data are from the survey sample covering birth cohorts 1948, 1953, 1967, 1972, and 1977, with tests typically taken at age 13. The plot shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln(\overline{x}_{c1}/\overline{x}_{c2})$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln(P_{c1}/P_{c2})$. The dashed line depicts the line of best fit.

the counterfactual scenario in which the lifetime skill premia $\mathbf{P}_c$ remain constant at their initial level $\mathbf{P}_{\underline{c}}$. In the counterfactual scenario, logical reasoning skill increases by 2.8 percentile points instead of 4.4 as in the actual data. Vocabulary knowledge skill increases by 3.0 percentile points rather than falling by 2.9 percentile points. In this sense, according to the model, changes in the lifetime skill premia $\mathbf{P}_c$ account for 36.8 percent of the increase in logical reasoning skill (with a standard error of 1.7 percent), and for more than the entire decline in vocabulary knowledge skill.

To unpack the findings in Figure 2.4, begin with estimation of the elasticity of substitution $\frac{1}{\rho-1}$. Under Assumption 1, all long-term change in relative skill levels across cohorts must be due to change in relative skill premia. In particular, the elasticity of substitution $\frac{1}{\rho-1}$ can be estimated as the ratio of the long-term change in relative skill levels to the long-term change in relative skill premia. Panel B of Figure 2.2 illustrates by plotting the log of the relative estimated average skill level $\ln(\overline{x}_{c1}/\overline{x}_{c2})$ against the log of the relative estimated (linearized) skill premia $\ln(P_{c1}/P_{c2})$. Under Assumption 1, the linear relative supply curve $\ln\left(\frac{\tilde{x}_{\underline{c}1}(\cdot)}{\tilde{x}_{\underline{c}2}(\cdot)}\right)$ defined by the estimated skill supply function $\tilde{\mathbf{x}}_{\underline{c}}(\cdot)$ for the 1962 birth cohort must pass through the points on the scatterplot for both the 1962 and 1975 birth cohorts. This implies an elasticity of substitution of $\frac{1}{\rho-1} = 0.383$, which is in turn the slope of the line $\ln\left(\frac{\tilde{x}_{\underline{c}1}(\cdot)}{\tilde{x}_{\underline{c}2}(\cdot)}\right)$ depicted on the plot.

Next, consider estimation of the remaining parameters of the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$. Given the data, under any elasticity of substitution less than 0.97, the model implies that changes in relative premia alone are too small to explain the large increase in logical reasoning skill. We can therefore infer an upward shift

Table 2.1: Summary of data and model implications

Panel A: Baseline

	Logical reasoning	Vocabulary knowledge
Initial lifetime skill premium, 1962	0.0048	0.0016
$P_{\underline{c}j}$	(0.0001)	(0.0001)
Change in lifetime skill premium, 1962–1975	-0.0008	-0.0007
$P_{\overline{c}j} - P_{\underline{c}j}$	(0.0001)	(0.0001)
Initial average skill rank, 1962	47.88	50.72
$\bar{x}_{\overline{c}j}$	(0.14)	(0.13)
Change in average skill rank 1962–1975	4.43	-2.92
$\bar{x}_{\overline{c}j} - \bar{x}_{\underline{c}j}$	(0.22)	(0.21)
Under estimated model:		
Change in average skill rank, 1962–1975 at initial skill premia	2.80	2.97
$\tilde{x}_{\overline{c}j}\left(\mathbf{P}_{\underline{c}}\right) - \tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}}\right)$	(0.21)	(0.22)
Share of observed change explained by change in skill premia	0.3681	2.0151
$1 - \frac{\tilde{x}_{\overline{c}j}\left(\mathbf{P}_{\underline{c}}\right) - \tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}}\right)}{\bar{x}_{\overline{c}j} - \bar{x}_{\underline{c}j}}$	(0.0175)	(0.1483)
Substitution parameter ρ	3.61 (0.76)	
[Implied elasticity of substitution $1/(\rho - 1)$]	[0.3830]	

Panel B: Accounting for Non-Cognitive Skills

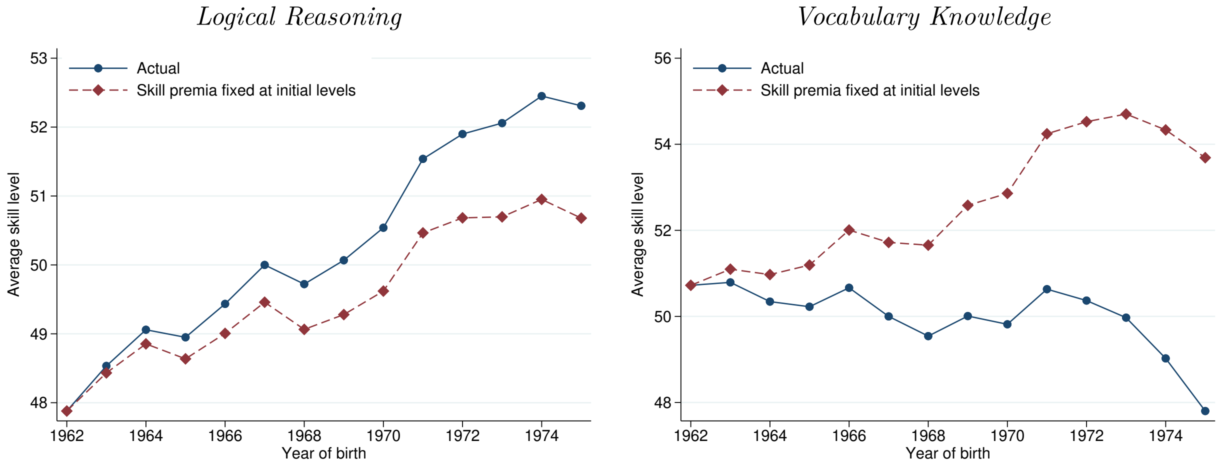
	Logical reasoning	Vocabulary knowledge
Initial lifetime skill premium, 1962	0.0037	0.0009
$P_{\underline{c}j}$	(0.0001)	(0.0001)
Change in lifetime skill premium, 1962–1975	-0.0009	-0.0006
$P_{\overline{c}j} - P_{\underline{c}j}$	(0.0001)	(0.0001)
Initial average skill rank, 1962	47.88	50.72
$\bar{x}_{\overline{c}j}$	(0.14)	(0.13)
Change in average skill rank 1962–1975	4.43	-2.92
$\bar{x}_{\overline{c}j} - \bar{x}_{\underline{c}j}$	(0.22)	(0.21)
Under estimated model:		
Change in average skill rank, 1962–1975 at initial skill premia	3.27	3.46
$\tilde{x}_{\overline{c}j}\left(\mathbf{P}_{\underline{c}}; \tilde{\mathbf{x}}_{\overline{c},L+1:J}\right) - \tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}}; \tilde{\mathbf{x}}_{\underline{c},L+1:J}\right)$	(0.22)	(0.24)
Share of observed change explained by change in skill premia	0.2617	2.1860
$1 - \frac{\tilde{x}_{\overline{c}j}\left(\mathbf{P}_{\underline{c}}; \tilde{\mathbf{x}}_{\overline{c},L+1:J}\right) - \tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}}; \tilde{\mathbf{x}}_{\underline{c},L+1:J}\right)}{\bar{x}_{\overline{c}j} - \bar{x}_{\underline{c}j}}$	(0.0206)	(0.1636)
Substitution parameter ρ	5.72 (1.65)	
[Implied elasticity of substitution $1/(\rho - 1)$]	[0.2120]	

Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In Panel A, estimates of $\overline{\mathbf{x}}_c$ and $\mathbf{P}_c$ follow Figure 2.1 with the linear fit used as our estimate of $\mathbf{P}_c$. Estimates of $\tilde{\mathbf{x}}_c(\cdot)$ follow the proof of Proposition 5. The unknown parameters are ρ and $\left\{\mathbf{K}_c, \overline{S}_c\right\}_{c=\underline{c}}^{\overline{c}}$. Take $\overline{\mathbf{x}}_c$ as our estimate of $\tilde{\mathbf{x}}_c$. Then estimate the elasticity of substitution $1/(\rho - 1)$ following equation (2.7). Next, estimate the relative cost parameters K_{c2}/K_{c1} in each cohort c following equation (2.8). From the normalization used in the proof of Proposition 5, estimate K_{c1} following equation (2.9), from which estimate K_{c2} using the ratio K_{c2}/K_{c1}. Finally, estimate the skill budget $\overline{S}_c$ following equation (2.10). In Panel B, estimates follow Section 2.6, with $L = 2$ and $J = 3$. We estimate $\mathbf{P}_{c,1:L}$ from earnings regressions that control for a standardized measure of non-cognitive skill, excluding from the sample any worker missing information on non-cognitive skill. The rest of the analysis follows similarly to Panel A, following the logic in the proof of Proposition 7.

in the first dimension of the skill supply function $\tilde{x}_{c1}(\cdot)$ across cohorts, i.e., growth in logical reasoning skill beyond what can be explained by changes in premia alone. And, given Assumption 1, the model implies that there must also have been an upward shift in the second dimension of the skill supply function $\tilde{x}_{c2}(\cdot)$ across cohorts, i.e., that vocabulary knowledge would have risen absent changes in skill premia.

Following the constant elasticity form of the transformation function in equation (2.4) and the log-linear

Figure 2.4: Decomposition of Change in Average Skill Level, Military Enlistment Sample

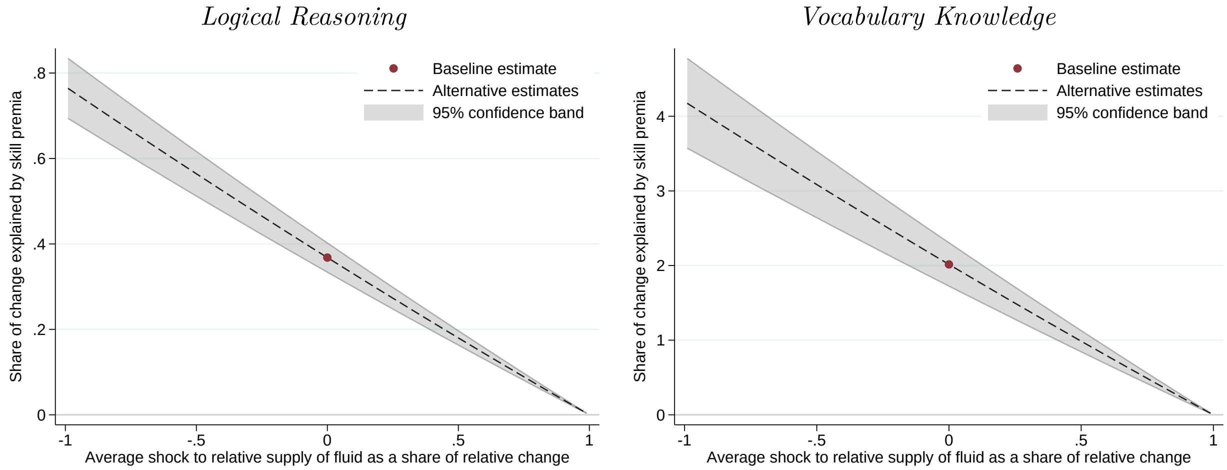


Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. Each plot depicts the average skill $\overline{\mathbf{x}}_c$ for each birth cohort c ("Actual") and the predicted average skill $\tilde{\mathbf{x}}_c\left(\mathbf{P}_{\underline{c}}\right)$ under the counterfactual in which lifetime skill premia remain at the level estimated for the 1962 birth cohort ("Skill premia fixed at initial levels"). Skills are expressed as a percentile of the distribution for the 1967 birth cohort.

form of the relative supply function in equation (2.5), our discussion has focused on ratios of skill premia rather than on their differences. It seems likely that a model focusing instead on differences in premia would imply a different conclusion regarding the role of changes in premia in explaining cohort trends in skill levels. To illustrate why, Panel C of Figure 2.2 presents an analogue of the scatterplot in Panel A of Figure 2.2, but replacing log ratios of skill levels and skill premia with their differences. Panel B of Figure 2.2 shows that the difference in premia between logical reasoning and vocabulary knowledge did not rise across successive cohorts in the way that Panel A of Figure 2.2 shows that the ratio of premia did. Following Figure 2.1, we find it intuitive that as the premium to vocabulary knowledge fell to a very low level while the premium to logical reasoning skill remained nontrivial, individuals would substitute effort away from vocabulary knowledge, as implied by the constant elasticity form of the transformation function in equation (2.4).

Appendix Table 2.B presents sensitivity analysis with respect to choices we have made in constructing the sample and variables for our analysis. Rows (b) and (c) concern the set of birth cohorts we include. Rows (d) and (e) concern the measurement of skills $\mathbf{x}_i$. Row (f) concerns the measurement of earnings w_{it}. Rows (g) and (h) concern the experience levels a and individuals i included in the analysis. Rows (i) through (m) concern the construction of estimates of lifetime skill premia $\mathbf{P}_c$ from estimates of period-specific premia $\mathbf{p}_{c+a,a}$. Rows (n) and (o) concern the smoothing of the estimated lifetime skill premia $\mathbf{P}_c$. Across these different sensitivity analyses, we estimate that changes in lifetime skill premia account for between 29.4 and 46.5 percent of the increase in logical reasoning skill, which can be compared to our baseline estimate of

Figure 2.5: Sensitivity to Departures From Zero Average Relative Supply Shock



Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. In each plot, the curve labeled "Alternative estimates" depicts the estimated share $1-\frac{\tilde{x}_{\bar{c}j}\left(\mathbf{P}_{\underline{c}}\right)-\tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}}\right)}{\tilde{x}_{\bar{c}j}-\tilde{x}_{\underline{c}j}}$ of the change in observed skills on dimension j explained by the change in skill premia (y-axis) as a function of the average relative supply shock $-\frac{1}{\bar{c}-\underline{c}}\sum_{c=\underline{c}}^{\bar{c}-1}\left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right)-\ln\left(\frac{K_{c1}}{K_{c2}}\right)\right]$ (x-axis). The average relative supply shock is expressed as a share of the estimated change $\ln\left(\tilde{x}_{\bar{c}1}\tilde{x}_{\bar{c}2}\right)-\ln\left(\tilde{x}_{\underline{c}1}/\tilde{x}_{\underline{c}2}\right)$ in relative skill levels between the 1962 and 1975 birth cohorts, with positive values implying changes in skill-producing technology that favor fluid relative to crystallized intelligence. The shaded region collects pointwise 95% confidence intervals obtained via a nonparametric bootstrap with 50 replicates. The estimate labeled "Baseline estimate" corresponds to the estimate in Panel A of Table 2.1, obtained under Assumption 1.

36.8 percent. Appendix Figure 2.N extends our analysis to a larger set of cohorts, and to women, using the survey sample. We estimate that changes in lifetime skill premia account for a larger share of the increase in logical reasoning skill than in our baseline estimate, though the estimates from the survey sample are less precise than our baseline estimate.

2.4.3 Sensitivity to Assumption 1

Figure 2.5 shows how our conclusions change as we depart from Assumption 1. The upper plot is for logical reasoning skill and the lower plot is for vocabulary knowledge. Each plot shows the relationship between the estimated share of the change in the given skill dimension explained by changes in the lifetime skill premia (y-axis) and the average relative shock to the supply of skill (x-axis). We measure the shock as a fraction of the observed change in relative skill levels. A positive shock implies that changes in skill-producing technology favored fluid intelligence over crystallized intelligence, on average across the cohorts that we study. A negative shock implies the reverse. A shock of zero corresponds to the case in which Assumption 1 holds, and thus to the estimates in Figure 2.4 and Panel A of Table 2.1.

A reader can use Figure 2.5 to gauge the effect of a given departure from Assumption 1 on our conclusions.

Figure 2.5 thus improves transparency in the sense of **AndrewsEtAl2020**; Andrews, Gentzkow, and Shapiro (2017) and Andrews and Shapiro (2021).

To illustrate the utility of Figure 2.5 with an example, consider the possibility that changes across cohorts in time spent in school shifted the relative supply of different skills. Carlsson et al. (2015) estimate that additional time in school improves performance on the vocabulary knowledge test that we study, and do not find evidence that additional time in school improves performance on the logical reasoning test. We estimate that, relative to the 1962 birth cohort, members of the 1975 birth cohort spent 0.40 more years in school as of the date of test-taking. If at least some of the increase in schooling time would have occurred absent changes in skill premia, then Carlsson et al.'s (2015) analysis implies that increased schooling time can be considered a positive shock to the relative supply of crystallized intelligence, or equivalently a negative shock to the relative supply of fluid intelligence. Figure 2.5 shows that if there is a negative shock to the relative supply of fluid intelligence, then our baseline estimates understate the share of the change in skill levels that can be accounted for by changes in skill premia. If we take the entire increase in schooling time as a supply shock, and assume no other shocks to the relative supply of the two skill dimensions, we can use the estimates in Carlsson et al. (2015) in tandem with Figure 2.5 to calculate that changes in lifetime skill premia explain 53.5 percent of the observed increase in logical reasoning skill, which is 16.7 percentage points more than our baseline estimate of 36.8 obtained under Assumption 1.[34]

A similar exercise is possible with respect to assumptions about the measurement of skill. To illustrate, Appendix Figure 2.O depicts our findings regarding trends in actual and counterfactual skills under the assumption that a portion of the cohort trend in logical reasoning skill (upper panel) or vocabulary knowledge skill (lower panel) is spurious. One possible source of spurious trends is a general improvement in test-taking ability (e.g., Neisser 1997; Jensen 1998, pp. 332-333), though this would not by itself explain the simultaneous rise in logical reasoning skill and decline in vocabulary knowledge. Another possible source of spurious trends, specific to vocabulary knowledge, is greater test difficulty for later cohorts due to gradual obsolescence of the words on the test (e.g., Hauser and Huang 1997; Alwin and Pacheco 2012; Roivainen 2014). Appendix Figure 2.O shows that if a portion of the measured decline in vocabulary knowledge is spurious, our analysis will tend to overstate the role of labor market returns in explaining cohort trends in logical skill, though

[34] Carlsson et al. (2015, Table 3, column 1) estimate that an additional 100 days of schooling increases performance in the vocabulary knowledge test by 0.112 standard deviations, relative to the population of test-takers in 1980–1994. Among individuals in our enlistment data, those born in 1975 completed on average 0.40 more years of schooling at enlistment than those born in 1962. As there are roughly 180 schooling days per year in Sweden (Carlsson et al. 2015, p. 538), this implies an increase of 0.0803 standard deviations in vocabulary knowledge skill. Interpolating around the median test score, we estimate that an increase of 0.0803 standard deviations in vocabulary test score is equivalent to an increase of 3.29 percentile points among those born in 1962. Based on the skill levels reported for the 1962 cohort in Panel A of Table 2.1, an increase of 3.29 percentile points in vocabulary knowledge skill would have reduced the log ratio of logical reasoning and vocabulary knowledge skills by 0.063, or by 0.426 of the observed change. Given a relative supply shock of -0.426, Figure 2.5 implies that changes in skill premia account for 53.5 percent of the observed increase in logical reasoning.

even if there were no trend in vocabulary knowledge we would still infer that 22.7 percent (SE = 0.6) of the trend in logical skill was due to changes in labor market returns. As more concrete evidence on trends in word usage, Appendix Figure 2.P shows estimates of the exposure of each cohort to words on example synonym questions for a recent enlistment battery, measuring word exposure based on usage in a major Swedish newspaper. The hypothesis that words on the enlistment battery are more familiar to those born closer to the time of the test design would predict an increasing trend in exposure. We do not find evidence of such a trend.

2.4.4 Sensitivity to Controls

We explore the sensitivity of our conclusions to adjusting for covariates. We adjust both the estimated trend in mean skills $\overline{\mathbf{x}}_c$ and the estimated trend in lifetime skill premia $\mathbf{P}_c$ with respect to individual-specific, time-invariant covariates $\mathbf{d}_i$ that are normalized to have mean zero among those born in 1967. We adjust the estimated trend in mean skills by estimating a regression of skills x_{ij} on cohort indicators and covariates $\mathbf{d}_i$, excluding the constant.[35] We then treat the coefficients on the cohort indicators as a covariate-adjusted measure of mean skills. We adjust the estimated trend in lifetime skill premia $\mathbf{P}_c$ by including the covariates $\mathbf{d}_i$ in the time-and-age-specific earnings regressions from which we estimate $\mathbf{p}_{t,a}$.

Selection of covariates for inclusion in this exercise is delicate. For adjusting the trend in mean skills, we wish to consider adjusting only for covariates whose cohort trends do not respond to skill premia $\mathbf{P}_c$. For example, if a trend in mean heights would have occurred even absent changes in $\mathbf{P}_c$, then it may be appropriate to adjust the trend in mean skills for the trend in mean heights, and thus to study the effect of skill premia $\mathbf{P}_c$ on the part of the trend in skills that cannot be accounted for by the trend in height. By contrast, if trends in the content of schooling occur in response to changes in skill premia $\mathbf{P}_c$, then these trends are part of the skill investment process that we model, and we do not want to study the effect of skill premia $\mathbf{P}_c$ on only the part of the trend in skills that cannot be accounted for by the trend in the content of schooling.[36] Likewise, for adjusting the trend in lifetime skill premia $\mathbf{P}_c$, we wish to consider adjusting only for covariates that exert a direct effect on earnings independently of their relationship to skills.

Appendix Table 2.C shows how our findings change when we adjust for age at enlistment, an indicator

[35] Within the model in Section 2.2, we may think of this exercise as re-normalizing the skill endowment μ_i to have cohort-specific mean $\Gamma\overline{\mathbf{d}}_c$ where $\overline{\mathbf{d}}_c$ is the cohort-specific mean of $\mathbf{d}_i$ and Γ is a matrix whose j^{th} row contains the coefficients on $\mathbf{d}_i$ in the regression of skills x_{ij} on cohort indicators and covariates $\mathbf{d}_i$.

[36] Trends in parents' skills may likewise be attributable to (earlier) trends in labor market returns. Suppose, for example, that for each cohort c and skill j, $K_{cj} = \underline{K}_{cj}\overline{x}_{c-g,j}^{-\phi}$ where $\underline{K}_{cj} > 0$ is a scalar, $\overline{x}_{c-g,j}$ is the mean skill level in the parental cohort born $g > 0$ years before cohort c, and $\phi \geq 0$ is a parameter governing the intergenerational transmission of skills. Then if we envision a counterfactual change to the time path of skill premia, the skill investment of cohort c will change both due to a direct effect on its incentives, and an indirect effect via the incentives of the parental cohort $c - g$.

for having completed secondary school at the time of enlistment or at age 18, log(height) and log(weight) measured at the time of enlistment, and an indicator for being born outside of Sweden. Across these exercises, we find that changes in labor market returns consistently account for at least 35.5 percent of the increase in logical skill, and for more than the entire decline in vocabulary knowledge skill.

2.4.5 Heterogeneity

Appendix Table 2.D shows how our findings change when we estimate the model separately for workers with below- vs. above-median parental earnings.[37] We estimate that changes in skill premia explain 1.3 percentage points more of the increase in logical reasoning skill for those whose parents have above-median earnings than for those whose parents have below-median earnings, though the difference is not statistically significant (SE = 4.3).

2.5 Trends in Emphasis among Parents, Schools, and Occupations

Sections 2.5.1 and 2.5.2 explore whether parents and schools increasingly emphasize reasoning over knowledge. Section 2.5.3 explores whether changes in the occupation mix favor reasoning-intensive as opposed to knowledge-intensive occupations. Evidence that parents, schools, and occupations have shifted to emphasize reasoning over knowledge does not, on its own, establish that changes in production technology are driving changes in skill investment. Such evidence can, however, serve to make tangible some of the real-world processes that underlie the skill investment decision modeled in Section 2.2.2 and the production economy modeled in Section 2.2.1.

2.5.1 Parents

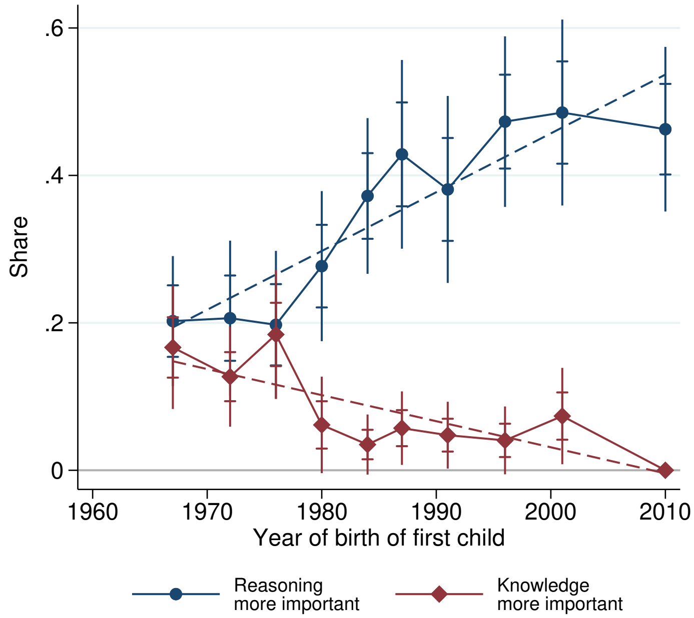
Panel A of Figure 2.6 depicts trends in the perceived importance of different skills among parents, as reported in the survey described in Section 2.3.2. Parents of more recent birth cohorts place more emphasis on reasoning skills and less emphasis on knowledge, compared to parents of earlier birth cohorts. Panel B depicts trends in respondents' perception of the importance of different skills in today's society, how much their own parents emphasized each skill, and how much their own primary school emphasized each skill. There is some visual evidence that younger parents perceive logical skills to be more important than do older parents. Parents' perceptions of what skills were emphasized by their own parents and primary schools

[37] To nest this exercise within the model in Section 2.2, we can suppose there are two distinct labor markets, one for each group of workers, with the two markets potentially linked by a common production function $F_t(\cdot)$.

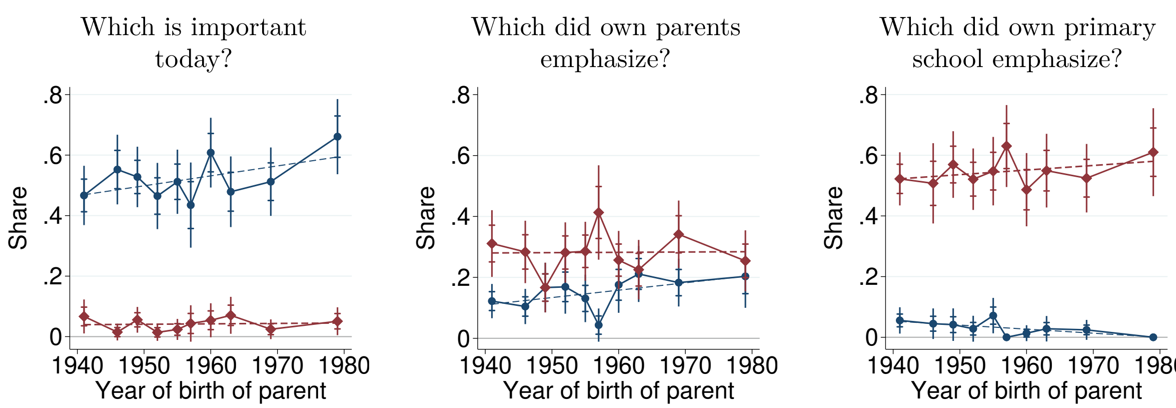
do not show a clear trend.

Figure 2.6: Trends in the Perceived Importance of Different Skills in the Survey of Parents' Perceptions

Panel A: Which Skill Did Parents Encourage More in Their Own Children?



Panel B: Other Measures of Importance



Notes: Data are from the original survey of parents' perceptions described in Section 2.3.2. Each figure shows the fraction of respondents rating reasoning as more important (circles) and the fraction rating knowledge as more important (diamonds), separately by decile of the birth cohort of the respondent's first child (Panel A) or of the respondent (Panel B), with deciles labeled by the integer-rounded mean year of birth within the decile. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates, stratified by birth cohort decile. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

2.5.2 Schools

We can also investigate changes in school curricula over the period we study. We focus on primary schooling because Figure 2.3 suggests that the trends in skill levels that we study emerge at young ages. The primary school curriculum in Sweden is summarized in an official Curriculum ("Läroplan") that is revised from time to time. Meeting society's demands is an explicit goal of the primary schooling system,[38] and although vocational training is not given in primary school, the needs of the workplace have sometimes played a direct role in the development of the Curriculum.[39]

Scholars of pedagogy in Sweden have noted a trend in the Curricula towards greater emphasis over time on problem solving and critical thinking. For example, in an investigation of long-term trends in the teaching of scientific inquiry, Johansson and Wickman (2012) conclude that, "The early Curricula of 1962 and 1969 lack the goal that students should learn to ask questions, formulate hypotheses or participate in the planning of investigations. That students should learn to formulate questions is first described in the 1980 Curriculum" (p. 205). Similar trends have been observed in other areas of study.[40] These trends seem consistent with a greater emphasis on reasoning as compared to knowledge,[41] though we note that, in our survey, parents' perceptions of their own primary schooling experience do not reflect such a trend (see Panel B of Figure 2.6).

Figure 2.7 presents an original quantitative analysis of trends in emphasis in the Curricula. Based on a close reading of the Curricula we selected a set of keywords related to reasoning and knowledge. For each cohort, we calculate the relative frequency of keywords related to reasoning vs. knowledge during the cohort's primary school years. The figure shows a trend across cohorts toward greater emphasis on reasoning relative to knowledge. Appendix Figure 2.Q lists the set of keywords we study and provides more details on data construction.

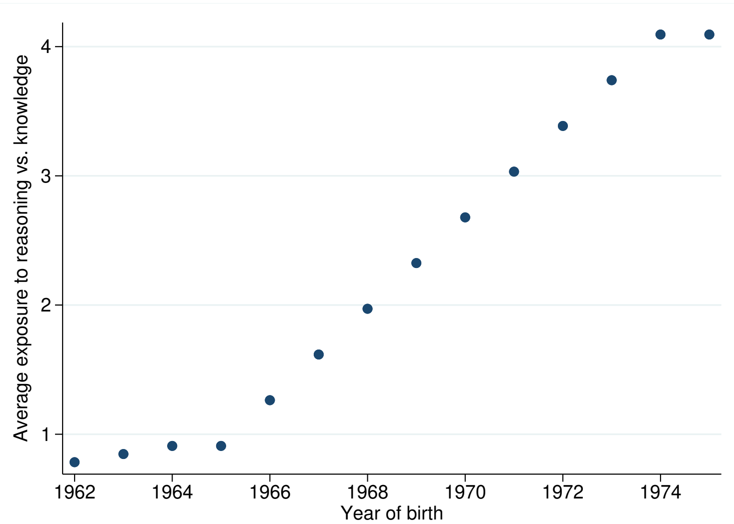
[38] For example, the first paragraph of the first section of the 1962 Curriculum states a goal of helping students develop into "capable and responsible members of society" (Skolöverstyrelsen, 1962, p. 13). The 1980 Curriculum repeats this language, quoting it as part of the Education Act (Skolöverstyrelsen, 1980, p. 13).

[39] For example, the 1962 Curriculum partly reflected the findings from systematic interviews of supervisors and employees regarding the knowledge demands of the workplace (Thavenius 1999, p. 43; Statens offentliga utredningar 1960, pp. 500-508).

[40] Löfdahl (1987) studies the physics Curriculum but also describes a more general evolution from 1962 to 1980 towards more emphasis on creativity and critical thinking (see also Johansson & Wickman, 2012, p. 199). Prytz (2015, p. 317) studies the mathematics Curriculum and notes a trend since the 1960s towards less emphasis on performing calculations. Dahlbäck and Lyngfelt (2017, pp. 167-168) study the evolution of the Curriculum and note that, compared to the 1969 Curriculum, the 1980 Curriculum places greater emphasis on the creative use of language.

[41] Larsson (2011) situates these trends in a transition from realism to progressivism in education. Trends toward greater emphasis on critical thinking and less emphasis on rote knowledge have been noted in many contexts, not only Sweden (see, e.g., Darling-Hammond et al., 2020). Bietenbeck (2014) finds using test score data from the US that modern teaching practices promote reasoning skills whereas traditional teaching practices promote factual knowledge.

Figure 2.7: Trends in Emphasis on Reasoning vs. Knowledge in Swedish Primary School Curricula



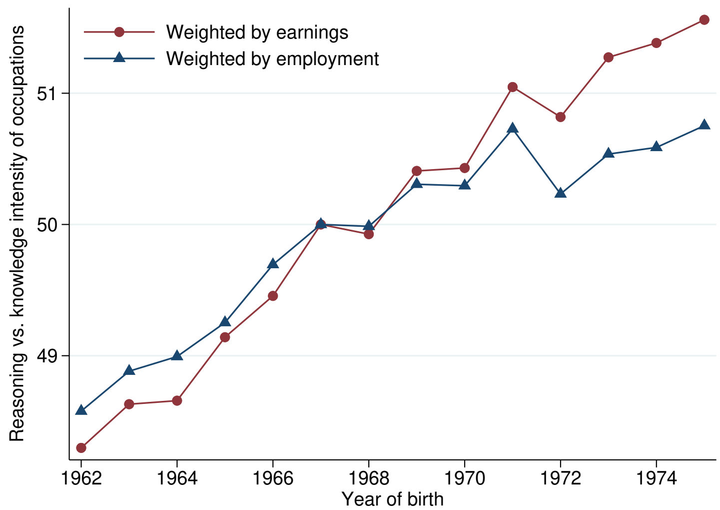
Notes: The plot shows the trend across birth cohorts in the emphasis on reasoning relative to knowledge in the Swedish primary school Curricula (Läroplan for grundskolan) prevailing during each cohort's primary schooling. We construct the series as follows. First, we associate each school year from 1963 through 1991 with the prevailing Curriculum, treating the 1962 Curriculum (Skolöverstyrelsen, 1962) as prevailing from 1963 through 1971, the 1969 Curriculum (Skolöverstyrelsen, 1969) as prevailing from 1972 through 1981, and the 1980 Curriculum (Skolöverstyrelsen, 1980) as prevailing from 1982 through 1991. Second, for each Curriculum we obtain the ratio of the number of appearances of keywords related to reasoning to the number of appearances of keywords related to knowledge. We choose these keywords based on a close reading of the Curricula; see Appendix Figure 2.Q for details. Third, for each cohort, we define the average exposure to reasoning vs. knowledge as the average of the ratio of keyword appearances over the cohort's primary school years, which we take to be the school years beginning in the fall of the year that members of the cohort turn age 7 and ending in the spring of the year that members of the cohort turn age 16.

2.5.3 Occupations

Figure 2.8 shows trends across cohorts in the average reasoning vs. knowledge intensity of occupations. We construct the series as follows. First, we measure the relative reasoning vs. knowledge intensity of occupations in Sweden by matching occupations to those in the US and taking data on the importance of different abilities and knowledge from the O*NET 25.0 database (US Department of Labor, Employment and Training Administration, 2020). Second, we compute for each occupation the percentile rank in the distribution of reasoning vs. knowledge intensity of occupations for the 1967 cohort. Finally, we take the weighted average across occupations within each cohort using as weights either total employment or total earnings among the men in the enlistment sample.

Figure 2.8 shows evidence of a trend towards relatively more reasoning-intensive occupations. The average man born in 1975 is employed in an occupation that is 2.2 percentile points more reasoning-intensive (relative to knowledge-intensive) than the average man born in 1962. The average krona earned by a man born in 1975 is earned by a man in an occupation 3.3 percentile points more reasoning-intensive than the average krona earned by a man born in 1962. Appendix Figure 2.R shows trends in shares of total employment and total earnings separately for each occupation.

Figure 2.8: Trends in the Reasoning vs. Knowledge Intensity of Men's Occupations in Sweden



Notes: The plot shows the trend across birth cohorts in the reasoning vs. knowledge intensity of occupations in the Swedish Occupational Register, measured as the mean percentile rank of the reasoning vs. knowledge intensity of the given cohort's occupations in the distribution of either total employment ("weighted by employment") or total earnings ("weighted by earnings") for the cohort 1967. We measure the distribution of employment and earnings across occupations in the Swedish Occupational Register using data on employment histories from 2004 onwards from Statistics Sweden (2021), using 4-digit Swedish Standard Classification of Occupations 96 (SSYK 96) codes, and taking each individual's occupation to be the one observed in the available year closest to the year the individual turns 40. For each US Department of Labor, Employment and Training Administration (2020) occupation we define the total importance of reasoning abilities by summing the importance scores of Inductive, Deductive, and Mathematical Reasoning abilities and dividing by the highest possible sum. Similarly, we define the total importance of knowledge by summing the importance scores of all knowledge categories and dividing by the highest possible sum. We then define the reasoning vs. knowledge intensity of each US Department of Labor, Employment and Training Administration (2020) occupation by taking the log of the ratio of the total importance of reasoning abilities to the total importance of knowledge. We define the reasoning vs. knowledge intensity of each Standard Occupational Classification 2010 (SOC 2010) occupation by taking the unweighted average reasoning vs. knowledge intensity of all corresponding occupations in US Department of Labor, Employment and Training Administration (2020). We match the occupations in the Swedish Occupational Register to SOC 2010 occupations by using the crosswalks from Statistics Sweden (2016c) and BLS (2015), manually excluding some matches to improve accuracy. We define the reasoning vs. knowledge intensity of each occupation in the Swedish Occupational Register by taking the employment-weighted mean reasoning vs. knowledge intensity of all corresponding SOC 2010 occupations, using May 2018 OES employment estimates (BLS 2019) as weights. Each series is normalized by adding a constant so that its value for the 1967 cohort is 50. This figure includes information from the O*NET 25.0 Database by the US Department of Labor, Employment and Training Administration (USDOL/ETA). Used under the CC BY 4.0 license. O*NET® is a trademark of USDOL/ETA. We have modified all or some of this information. USDOL/ETA has not approved, endorsed, or tested these modifications.

It is important to caveat that the concepts of reasoning and knowledge we measure here do not correspond exactly to those measured by the enlistment tests we study, that the join between Swedish and US occupation codes is imperfect, and that the O*NET scores are static, so the scores do not reflect changes over time in the demands of different occupations. Still, we find the pattern in Figure 2.8 interesting in light of the growth in the relative premium to fluid intelligence that we document in Section 2.4.

2.6 Non-cognitive Skills

There is evidence of rising labor-market returns to non-cognitive skill (e.g., Deming, 2017; Edin et al., 2022). We can extend our analysis to incorporate non-cognitive skills. Suppose that dimensions $j \in \{1, ..., L\}$, for $2 \leq L < J$ are dimensions of cognitive skill, and the remaining dimensions $j \in \{L+1, ..., J\}$ are dimensions of non-cognitive skill. Suppose further that

$$S_c(\tilde{\mathbf{x}}) = s_c\left(\left(\sum_{j=1}^{L} K_{cj}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{1}{\rho}}, \tilde{\mathbf{x}}_{L+1:J}\right) \tag{2.6}$$

where $\tilde{\mathbf{x}}_{L+1:J} = (\tilde{x}_{L+1}, ..., \tilde{x}_J)$ is the non-cognitive skill investment and $s_c(\cdot)$ is an aggregator strictly increasing in its first argument.[42] We suppose conditions on $s_c(\cdot)$ sufficient to ensure a unique, strictly positive solution $\tilde{\mathbf{x}}_c(\mathbf{P}_c)$ to the worker's skill investment problem for any $\mathbf{P}_c > 0$. We define a cognitive skill supply function $\tilde{\mathbf{x}}_{c,1:L}(\cdot; \mathbf{x}_{L+1:J})$ that describes the optimal level of cognitive skill investment $\tilde{\mathbf{x}}_{c,1:L} = (\tilde{x}_{c,1}, ..., \tilde{x}_{c,L})$ for workers in cohort c given any lifetime skill premia $\mathbf{P}_c > 0$ and any level $\mathbf{x}_{L+1:J}$ of non-cognitive skill investment.

For each worker i we observe

$$\hat{\mathbf{x}}_i = \left(\mathbf{x}_{i,1:L}, \mathbf{A}_{c(i)}\left(\mathbf{x}_{i,L+1:J}\right)\right)$$

where $\mathbf{A}_c(\cdot)$ is a cohort-specific, possibly unknown affine map. The presence of the map $\mathbf{A}_c(\cdot)$ reflects the fact that, in our data, the measure of non-cognitive skill is standardized and thus not directly comparable across cohorts.[43]

Analogous to our baseline analysis, from data on each cohort's cognitive skill premia $\mathbf{P}_{c,1:L}$ and mean observed skill levels $\hat{\mathbf{x}}_c$, it is possible to identify the cognitive skill supply function $\tilde{\mathbf{x}}_{c,1:L}(\cdot; \tilde{\mathbf{x}}_{c,L+1:J})$ where non-cognitive skill $\tilde{\mathbf{x}}_{c,L+1:J} = \tilde{\mathbf{x}}_{c,L+1:J}(\mathbf{P}_c)$ is fixed at its equilibrium value for each cohort.

Proposition 7. *Under Assumption 1, if* $\frac{P_{\bar{c}1}}{P_{\bar{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$*, then the cognitive skill supply function* $\tilde{\mathbf{x}}_{c,1:L}(\cdot; \tilde{\mathbf{x}}_{c,L+1:J})$ *for each cohort* c *is identified from data* $\{(\alpha\mathbf{P}_{c,1:L}, \hat{\mathbf{x}}_c)\}_{c=\underline{c}}^{\bar{c}}$*, where the scalar* $\alpha > 0$ *may be unknown.*

[42] An example is the two-level constant elasticity function (e.g., Sato 1967; Goldin and Katz 2008, Chapter 8, equations 1 and 2):

$$s_c\left(\left(\sum_{j=1}^{L} K_{cj}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{1}{\rho}}, \tilde{\mathbf{x}}_{L+1:J}\right) = \left(\lambda\left(\sum_{j=1}^{L} K_{cj}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{\sigma}{\rho}} + (1-\lambda)\left(\sum_{j=L+1}^{J} K_{cj}^{\nu-1}\tilde{x}_j^{\nu}\right)^{\frac{\sigma}{\nu}}\right)^{\frac{1}{\sigma}}$$

where ν, σ, and λ are parameters.

[43] Edin et al. (2022, Appendix A1.2) discuss the implications of standardization for the estimation of returns to non-cognitive skill.

Our assumptions are also sufficient to identify the lifetime cognitive skill premia up to scale.

Proposition 8. *Under Assumption 2, for some scalar $\alpha > 0$, a multiple $\alpha \mathbf{P}_{c,1:L}$ of the lifetime cognitive skill premia for each cohort c is identified from the conditional expectation function of the log of earnings,*

$$\mathrm{E}\left(\ln\left(w_{it}\right)|\hat{\mathbf{x}}_i = \hat{\mathbf{x}},\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right),$$

for each time period $t \in \{c+1, ..., c+A\}$.

Notice that our assumptions are not generally sufficient to identify the lifetime non-cognitive skill premia $\mathbf{P}_{c,L+1:J}$ up to scale due to the presence of the map $\mathbf{A}_c(\cdot)$.

Following the logic of Propositions 7 and 8 and their proofs, we estimate the cognitive skill supply function as follows. First, we re-estimate lifetime skill premia $\mathbf{P}_c$ following the procedure outlined in Section 2.4.1, but including the standardized measure of non-cognitive skill as an additional covariate in each earnings regression. Second, we estimate the cognitive skill supply function $\tilde{\mathbf{x}}_{c,1:L}\left(\cdot;\tilde{\mathbf{x}}_{c,L+1:J}\right)$ following the steps we used to estimate the skill supply function in Section 2.4.2, but using the re-estimated lifetime skill premia.

Panel B of Table 2.1 presents our estimates. The estimated cognitive skill supply function implies that, fixing the level of non-cognitive skill at its equilibrium level, changes in labor market returns account for 26.2 percent of the increase in logical skill (with a standard error of 2.1 percent), and for more than the entire decline in vocabulary knowledge skill. The estimated role of changing labor market returns reported in Panel B is meaningfully smaller than in our baseline analysis reported in Panel A, as is the estimated elasticity of substitution.

2.7 Conclusions

We develop a quantitative economic model of the evolution of multidimensional skills across cohorts. We estimate the model using administrative data from Sweden. The estimated model implies that a significant portion of the puzzling "Flynn effect" of rising fluid intelligence is due to substitution in investment across different dimensions of skill. The model also explains the decline in crystallized intelligence across cohorts in our setting. The model is consistent with evidence of a trend towards greater emphasis on reasoning relative to knowledge among parents, schools, and occupations. We extend our analysis to incorporate non-cognitive skill. We conclude that it is fruitful to incorporate market-driven incentives into the analysis of cohort trends in measured intelligence.

We treat the labor demand side of our model abstractly and do not offer a detailed account of the causes of cohort trends in measured labor market returns to skill. Our analysis does, however, suggest some possible explanations for trends in labor market returns to skill. We estimate an increase in the overall supply of skill across cohorts. All else equal, an increase in the supply of skill would tend to lower its return, consistent with our finding of declining returns to cognitive skill across cohorts. Likewise, our finding of an increase in the relative return to reasoning, as compared to knowledge, seems consistent with the trends in occupational composition that we document. We think that developing a more detailed model of skill demand that can be combined with our model of skill supply to explain cohort trends in returns to skill is an interesting direction for future work.

APPENDIX

2.A Proofs

Proof of Proposition 5

From Assumption 1 and equation (2.5) we have that

$$\frac{1}{\rho - 1} = \frac{\ln\left(\frac{\tilde{x}_{\bar{c}1}}{\tilde{x}_{\bar{c}2}}\right) - \ln\left(\frac{\tilde{x}_{\underline{c}1}}{\tilde{x}_{\underline{c}2}}\right)}{\ln\left(\frac{P_{\bar{c}1}}{P_{\bar{c}2}}\right) - \ln\left(\frac{P_{\underline{c}1}}{P_{\underline{c}2}}\right)} \tag{2.7}$$

where the existence of the ratio on the right is guaranteed because $\frac{P_{\bar{c}1}}{P_{\bar{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$. Thus ρ is identified.

Because $\mathbf{P}_c > 0$, an analogue of equation (2.5) holds for any pair of dimensions $(1, j)$. Thus given ρ the ratio $\frac{K_{cj}}{K_{c1}}$ is identified for all c and j via the relation

$$\ln\left(\frac{K_{cj}}{K_{c1}}\right) = \ln\left(\frac{\tilde{x}_{c1}}{\tilde{x}_{cj}}\right) - \frac{1}{\rho - 1}\ln\left(\frac{P_{c1}}{P_{cj}}\right). \tag{2.8}$$

From the budget constraint in (2.2) and the transformation function in (2.4), observe that multiplying $\mathbf{K}_c$ by any positive constant κ is equivalent to multiplying $\overline{S}_c$ by $\kappa^{\frac{1-\rho}{\rho}}$. Therefore fix the scale of $\mathbf{K}_c$ by supposing that its average element equals one, i.e., $\sum_{j=1}^{J} K_{cj} = J$. Then $\sum_{j=1}^{J} K_{cj} = \sum_{j=1}^{J}\left(\frac{K_{cj}}{K_{c1}}\right) K_{c1} =$

$K_{c1}\sum_{j=1}^{J}\left(\frac{K_{cj}}{K_{c1}}\right)=J$, which from (2.8) implies

$$K_{c1}=\frac{J}{\sum_{j=1}^{J}\frac{\tilde{x}_{c1}}{\tilde{x}_{cj}}\left(\frac{P_{cj}}{P_{c1}}\right)^{\frac{1}{\rho-1}}}. \tag{2.9}$$

Thus $\mathbf{K}_c$ is identified for each cohort c given ρ and the ratios $\frac{K_{cj}}{K_{c1}}$.

Finally, $\overline{S}_c$ is identified for all c given ρ and $\mathbf{K}_c$ because, from the solution to the worker's problem,

$$\overline{S}_c=\frac{\tilde{x}_{c1}\left(\sum_{j=1}^{J}P_{cj}^{\frac{\rho}{\rho-1}}K_{cj}^{-1}\right)^{\frac{1}{\rho}}}{P_{c1}^{\frac{1}{\rho-1}}K_{c1}^{-1}}. \tag{2.10}$$

Proof of Corollary 1

Let $\hat{\mathbf{P}}_c=|\alpha\mathbf{P}_c|=|\alpha|\,\mathbf{P}_c$ for $\alpha\neq 0$. Because $\frac{\hat{P}_{c1}}{\hat{P}_{cj}}=\frac{P_{c1}}{P_{cj}}$ for all c and j, the arguments in the proof of Proposition 5 directly establish identification of ρ and identification of $\mathbf{K}_c$ up to a normalization. Then $\overline{S}_c$ is identified for all c given ρ and $\mathbf{K}_c$ because

$$\overline{S}_c=\frac{\tilde{x}_{c1}\left(\sum_{j=1}^{J}P_{cj}^{\frac{\rho}{\rho-1}}K_{cj}^{-1}\right)^{\frac{1}{\rho}}}{P_{c1}^{\frac{1}{\rho-1}}K_{c1}^{-1}}=\frac{\tilde{x}_{c1}\left(\sum_{j=1}^{J}\hat{P}_{cj}^{\frac{\rho}{\rho-1}}K_{cj}^{-1}\right)^{\frac{1}{\rho}}}{\hat{P}_{c1}^{\frac{1}{\rho-1}}K_{c1}^{-1}}.$$

Proof of Proposition 6

From equation (2.1) we have that for each period t

$$\begin{aligned}\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i=\mathbf{x},\mathbf{d}_{it}=\mathbf{d},c\left(i\right)=c\right)&=\mathrm{E}\left(B_{t,a(i,t)}+\mathbf{p}_{t,a(i,t)}^{'}\mathbf{x}_i+\ln\left(z_{it}\right)|\mathbf{x}_i=\mathbf{x},\mathbf{d}_{it}=\mathbf{d},c\left(i\right)=c\right)\\&=B_{t,t-c}+\mathbf{p}_{t,t-c}^{'}\mathbf{x}+\mathrm{E}\left(\ln\left(z_{it}\right)|\mathbf{x}_i=\mathbf{x},\mathbf{d}_{it}=\mathbf{d},c\left(i\right)=c\right).\end{aligned}$$

Because $\mathbf{x}_i=\tilde{\mathbf{x}}_{c(i)}+\mu_i$ for all i, we also have that

$$\begin{aligned}\mathrm{E}\left(\ln\left(z_{it}\right)|\mathbf{x}_i=\mathbf{x},\mathbf{d}_{it}=\mathbf{d},c\left(i\right)=c\right)&=\mathrm{E}\left(\ln\left(z_{it}\right)|\tilde{\mathbf{x}}_c+\mu_i=\mathbf{x},\mathbf{d}_{it}=\mathbf{d},c\left(i\right)=c\right)\\&=\mathrm{E}\left(\ln\left(z_{it}\right)|\mu_i=\mathbf{x}-\tilde{\mathbf{x}}_c,\mathbf{d}_{it}=\mathbf{d},c\left(i\right)=c\right)\\&=\zeta_{t,t-c}+\tilde{\alpha}\mathbf{p}_{t,t-c}^{'}\left(\mathbf{x}-\tilde{\mathbf{x}}_c\right)+\mathbf{d}^{'}\beta_{t,t-c}\end{aligned}$$

where the last equality uses Assumption 2. It follows that

$$\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) = \tilde{B}_{t,t-c} + \alpha\mathbf{p}'_{t,t-c}\mathbf{x} + \mathbf{d}'\beta_{t,t-c}$$

where $\tilde{B}_{t,t-c} = \left(B_{t,t-c} + \zeta_{t,t-c} - \tilde{\alpha}\mathbf{p}'_{t,t-c}\tilde{\mathbf{x}}_c\right)$ and $\alpha = 1 + \tilde{\alpha}$. Since $\tilde{\alpha} \neq -1$, we have $\alpha \neq 0$. Identification of $\mathbf{p}_{t,t-c}$ up to scale is then immediate, from which identification of $\mathbf{P}_c$ up to scale follows directly from equation (2.3).

Proof of Proposition 7

Recall that the worker maximizes $\mathbf{P}'_{c(i)}\tilde{\mathbf{x}}_i$ subject to $\tilde{\mathbf{x}}_i \geq 0$ and $S_{c(i)}\left(\tilde{\mathbf{x}}_i\right) \leq \overline{S}_{c(i)}$, where $\tilde{\mathbf{x}}_i = \mathbf{x}_i - \mu_i$. Fixing non-cognitive skill investment at $\tilde{\mathbf{x}}_{i,L+1:J} = \tilde{\mathbf{x}}_{c(i),L+1:J} \geq 0$, and taking account of the form of the transformation function in (2.6), we can rewrite the worker's problem as maximizing $\mathbf{P}'_{c(i),1:L}\tilde{\mathbf{x}}_{i,1:L}$ subject to $\tilde{\mathbf{x}}_{i,1:L} \geq 0$ and $\left(\sum_{j=1}^{L} K^{\rho-1}_{c(i)j}\tilde{x}^{\rho}_j\right)^{\frac{1}{\rho}} \leq s^{-1}_{c(i)}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right)$, where $s^{-1}_{c(i)}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right)$ solves $s_{c(i)}\left(s^{-1}_{c(i)}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right), \tilde{\mathbf{x}}_{c(i),L+1:J}\right) = \overline{S}_{c(i)}$, is unique by the strict monotonicity of $s_c\left(\cdot\right)$ in its first argument, and is strictly positive because the worker's problem is assumed to have a strictly positive solution. We have demonstrated that the worker's problem of choosing cognitive skills given non-cognitive skills is equivalent to the worker's problem in Section 2.2.3, replacing J with L and $\overline{S}_{c(i)}$ with $s^{-1}_{c(i)}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right)$. The results of Proposition 5 and Corollary 1 thus apply given Assumption 1.

Proof of Proposition 8

We have that

$$\begin{aligned}
\mathrm{E}\left(\ln\left(w_{it}\right)|\hat{\mathbf{x}}_i = \hat{\mathbf{x}},\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right) &= \mathrm{E}\left(\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right)|\hat{\mathbf{x}}_i = \hat{\mathbf{x}}\right) \\
&= \mathrm{E}\left(\tilde{B}_{t,t-c} + \alpha\mathbf{p}'_{t,t-c}\mathbf{x} + \mathbf{d}'\beta_{t,t-c}|\hat{\mathbf{x}}_i = \hat{\mathbf{x}}\right) \\
&= \tilde{B}_{t,t-c} + \alpha\mathbf{p}'_{t,t-c,1:L}\mathbf{x}_{1:L} + \alpha\mathbf{p}'_{t,t-c,L+1:J}\mathbf{A}^{-1}_c\left(\hat{\mathbf{x}}_{L+1:J}\right) + \mathbf{d}'\beta_{t,t-c}
\end{aligned}$$

where the first step follows from the law of total expectation, the second from the proof of Proposition 6, and the third from the invertibility of $\mathbf{A}_c$. Because $\mathbf{A}^{-1}_c\left(\cdot\right)$ is linear in $\hat{\mathbf{x}}_{L+1:J}$, identification of $\mathbf{p}'_{t,t-c,1:L}$ up to scale is immediate, from which identification of $\mathbf{P}_{c,1:L}$ up to scale follows directly from equation (2.3).

2.B Identification of the Skill Supply Function with a Social Multiplier

Suppose that $K_{cj} = \overline{K}_{cj}\overline{x}_{cj}^{-\upsilon}$ where $\upsilon \in [0,1)$ is a parameter governing the strength of social spillovers in skill investment and $\overline{\mathbf{K}}_c \in \mathbb{R}^J_{>0}$ is a vector of cost parameters. Each worker chooses skill investment taking the average skill $\overline{\mathbf{x}}_{c(i)}$ of their cohort $c(i)$ as given.

Assumption 3. *(Zero average relative supply shock.) We assume that*

$$\frac{1}{\overline{c}-\underline{c}}\sum_{c=\underline{c}}^{\overline{c}-1}\left[\ln\left(\frac{\overline{K}_{c+1,1}}{\overline{K}_{c+1,2}}\right)-\ln\left(\frac{\overline{K}_{c1}}{\overline{K}_{c2}}\right)\right]=0.$$

Proposition 9. *Under Online Appendix Assumption 3, if $\frac{P_{\overline{c}1}}{P_{\overline{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$, then the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$ for each cohort c is identified from data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$.*

Proof of Online Appendix Proposition 9

In the model in Section 2.2.3 the skill supply function is given by

$$\tilde{x}_{cj}(\mathbf{P}_c) = \frac{P_{cj}^{\frac{1}{\rho-1}}K_{cj}^{-1}}{\left(\sum_{j'=1}^{J}P_{cj'}^{\frac{\rho}{\rho-1}}K_{cj'}^{-1}\right)^{\frac{1}{\rho}}}\overline{S}_c \tag{2.11}$$

for each skill $j \in \{1,...,J\}$. Recalling that $K_{cj} = \overline{K}_{cj}\overline{x}_{cj}^{-\upsilon}$ and imposing the equilibrium condition that $\tilde{\mathbf{x}}_c = \overline{\mathbf{x}}_c$ we have that

$$\tilde{x}_{cj}(\mathbf{P}_c) = \frac{P_{cj}^{\frac{1}{\rho-1}}\overline{K}_{cj}^{-1}\left(\tilde{x}_{cj}(\mathbf{P}_c)\right)^{\upsilon}}{\left(\sum_{j'=1}^{J}P_{cj'}^{\frac{\rho}{\rho-1}}\overline{K}_{cj'}^{-1}\left(\tilde{x}_{cj'}(\mathbf{P}_c)\right)^{\upsilon}\right)^{\frac{1}{\rho}}}\overline{S}_c \tag{2.12}$$

for each skill $j \in \{1,...,J\}$. Define $\tilde{\mathbf{K}}_c$ such that $\tilde{K}_{cj} = \overline{K}_{cj}^{\frac{1}{1-\upsilon}}$ and notice that $\tilde{\mathbf{K}}_c \in \mathbb{R}^J_{>0}$ and that

$$\frac{1}{\overline{c}-\underline{c}}\sum_{c=\underline{c}}^{\overline{c}-1}\left[\ln\left(\frac{\tilde{K}_{c+1,1}}{\tilde{K}_{c+1,2}}\right)-\ln\left(\frac{\tilde{K}_{c1}}{\tilde{K}_{c2}}\right)\right]=0$$

by Online Appendix Assumption 3. Define $\tilde{\rho}$ such that

$$\frac{1}{\tilde{\rho}-1} = \frac{1}{(\rho-1)(1-\upsilon)}$$

and notice that $\tilde{\rho} > 1$. Define $\tilde{S}_c = \overline{S}_c^{\frac{\rho}{\tilde{\rho}}}$ and notice that $\tilde{S}_c > 0$. Then the unique solutions to the J equations in (2.12) are given by

$$\tilde{x}_{cj}\left(\mathbf{P}_c\right) = \frac{P_{cj}^{\frac{1}{\tilde{\rho}-1}} \tilde{K}_{cj}^{-1}}{\left(\sum_{j'=1}^{J} P_{cj'}^{\frac{\tilde{\rho}}{\tilde{\rho}-1}} \tilde{K}_{cj'}^{-1}\right)^{\frac{1}{\tilde{\rho}}}} \tilde{S}_c \tag{2.13}$$

for each skill $j \in \{1, ..., J\}$. Because (2.13) is isomorphic to (2.11), replacing $\mathbf{K}_c$ with $\tilde{\mathbf{K}}_c$, ρ with $\tilde{\rho}$, and $\overline{S}_c$ with $\tilde{S}_c$, and because an analogue of Assumption 1 in the main text holds for $\tilde{\mathbf{K}}_c$, Proposition 5 in the main text applies directly.

2.C Identification of Lifetime Skill Premia with Mismeasured Skills

Let $\hat{\mathbf{x}}_i$ denote a measurement of $\mathbf{x}_i$. For simplicity we set aside the role of covariates $\mathbf{d}_{it}$.

Assumption 4. *The measurement error in each cohort c obeys*

$$\mathrm{E}\left(\hat{\mathbf{x}}_i - \mathbf{x}_i | \mu_i = \mu, c\left(i\right) = c\right) = 0 \tag{2.14}$$

and

$$\mathrm{Var}\left(\hat{\mathbf{x}}_i - \mathbf{x}_i | c\left(i\right) = c\right) = \hat{\alpha}\, \mathrm{Var}\left(\hat{\mathbf{x}}_i | c\left(i\right) = c\right), \tag{2.15}$$

where the scalar $\hat{\alpha} \in [0, 1)$ may be unknown.

Appendix Assumption 4 implies that the measurement error in $\hat{\mathbf{x}}_i$ has mean zero conditional on true skills and has variance proportional to both measured and true skills.

Assumption 5. *The values of z_{it} in each period t obey*

$$\mathrm{E}\left(\ln\left(z_{it}\right) | \hat{\mathbf{x}}_i - \mathbf{x}_i = \xi, \mu_i = \mu, c\left(i\right) = c\right) = \mathrm{E}\left(\ln\left(z_{it}\right) | \mu_i = \mu, c\left(i\right) = c\right) = \zeta_{t,t-c} + \tilde{\alpha} \mathbf{p}_{t,t-c}^{'} \mu, \tag{2.16}$$

where the scalars $\zeta_{t,t-c}$ and $\tilde{\alpha} \geq 0$ may be unknown.

Appendix Assumption 5 implies that a version of Assumption 2 in the main text holds, and that unobserved determinants of log earnings are mean-independent of the measurement error in skills.

Appendix Assumptions 4 and 5 are sufficient to identify the cohort-and-period-specific skill premia $\mathbf{p}_{t,t-c}$, and hence the lifetime skill premia $\mathbf{P}_c$, up to scale, from the conditional expectation function of the log of earnings given measured skills.

Proposition 10. *Under Appendix Assumptions 4 and 5, for some scalar $\alpha > 0$, a multiple $\alpha \mathbf{P}_c$ of the lifetime skill premia for each cohort c is identified from the conditional expectation function of the log of earnings given measured skills,*

$$\mathrm{E}\left(\ln\left(w_{it}\right)|\hat{\mathbf{x}}_i = \hat{\mathbf{x}}, c\left(i\right) = c\right),$$

for each time period $t \in \{c+1, ..., c+A\}$.

Proof of Appendix Proposition 10

Fix a cohort c and period t. From (2.14) and (2.15) we have that

$$\mathrm{Var}\left(\hat{\mathbf{x}}_i|c\left(i\right) = c\right) = \left(1-\hat{\alpha}\right)^{-1}\mathrm{Var}\left(\mathbf{x}_i|c\left(i\right) = c\right).$$

From (2.1) in the main text, (2.14), and (2.16) we have that

$$\begin{aligned}
\mathrm{Cov}\left(\hat{\mathbf{x}}_i, \ln\left(w_{it}\right)|c\left(i\right) = c\right) &= \mathrm{Cov}\left(\hat{\mathbf{x}}_i, \mathbf{p}_{t,t-c}^{'}\mathbf{x}_i + \ln\left(z_{it}\right)|c\left(i\right) = c\right) \\
&= \mathrm{Cov}\left(\mathbf{x}_i, \left(1+\tilde{\alpha}\right)\mathbf{p}_{t,t-c}^{'}\mathbf{x}_i|c\left(i\right) = c\right) \\
&= \left(1+\tilde{\alpha}\right)\mathrm{Var}\left(\mathbf{x}_i|c\left(i\right) = c\right)\mathbf{p}_{t,t-c}.
\end{aligned}$$

The population regression of $\ln\left(w_{it}\right)$ on $\hat{\mathbf{x}}_i$ and a constant therefore yields coefficients

$$\mathrm{Var}\left(\hat{\mathbf{x}}_i|c\left(i\right) = c\right)^{-1}\mathrm{Cov}\left(\hat{\mathbf{x}}_i, \ln\left(w_{it}\right)|c\left(i\right) = c\right) = \alpha\mathbf{p}_{t,t-c}$$

for $\alpha = \left(1-\hat{\alpha}\right)\left(1+\tilde{\alpha}\right) > 0$. Because the population regression is available from the conditional expectation function, identification of $\mathbf{p}_{t,t-c}$ up to scale is then immediate, from which identification of $\mathbf{P}_c$ up to scale follows directly from equation (2.3) in the main text.

2.D Additional Tables and Figures

Table 2.A: Number of individuals by birth cohort, military enlistment and survey samples

(a) *Military Enlistment Data*

Birth cohort	Number of individuals
1962	52,317
1963	55,526
1964	58,639
1965	55,018
1966	39,056
1967	47,767
1968	49,965
1969	48,850
1970	48,815
1971	51,108
1972	50,824
1973	47,353
1974	47,923
1975	38,069
Total	691,230

(b) *Survey Data*

Birth cohort	Number of individuals
1948	5,361
1953	4,699
1967	3,907
1972	3,899
1977	1,966
Total	19,832

Notes: Each panel shows the number of individuals in each birth cohort for whom we measure valid logical reasoning and vocabulary knowledge test scores. Panel (a) shows counts for the military enlistment data. Panel (b) shows counts for the survey data.

Table 2.B: Sensitivity of main results to different specifications

	Initial lifetime skill premium, 1962 $\hat{P}_{\underline{c}j}$		**Change in lifetime skill premium** $\hat{P}_{\overline{c}j} - \hat{P}_{\underline{c}j}$		**Initial average skill level** $\bar{x}_{\underline{c}j}$		**Change in average skill level** $\bar{x}_{\overline{c}j} - \bar{x}_{\underline{c}j}$		**Share of change explained by change in skill premia** $1 - \frac{\tilde{x}_{\overline{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\overline{x}_{\overline{c}j} - \underline{x}_{\underline{c}j}}$	
Specification	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary
(a) Baseline	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3681	2.0151
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(b) Exclude birth cohort 1962	0.0047	0.0016	-0.0007	-0.0008	48.53	50.79	4.09	-3.24	0.3940	1.8008
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.12)	(0.13)	(0.21)	(0.25)	(0.0250)	(0.1294)
(c) Exclude birth cohort 1975	0.0048	0.0016	-0.0008	-0.0006	47.88	50.72	4.95	-1.84	0.3153	2.9520
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.23)	(0.19)	(0.0114)	(0.3006)
(d) Replace percentile rank with percent of maximum score attained	0.0074	0.0025	-0.0009	-0.0009	59.96	61.07	3.30	-1.04	0.2936	3.2777
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.09)	(0.07)	(0.14)	(0.11)	(0.0122)	(0.3969)
(e) Replace logical reasoning skill with logical-spatial composite	0.0050	0.0020	-0.0010	-0.0006	46.42	50.72	5.10	-2.92	0.4545	2.0408
	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.13)	(0.13)	(0.21)	(0.21)	(0.0174)	(0.1486)
(f) Include business income in earnings measure	0.0049	0.0016	-0.0009	-0.0007	47.88	50.72	4.43	-2.92	0.3587	2.0302
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0168)	(0.1486)
(g) Age range 35–55	0.0049	0.0019	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.4393	1.9008
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0196)	(0.1396)
(h) Age range 30–60	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3780	1.9992
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0177)	(0.1471)
(i) Restrict to modal full-year workers	0.0043	0.0016	-0.0006	-0.0006	49.07	51.61	3.74	-3.52	0.4653	1.5987
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.14)	(0.23)	(0.21)	(0.0258)	(0.0982)
(j) Extrapolate from ages 35+	0.0048	0.0016	-0.0008	-0.0008	47.88	50.72	4.43	-2.92	0.3591	2.0296
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0174)	(0.1497)
(k) Extrapolate from oldest available age	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3703	2.0115
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0179)	(0.1486)
(m) NPV with discount factor 0.93 skill premium series	0.0048	0.0015	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3396	2.0609
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0169)	(0.1519)
(m) NPV with discount factor 0.99 skill premium series	0.0048	0.0017	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3931	1.9750
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0182)	(0.1452)
(n) Quadratic smoothing for estimated skill premium series	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3680	2.0154
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(o) No smoothing for estimated skill premium series	0.0050	0.0013	-0.0010	-0.0006	47.88	50.72	4.43	-2.92	0.3085	2.1108
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.14)	(0.13)	(0.23)	(0.20)	(0.0308)	(0.1648)

Notes: This table summarizes the sensitivity of our main results to different specifications. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In each replicate, for each cohort c, we draw men with replacement from the population in that cohort, and recalculate all data-dependent objects. We exclude three and five bootstrap replicates from the calculation of standard errors for rows (e) and (o), respectively, due to values inconsistent with the model. Row (a) reproduces our baseline estimates from Panel A of Table 2.1. Row (b) changes the initial birth cohort $\underline{c}$ to be those born in 1963, and row (c) changes the final birth cohort $\overline{c}$ to be those born in 1974. Both rows (b) and (c) multiply estimated changes by 13/12 for comparability with the other specifications. Row (d) replaces the logical reasoning and vocabulary knowledge skill measures with the percent of the maximum possible score attained by the individual. Row (e) replaces the logical reasoning skill measure with the first component from a principal component analysis of logical reasoning and spatial reasoning skill measures for the 1967 birth cohort. Spatial reasoning skills are measured using a task in which individuals are asked to identify a three-dimensional object that corresponds to an unfolded piece of metal (Carlstedt, 2000; Carlstedt & Mårdberg, 1993). Row (f) incorporates business income into our measure of earnings for the years 1990–2018. Rows (g) and (h) vary the ages of working life that we consider for estimating the lifetime skill premia $\mathbf{P}_c$. Row (i) excludes individuals for whom the greatest mode of annual months worked in sample years is less than 12. We define an individual as being employed in a given month if that month falls between the first and last month of employment for at least one of the jobs he holds during the year. Rows (j) and (k) vary the ages over which we average the estimated premia $\mathbf{p}_{c+a,a}$ in order to infer premia for ages for which earnings data are unavailable. Rows (l) and (m) vary the discount factor δ that we use in the calculation of $\mathbf{P}_c$ via equation (2.3). Rows (n) and (o) use, respectively, a quadratic fit (second-order polynomial) and no smoothing at all, instead of a linear fit, for the relationship between estimated lifetime skill premia and cohort.

Table 2.C: Sensitivity of main results to adjusting for control variables

	Initial lifetime skill premium, 1962 $\hat{P}_{\underline{c}j}$		**Change in lifetime skill premium** $\hat{P}_{\bar{c}j} - \hat{P}_{\underline{c}j}$		**Initial average skill level** $\bar{x}_{\underline{c}j}$		**Change in average skill level** $\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}$		**Share of change explained by change in skill premia** $1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\bar{x}_{\bar{c}j} - \underline{x}_{\underline{c}j}}$	
Specification	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary
(a) Baseline (no controls)	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3681	2.0151
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(b) Age at enlistment (indicators)	0.0047	0.0016	-0.0007	-0.0007	48.21	51.03	4.43	-2.84	0.3596	2.0592
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.13)	(0.13)	(0.23)	(0.21)	(0.0179)	(0.1575)
(c) Completed secondary education	0.0048	0.0016	-0.0008	-0.0007	47.22	50.03	4.48	-2.86	0.3594	2.0612
at enlistment (indicator)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.23)	(0.21)	(0.0175)	(0.1577)
(d) Completed secondary education	0.0048	0.0016	-0.0008	-0.0007	47.10	49.87	4.15	-3.20	0.3860	1.8431
at age 18 (indicator)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.20)	(0.0193)	(0.1196)
(e) log(height) and log(weight)	0.0047	0.0015	-0.0008	-0.0006	47.94	50.79	4.47	-2.96	0.3554	2.0301
at enlistment	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0178)	(0.1489)
(f) Born outside Sweden (indicator)	0.0048	0.0016	-0.0008	-0.0007	47.90	50.72	4.30	-3.03	0.3756	1.9370
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.13)	(0.13)	(0.23)	(0.21)	(0.0189)	(0.1376)

Notes: This table summarizes the sensitivity of our main results to adjusting for different control variables. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In each replicate, for each cohort c, we draw men with replacement from the population in that cohort, and recalculate all data-dependent objects. Row (a) reproduces our baseline estimates with no controls from Panel A of Table 2.1. Each subsequent row includes a different control variable or variables. Control variables are included when estimating cohort- and age-specific skill premia and are used to adjust the estimated average skill levels, following Section 2.4.4. In each row, we omit individuals with missing or invalid values of the relevant control variables. In specification (b), we control for indicators for the number of years between the person's year of enlistment and year of birth. In specification (c), we define a person as having completed secondary education at enlistment if the person's enlistment date occurs on or after June 1 of the year in which they complete secondary education.

Table 2.D: Sensitivity of main results to adjusting for control variables

	Initial lifetime skill premium, 1962 $\hat{P}_{\underline{c}j}$		**Change in lifetime skill premium** $\hat{P}_{\bar{c}j} - \hat{P}_{\underline{c}j}$		**Initial average skill level** $\bar{x}_{\underline{c}j}$		**Change in average skill level** $\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}$		**Share of change explained by change in skill premia** $1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\overline{x}_{\bar{c}j} - \underline{x}_{\underline{c}j}}$	
Specification	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary	Logical	Vocabulary
(a) Baseline	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3681	2.0151
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(b) Below-median parental earnings	0.0044	0.0012	-0.0006	-0.0006	43.59	46.73	3.51	-3.39	0.3518	1.7200
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.17)	(0.21)	(0.29)	(0.29)	(0.0375)	(0.1457)
(c) Above-median parental earnings	0.0048	0.0017	-0.0010	-0.0008	52.43	54.96	4.98	-2.81	0.3646	2.1799
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.21)	(0.20)	(0.31)	(0.30)	(0.0232)	(0.2213)

Notes: This table summarizes the model implications for different subsamples. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In each replicate, for each cohort c, we draw men with replacement from the population in that cohort, and recalculate all data-dependent objects. We exclude one bootstrap replicate from the calculation of standard errors for row (b) due to values inconsistent with the model. Row (a) reproduces our baseline estimates from Panel A of Table 2.1. Rows (b) and (c) estimate the model for individuals with below- or above-median parental earnings, respectively, within their given cohort. For each sample individual, we define parental earnings as the average lifetime earnings of all biological or adoptive parents, with lifetime earnings given by the average earnings over all ages 30 through 55 observed in the data.

Table 2.E: Trends in lifetime skill premia using survey test scores as instruments

Panel A: Trends in lifetime skill premia

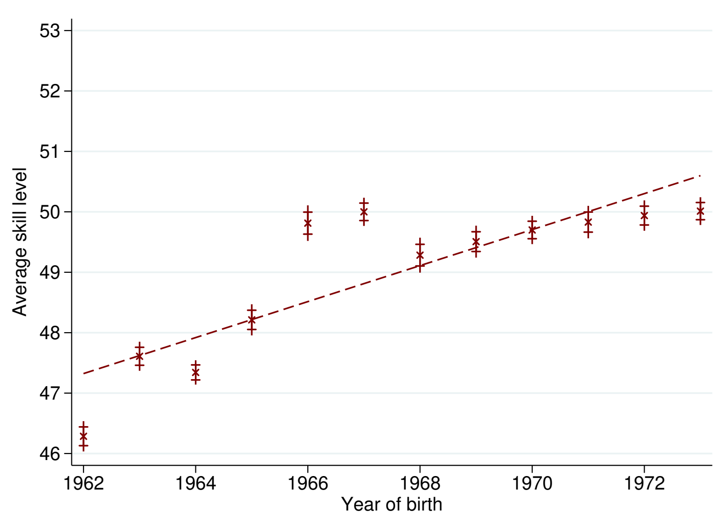
	Linear trend	OLS	IV
Change from 1967 to 1972 in lifetime premium to:			
Logical reasoning skill (P_{c1})	-0.000298	0.000622	0.002109
	(0.000040)	(0.000709)	(0.001870)
Vocabulary knowledge skill (P_{c2})	-0.000266	-0.000353	-0.001547
	(0.000042)	(0.000740)	(0.001903)
Number of individuals			
1967 cohort	42,427	2,927	2,927
1972 cohort	45,397	3,451	3,451

Panel B: Correlations in skill measures

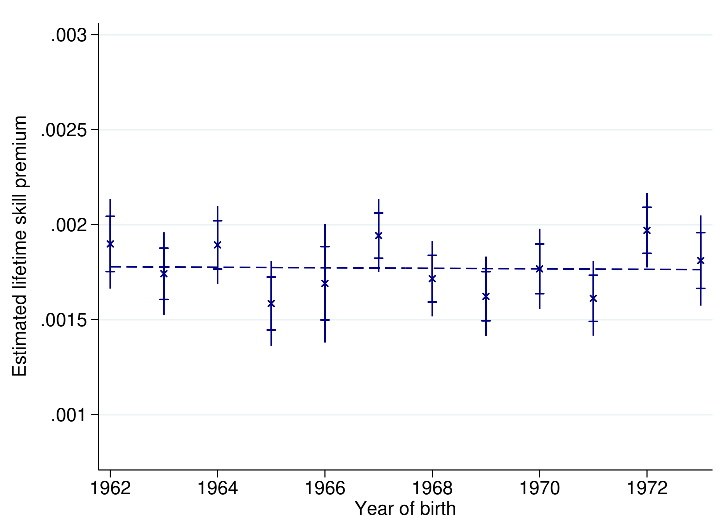
	Cohort		
	1967	1972	Difference
Correlation between survey and enlistment data in:			
Logical reasoning skill (x_{i1})	0.6557	0.6795	0.0237
	(0.0119)	(0.0085)	(0.0157)
Vocabulary knowledge skill (x_{i2})	0.6738	0.6910	0.0172
	(0.0106)	(0.0080)	(0.0129)
Number of individuals	2,927	3,451	

Notes: Panel A compares the estimated change in lifetime skill premia between birth cohorts 1967 and 1972 based on different estimation methods. The first column is based on the linear trend fitted to the series of estimated lifetime skill premia for the enlistment data, where tests were typically taken at age 18 or 19, as shown in Panel B of Figure 2.1 in the main text. The second and third columns are the differences between the lifetime skill premia for the two cohorts, as estimated on the set of individuals who have valid logical reasoning and vocabulary knowledge test scores in both the enlistment and survey data, where tests were typically taken at age 13. In the second (OLS) column, we estimate the lifetime skill premia for each cohort as the net present value of age-specific skill premia estimated via OLS, following the approach in Section 2.4.1 in the main text. In the third (IV) column, we estimate the lifetime skill premia for each cohort as the net present value of age-specific skill premia estimated via IV, treating age-13 test scores as instruments for age-18/19 test scores. Panel B compares, between birth cohorts 1967 and 1972, the Pearson correlation of skills measured in the survey data with skills measured in the enlistment data. In both panels, standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates.

Figure 2.A: Trends in Technical Knowledge and Technical Knowledge Premia Across Birth Cohorts 1962–1973, Military Enlistment Sample



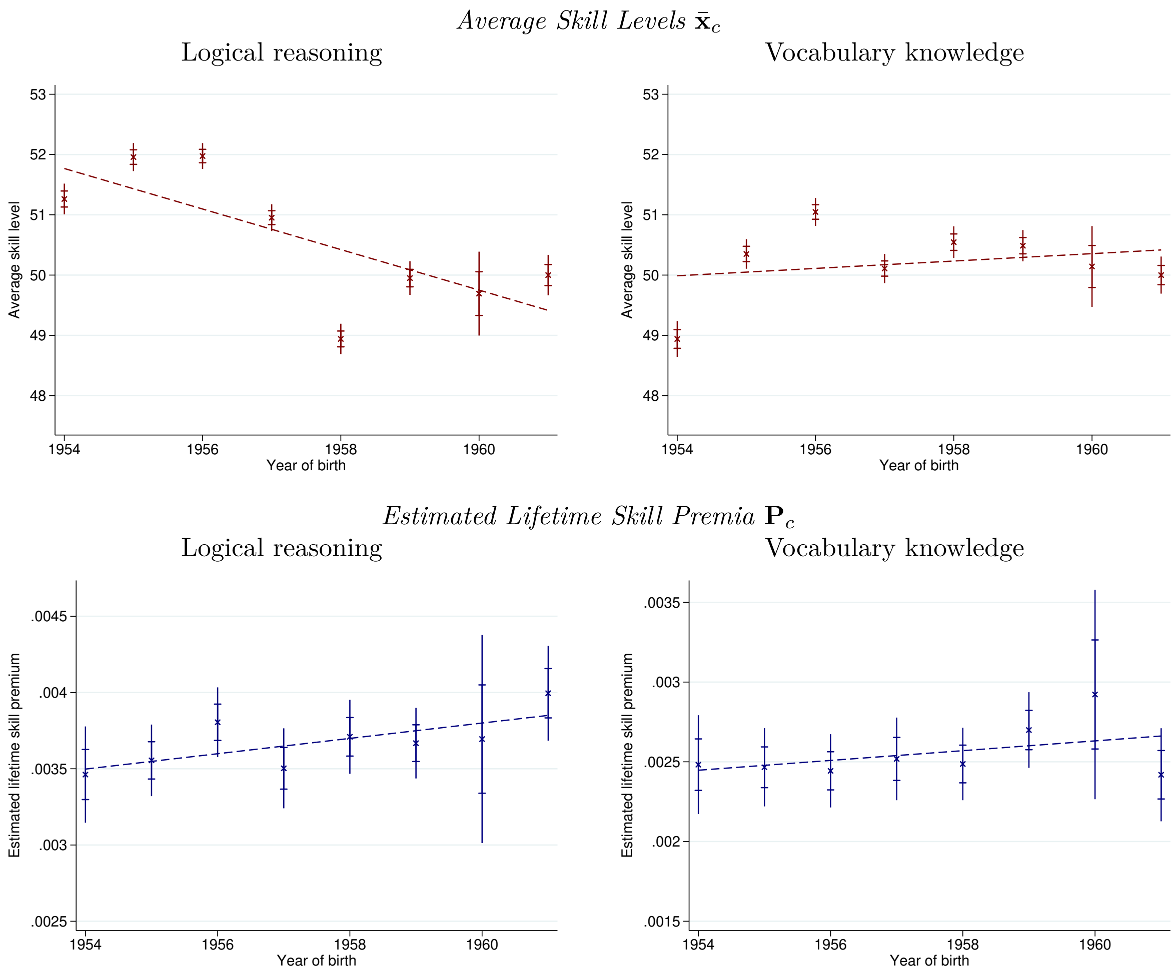
(a) Average Skill Level $\bar{x}_{cj}$



(b) Estimated Lifetime Skill Premium P_{cj}

Notes: Data are from the military enlistment sample for birth cohorts 1962–1973. We exclude birth cohorts 1974 and 1975 because of significant amounts of missing data on technical knowledge test scores for these cohorts. The left plot depicts the average technical knowledge skill $\bar{x}_{cj}$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1967 birth cohort. The right plot depicts the estimated lifetime skill premium P_{cj} for technical knowledge for each birth cohort, constructed as described in Section 2.4.1 in the main text. These skill premia are estimated controlling for logical reasoning and vocabulary knowledge skills. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

Figure 2.B: Trends in Skills and Skill Premia across Birth Cohorts 1954–1961, Military Enlistment Sample



Notes: Data are from the military enlistment sample covering Swedish men born between 1954 and 1961 and who enlisted before 1980. For these birth cohorts, information on logical reasoning and vocabulary knowledge skills is based on scores from tests administered at military enlistment, called the Enlistment Battery 67. The first row of plots depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1961 birth cohort. The second row of plots depicts the estimated lifetime skill premia $\mathbf{P}_c$ for each birth cohort, constructed as described in Section 2.4.1 in the main text. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

Figure 2.C: Structure of the Survey of Parents' Perceptions

Panel A: Consent Form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Samtyckesblankett för Enkät-undersökning alt. Elektroniska val

Stockholms universitet är personuppgiftsansvarig för den behandling av personuppgifter som sker i Survey and Report.

Den lagliga grunden för behandlingen är att du gett samtycke till behandlingen i någon enkät.
För att projektet ska kunna utföras kommer ansvarig för studien och ansvarig för projektet ha tillgång till personuppgifterna. Uppgifterna kommer att behandlas så att inte obehöriga kan ta del av dem.

Om detta material bedöms ha ett bestående värde enligt de riktlinjer som anges i 6-8 §§ i RA-FS 1999:1 kommer det att bevaras för framtiden.

Enligt EU:s dataskyddsförordning samt nationell kompletterande lagstiftning har du rätt att:

återkalla ditt samtycke utan att det påverkar lagligheten av behandling som skett i enlighet med samtycket innan det återkallades,

begära tillgång till dina personuppgifter,

få dina personuppgifter rättade,

få dina personuppgifter raderade,

få behandlingen av dina personuppgifter begränsad.

Under vissa omständigheter medger dataskyddsförordningen samt kompletterande nationell lagstiftning undantag för dessa rättigheter, som kan komma att tillämpas.

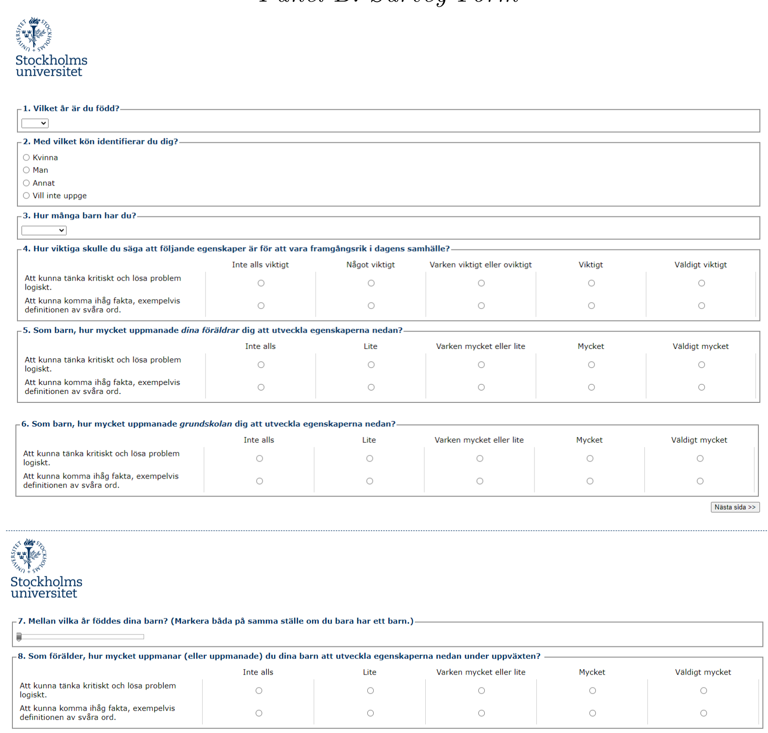
Om du vill åberopa någon av dessa rättigheter Kontakta Dataskyddsombudet vid Stockholms universitet (dso@su.se).

Mer information om detta finns på Datainspektionens hemsida. https://www.datainspektionen.se/

☐ Jag har läst och accepterar villkoren

Jag nekar samtycke | Jag samtycker

Panel B: Survey Form


Stockholms universitet

1. Vilket år är du född?

2. Med vilket kön identifierar du dig?
- Kvinna
- Man
- Annat
- Vill inte uppge

3. Hur många barn har du?

4. Hur viktiga skulle du säga att följande egenskaper är för att vara framgångsrik i dagens samhälle?

	Inte alls viktigt	Något viktigt	Varken viktigt eller oviktigt	Viktigt	Väldigt viktigt
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

5. Som barn, hur mycket uppmanade *dina föräldrar* dig att utveckla egenskaperna nedan?

	Inte alls	Lite	Varken mycket eller lite	Mycket	Väldigt mycket
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

6. Som barn, hur mycket uppmanade *grundskolan* dig att utveckla egenskaperna nedan?

	Inte alls	Lite	Varken mycket eller lite	Mycket	Väldigt mycket
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

Nästa sida >>

Stockholms universitet

7. Mellan vilka år föddes dina barn? (Markera båda på samma ställe om du bara har ett barn.)

8. Som förälder, hur mycket uppmanar (eller uppmanade) du dina barn att utveckla egenskaperna nedan under uppväxten?

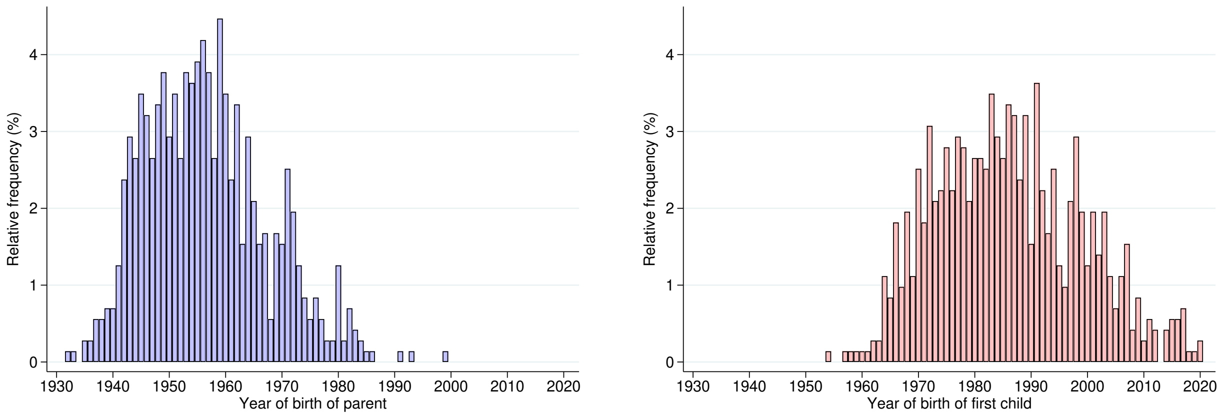
	Inte alls	Lite	Varken mycket eller lite	Mycket	Väldigt mycket
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

<< Föregående sida | Skicka nu

Notes: This figure shows the content and structure of the survey on parents' perceptions described in Section 2.3.2 in the main text. Panel A displays the consent form and Panel B displays the survey form, both in the original Swedish.

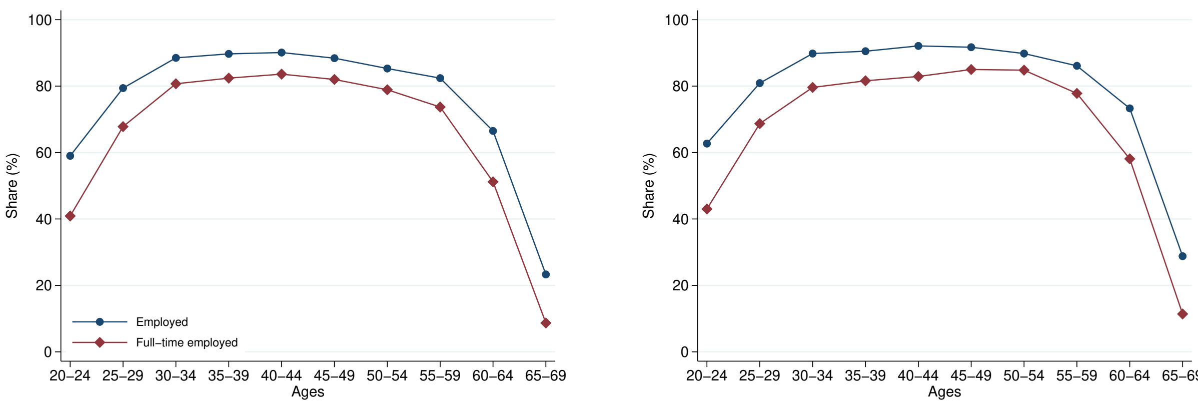
Figure 2.D: Distributions of Year of Birth of Respondent and First Child in the Survey of Parents' Perceptions

Panel A: Respondent | Panel B: Respondent's First Child



Notes: Data come from the survey of parents' perceptions described in Section 2.3.2 in the main text. Panel A shows the distribution of the year of birth of the respondent. Panel B shows the distribution of the year of birth of the respondent's first child.

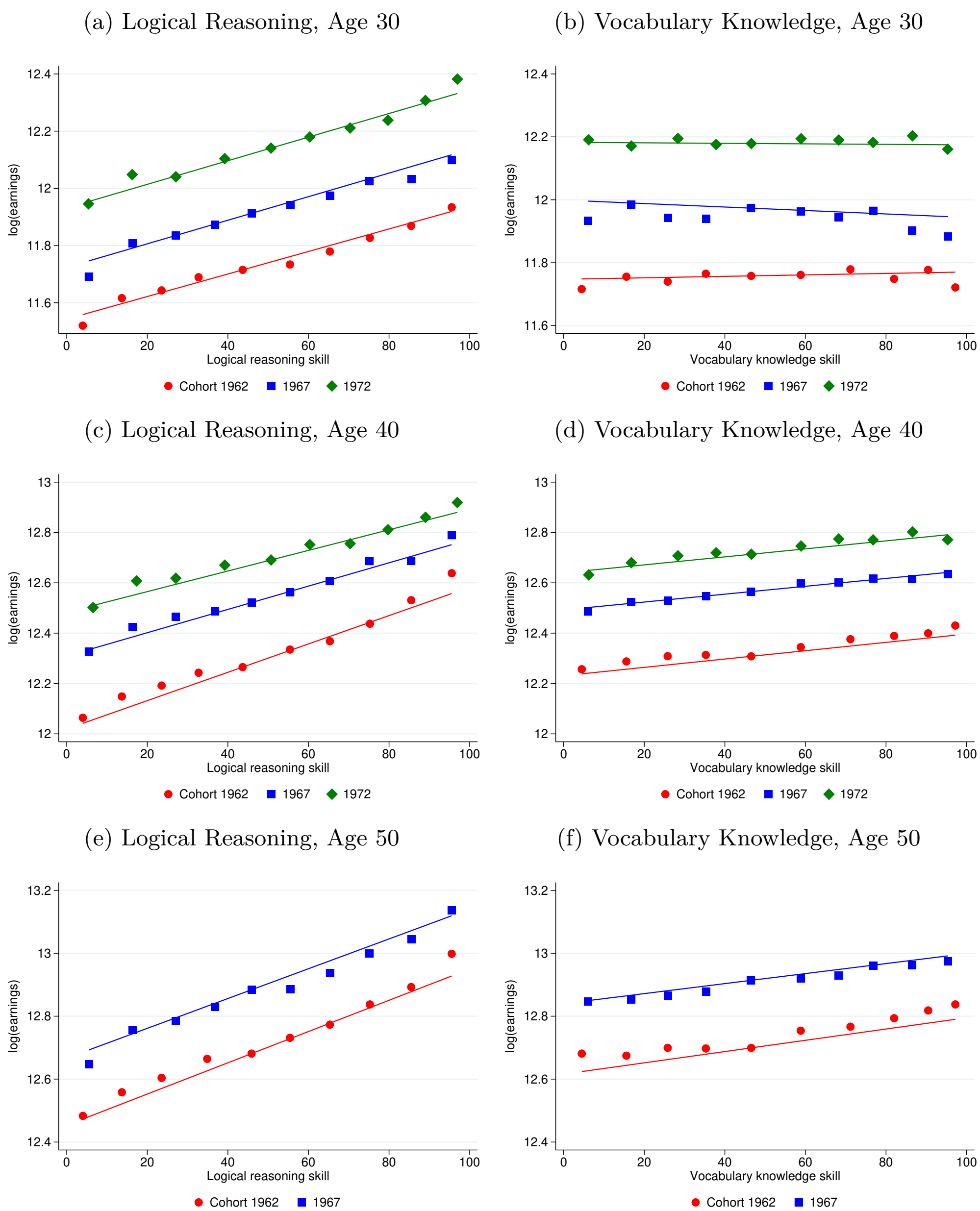
Figure 2.E: Male Employment Rates by Age Group for Selected Years



(a) 2010 (b) 2019

Notes: This figure shows the rates of employment and full-time employment among men in Sweden in 2010 and 2019, separately by age group, based on data from the Swedish Labour Force Surveys (Statistics Sweden, 2020a). We define an individual as employed if he meets the definition of employment used by the International Labor Organization (see, e.g., Eurostat, 2021). We define an employed individual as full-time employed if he reports working full-time in the survey.

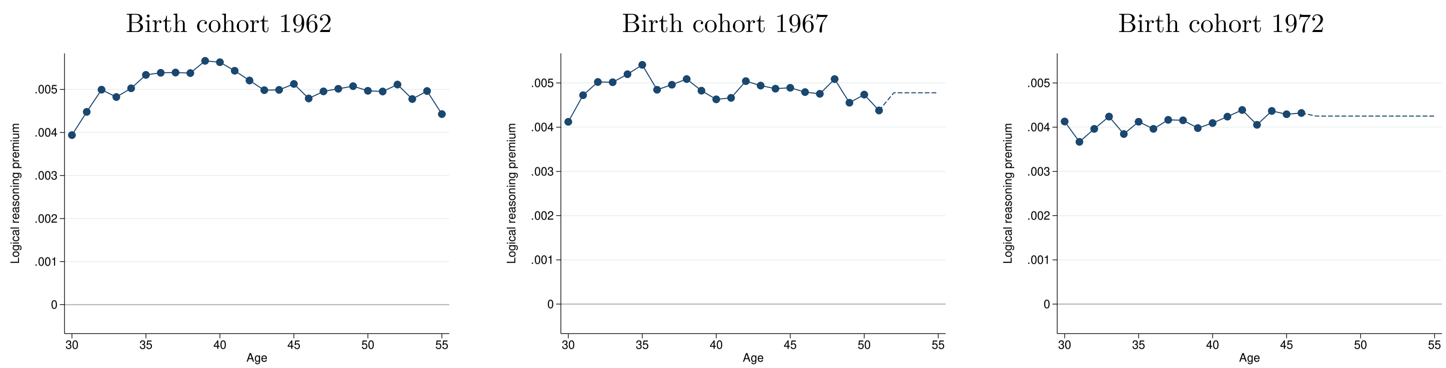
Figure 2.F: Illustrating the Relationship Between Log(Earnings) and Skill Percentile, Military Enlistment Sample



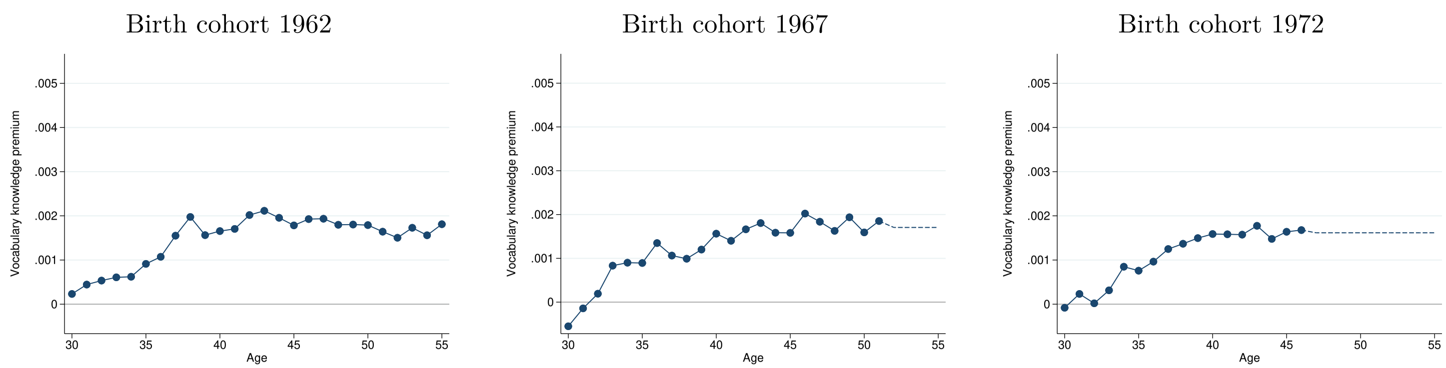
Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. This figure illustrates the relationship between the mean of log annual earnings and logical reasoning and vocabulary knowledge skill for birth cohorts 1962, 1967, and 1972, at ages 30, 40, and 50. For each cohort, age, and skill dimension, we estimate a regression of log(earnings) on indicators for decile of skill. We plot the coefficients on the decile indicators, shifted by a constant so that their mean value coincides with the sample mean of log(earnings), against the average value of the given skill within the decile. We also plot a line whose slope is equal to the estimated premium $p_{c+a,a,j}$ of the given skill dimension, estimated from a regression of log(earnings) on skills $\mathbf{x}_i$, and whose intercept is chosen so that the line coincides with the decile coefficient at the fifth decile.

Figure 2.G: Illustrating the Extrapolation of Skill Premia to Ages with No Earnings Data, Military Enlistment Sample

Logical Reasoning Skill Premium

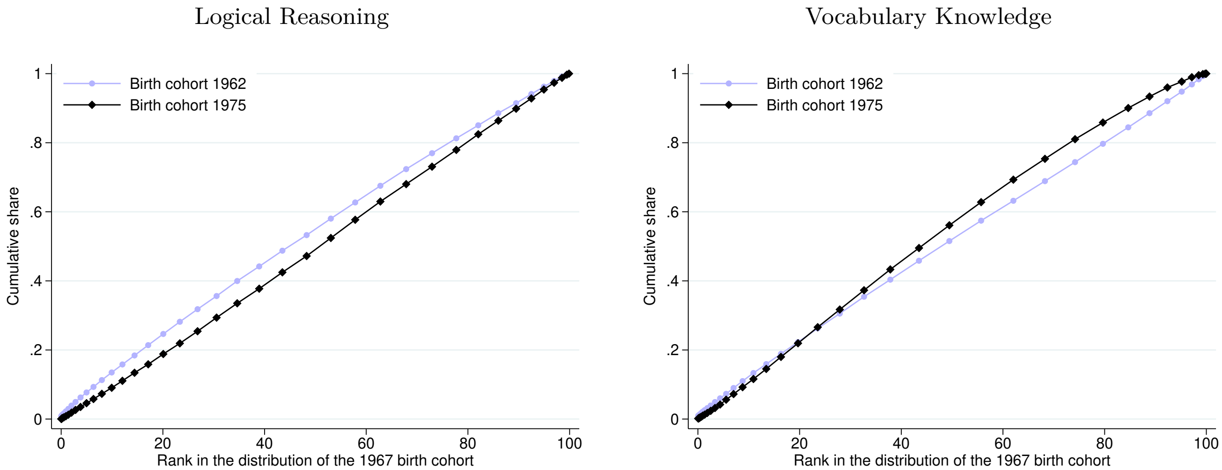


Vocabulary Knowledge Skill Premium



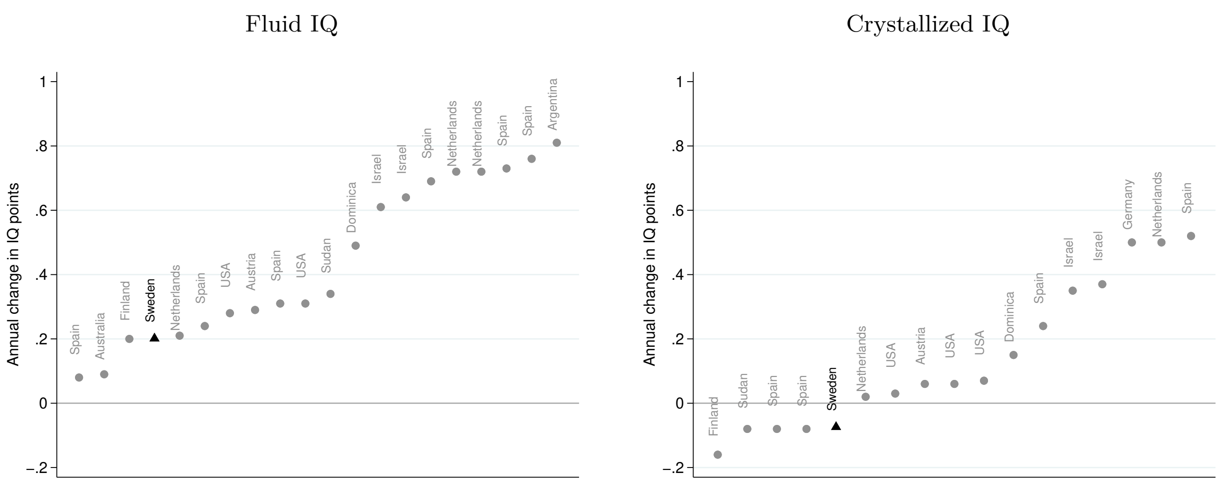
Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. The plots illustrate how we estimate skill premia for ages of working life for which we do not observe earnings. The upper row of plots illustrates for logical reasoning and the lower row of plots illustrates for vocabulary knowledge. Each row includes plots for birth cohorts 1962, 1967, and 1972. For each cohort, we estimate the skill premia $\mathbf{p}_{c+a,a}$ in ages for which we do not observe earnings (dashed line) by taking the average estimated skill premia across all ages 40+ for which we do observe earnings (markers).

Figure 2.H: Distributions of Skills in the 1962 and 1975 Birth Cohorts, Military Enlistment Sample



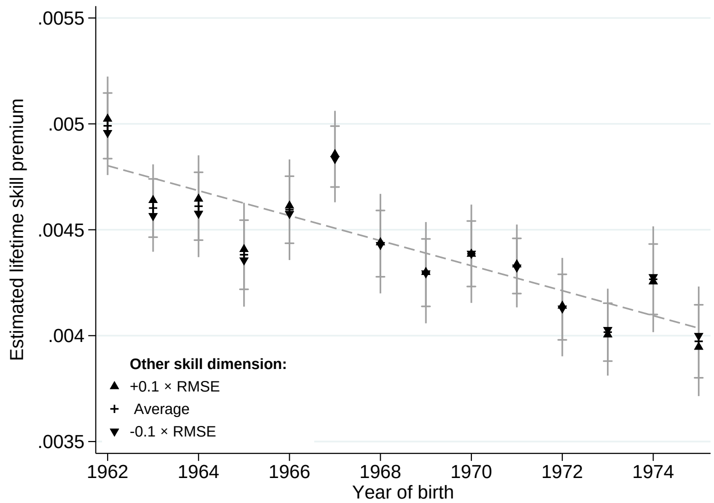
Notes: Data are from the military enlistment sample covering birth cohorts 1962 and 1975, with tests typically taken at age 18 or 19. Each plot depicts the empirical cumulative distribution function of skills x_{ij} for a given dimension j for members i of the 1962 and 1975 birth cohorts. Skills are expressed as a percentile of the distribution for the 1967 birth cohort.

Figure 2.I: Measured Trends in Fluid and Crystallized IQ

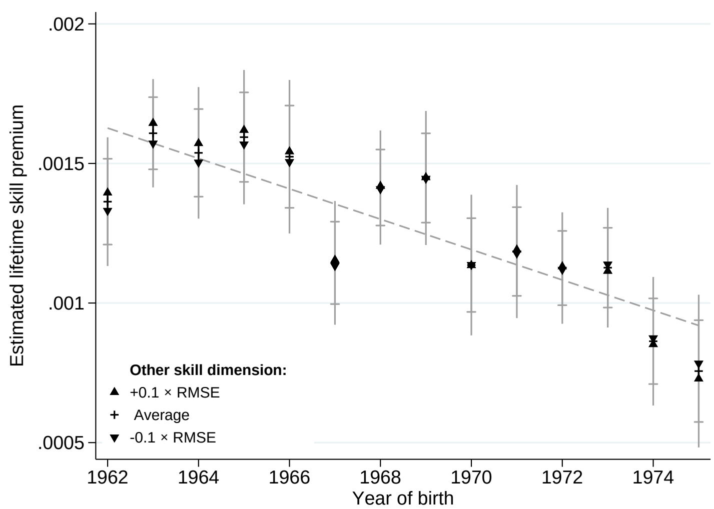


Notes: Data are from Pietschnig and Voracek (2015, Table S1, circles) or from the military enlistment sample covering birth cohorts 1962–1975 (triangles). We select from Pietschnig and Voracek's meta-analysis all single-country studies of fluid or crystallized intelligence covering healthy adults with a sample size of at least 100 and a study period ending in 1980 or later. We classify studies of PIQ as fluid and studies of VIQ or verbal as crystallized. We plot the annual IQ gain in each study, labeling each study with the country in which the sample was obtained. For comparison, we also plot the annual IQ gain in the enlistment sample, which we calculate by standardizing the raw score on the logical reasoning (fluid) and vocabulary knowledge (crystallized) tests to have a mean of 100 and a standard deviation of 15 in the 1967 cohort.

Figure 2.J: Trends in Skill Premia across Birth Cohorts 1962–1975, Allowing for Interactions, Military Enlistment Sample



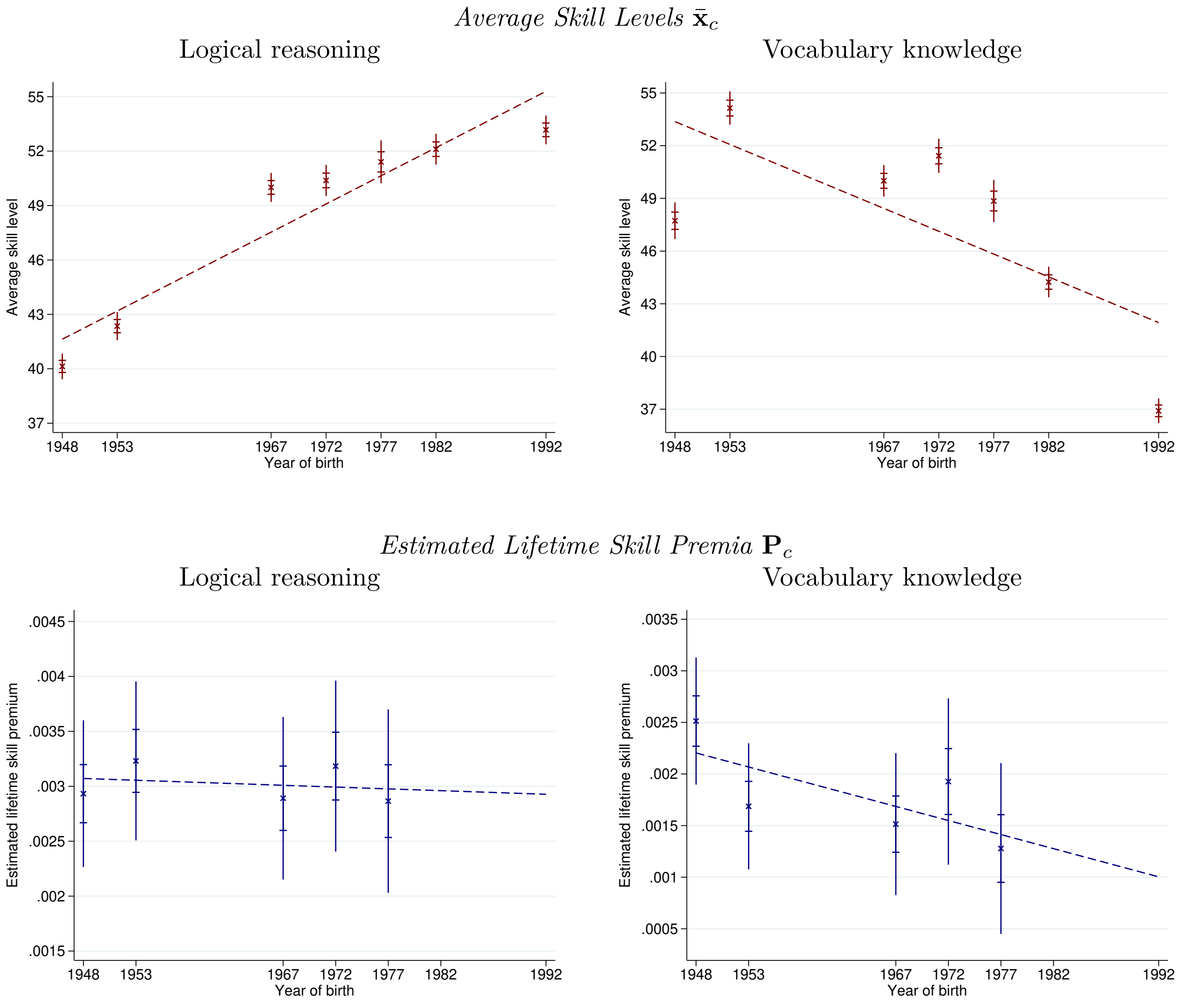
Logical Reasoning



Vocabulary Knowledge

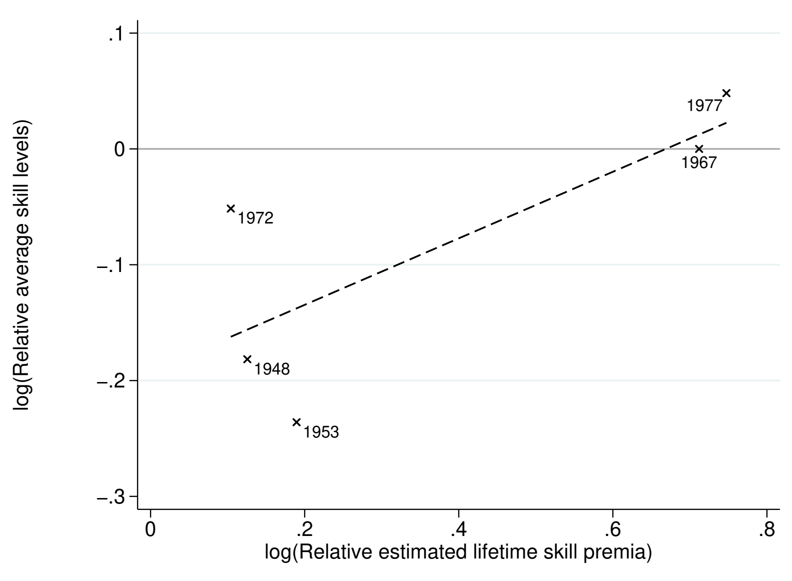
Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. We construct the plots as follows. For each cohort c and each year t for which we measure earnings, we estimate a generalization of equation (2.1) in the main text that includes an interaction $x_{i1}x_{i2}$ between the two skill dimensions. From these estimates we calculate cohort-and-year-specific skill premia for each skill dimension j, evaluated at three different levels of skill on the other dimension $j' \neq j$: the cohort average, 0.1 root mean squared error (RMSE) above the cohort average, and 0.1 RMSE below the cohort average, where the RMSE is calculated from a cohort-specific regression of skill $x_{ij'}$ on indicators for skill x_{ij}. We then follow the approach described in Section 2.4.1 in the main text to estimate the cohort-and-year-specific premia for years outside of our sample, and we compute lifetime premia following equation (2.3) in the main text. For each dimension j, the plot depicts the lifetime premium for an individual in each cohort c whose skill on the other dimension $j' \neq j$ is equal to the cohort average ("Average"), an individual whose skill on the other dimension is 0.1 RMSE above the cohort average ("+0.1 × RMSE"), and an individual whose skill on the other dimension is 0.1 RMSE below the cohort average ("$-0.1 \times$ RMSE"). Each plot includes a line of best fit, 95 percent pointwise confidence intervals (inner grey intervals, marked by dashes), and uniform confidence bands (outer grey intervals, marked by line segments) corresponding to the "Average" series. Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019).

Figure 2.K: Trends in Skills and Skill Premia across Birth Cohorts, Survey Sample



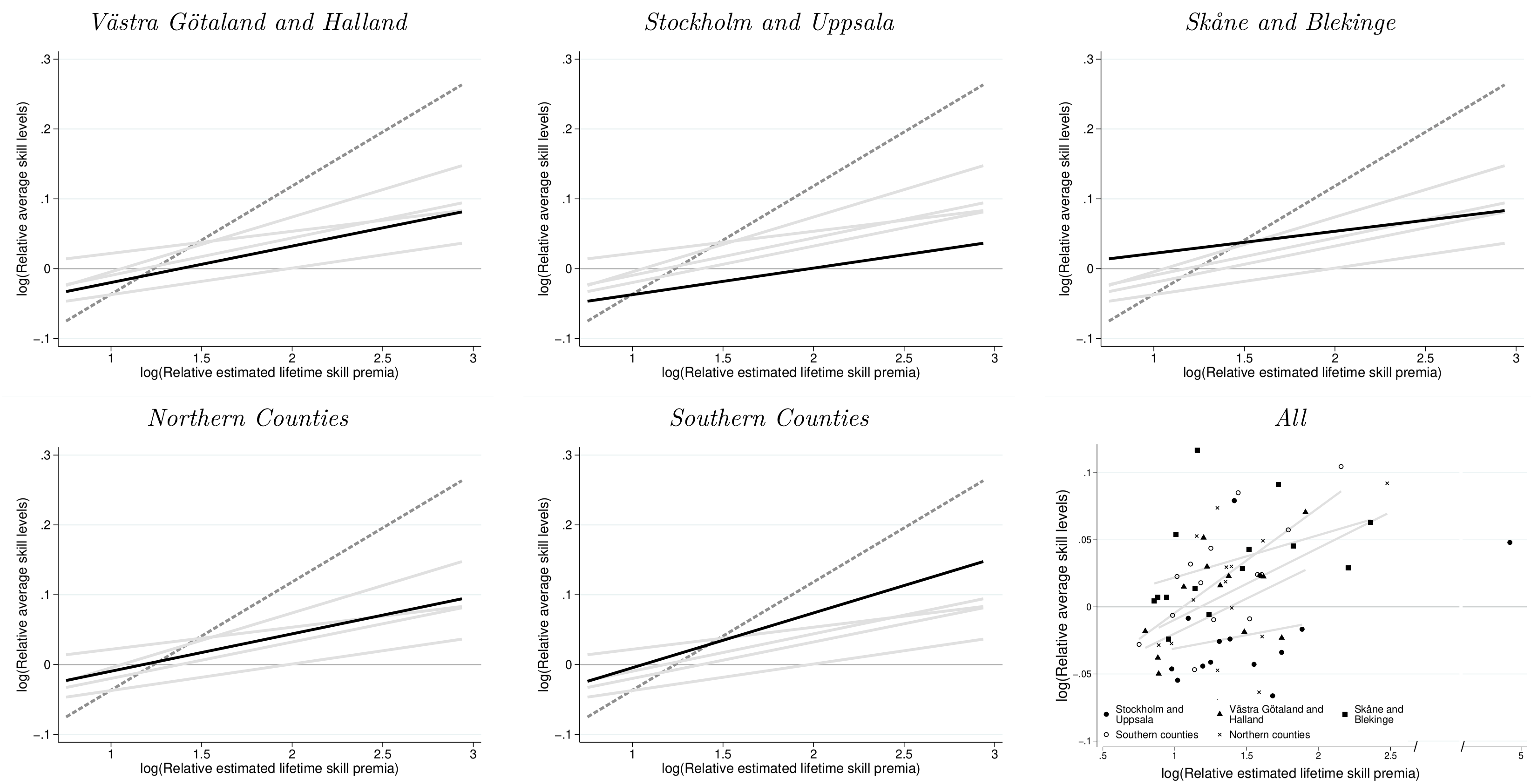
Notes: Data are from the survey sample covering birth cohorts 1948, 1953, 1967, 1972, 1977, 1982, and 1992. The first row of plots depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1967 birth cohort. The second row of plots depicts the estimated lifetime skill premia $\mathbf{P}_c$ for each birth cohort c in 1948, 1953, 1967, 1972, and 1977, constructed as described in Section 2.4.1 in the main text. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019) Each plot depicts the line of best fit through the estimated points.

Figure 2.L: Evolution of Relative Skill Levels and Relative Skill Premia, Women in Survey Sample



Notes: Data are from the survey sample covering birth cohorts 1948, 1953, 1967, 1972, and 1977, with tests typically taken at age 13, for female respondents. The plot shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$. The dashed line depicts the line of best fit.

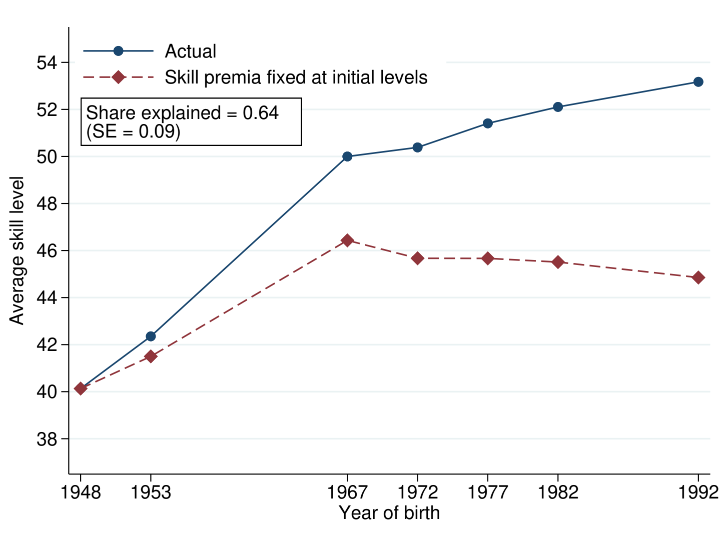
Figure 2.M: Evolution of Relative Skill Levels and Relative Skill Premia by Region, Military Enlistment Sample



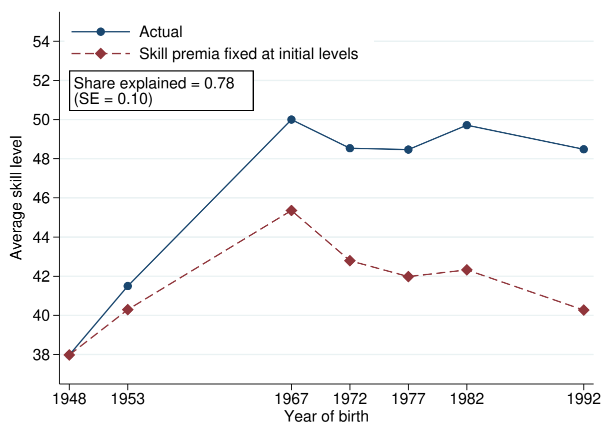
Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. The lower right plot shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln(\overline{x}_{c1}/\overline{x}_{c2})$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln(P_{c1}/P_{c2})$, separately by birth region. Each marker corresponds to a cohort and region. The light gray lines depict the lines of best fit for each region. Each of the other plots shows the line of best fit for the given region (black line), the lines of best fit for the other regions (light gray lines), and the line of best fit for the full sample (dashed line). We classify individuals into mutually exclusive and exhaustive regions according to their county of birth, excluding those born outside of Sweden, using data from Statistics Sweden (2016a). We classify Värmland, Örebro, Västmanland, Dalarna, Gävleborg, Västernorrland, Jämtland, Västerbotten, and Norrbotten as Northern counties, Södermanland, Östergötland, Jönköping, Kronoberg, Kalmar, and Gotland as Southern counties, and the remaining counties eponymously.

Figure 2.N: Decomposition of Change in Average Logical Reasoning Skill, Survey Sample

Men



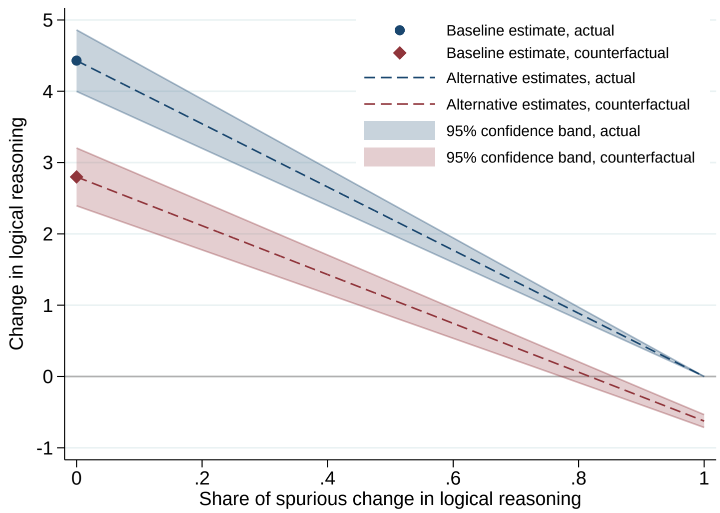
Women



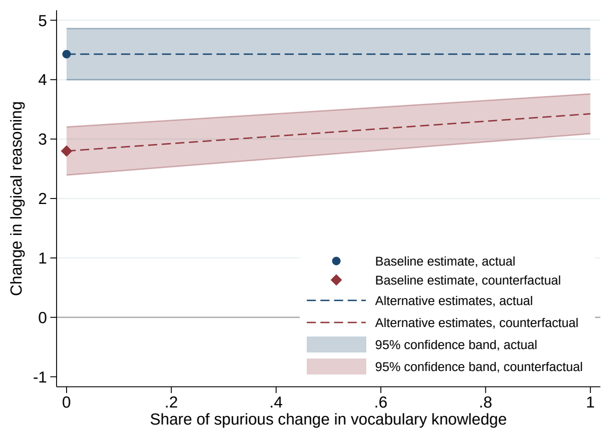
Notes: Data are from the survey sample of male respondents (upper panel) and female respondents (lower panel) covering birth cohorts 1948, 1953, 1967, 1972, 1977, 1982, and 1992, with tests typically taken at age 13. Each plot depicts the average logical reasoning skill $\bar{x}_{c1}$ for each birth cohort c ("Actual") and the predicted average skill $\tilde{x}_{c1}\left(\overline{\mathbf{P}}_{\underline{c}}\right)$ under the counterfactual in which lifetime skill premia remain at the level estimated for the 1948 birth cohort ("Skill premia fixed at initial levels"). Skills are expressed as a percentile of the distribution for the 1967 birth cohort. We fit the model as in Figure 2.4 in the main text, separately for men and women, taking the linear fit for the cohorts through 1977 (depicted for men in Appendix Figure 2.K) as our estimate of the lifetime skill premia $\mathbf{P}_c$ for all cohorts. The text box in each plot shows the estimated share $1 - \frac{\tilde{x}_{\bar{c}1}\left(\mathbf{P}_{\underline{c}}\right) - \tilde{x}_{\underline{c}1}\left(\mathbf{P}_{\underline{c}}\right)}{\bar{x}_{\bar{c}1} - \bar{x}_{\underline{c}1}}$ of the observed change from 1948 through 1992 that is accounted for by changes in skill premia ("Share explained"). The standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. We exclude seven and three bootstrap replicates from the calculation of standard errors for the upper and lower plots, respectively, due to values inconsistent with the model.

Figure 2.O: Sensitivity to Spurious Cohort Trends in Skills

Logical Reasoning

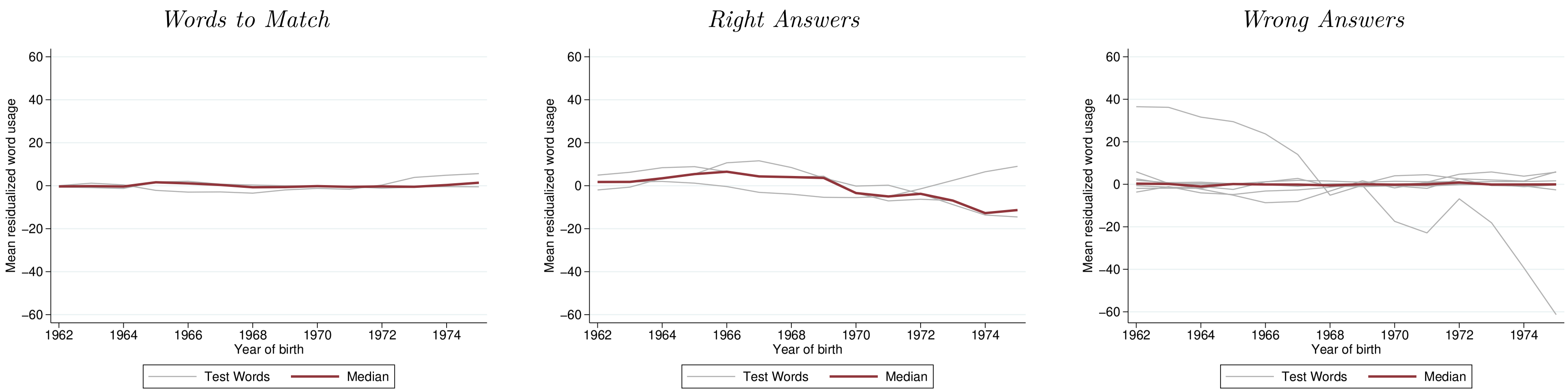


Vocabulary Knowledge



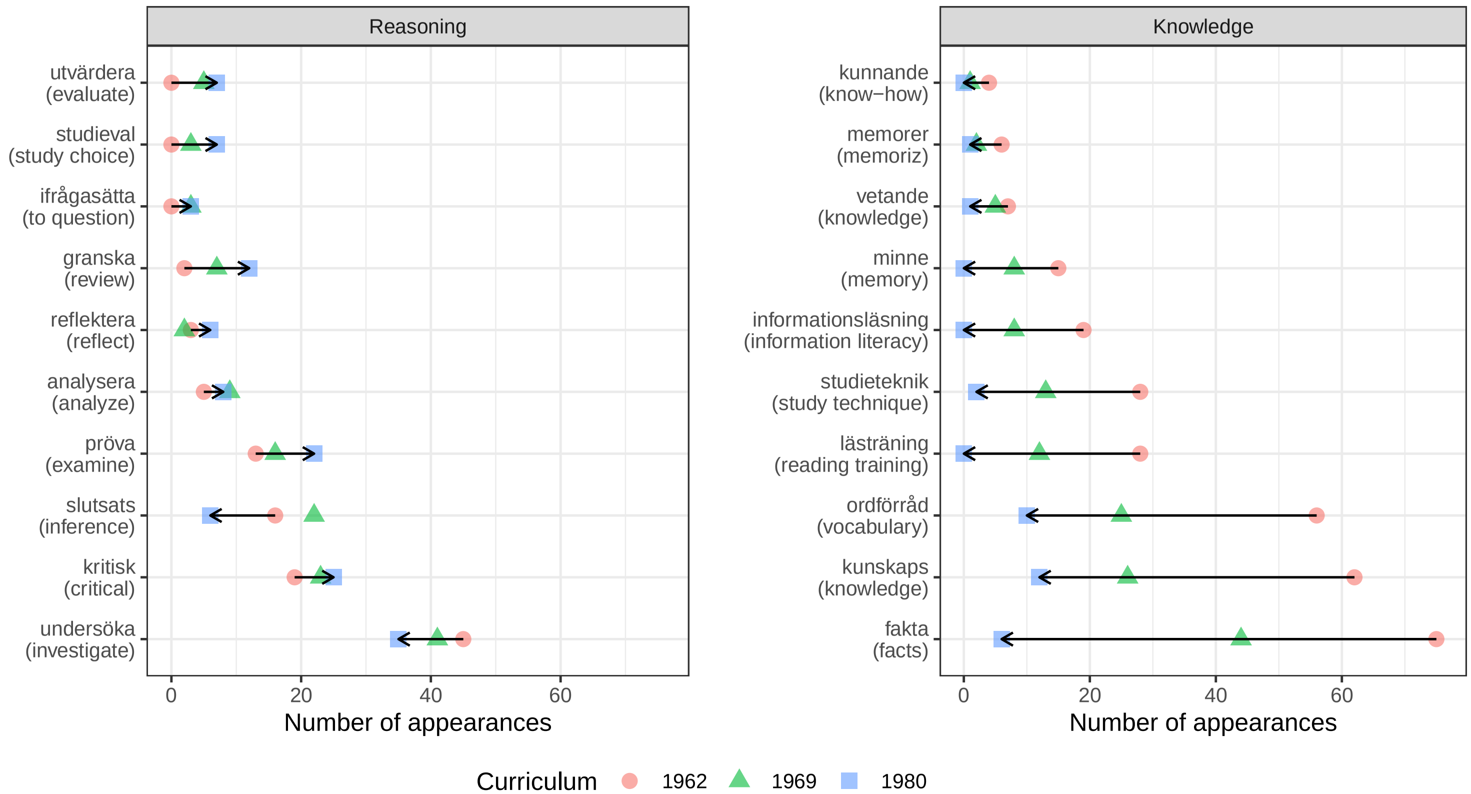
Notes: Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. To construct each plot, we assume that the true mean skill level $\tilde{x}_{\overline{c}j}$ on dimension j in the 1975 birth cohort $\overline{c}$ is given by $\omega_j \overline{x}_{\underline{c}j} + (1-\omega_j)\,\overline{x}_{\overline{c}j}$, such that $\omega_j \in [0,1]$ denotes the fraction of the observed change $\overline{x}_{\overline{c}j} - \overline{x}_{\underline{c}j}$ that is spurious. We then re-estimate our model following the methods in Table 2.1 in the main text and calculate, for each ω_j, the implied actual change in logical reasoning skill $\tilde{x}_{\overline{c}1}\left(\mathbf{P}_{\overline{c}}\right) - \tilde{x}_{\underline{c}1}\left(\mathbf{P}_{\underline{c}}\right)$ and the implied counterfactual change in logical reasoning skill $\tilde{x}_{\overline{c}1}\left(\mathbf{P}_{\underline{c}}\right) - \tilde{x}_{\underline{c}1}\left(\mathbf{P}_{\underline{c}}\right)$ if skill premia had remained constant at their level for the 1962 birth cohort. Each plot depicts the actual and counterfactual change in logical reasoning skill (y-axis) as a function of the fraction of the observed change that is spurious (x-axis). The upper plot depicts the implications of a spurious change in logical reasoning skill ($\omega_1 \in [0,1]$, $\omega_2 = 0$). The lower plot depicts the implications of a spurious change in vocabulary knowledge ($\omega_1 = 0$, $\omega_2 \in [0,1]$). For each depicted series, the shaded region collects pointwise 95% confidence intervals obtained via a nonparametric bootstrap with 50 replicates. The estimates labeled "Baseline estimate" correspond to the estimates in Panel A of Table 2.1 in the main text, i.e., the case in which $\omega_1 = \omega_2 = 0$.

Figure 2.P: Trends in Usage of Words in Example Vocabulary Questions



Notes: Data are from Kungliga biblioteket (2022). We construct the plots as follows. We begin with the example questions for Enlistment Battery 2000 posted at Rekryteringsmyndigheten (2010). We classify the words of interest in the example synonym questions Rekryteringsmyndigheten (2012) into the words the test-taker is asked to match to a synonym ("words to match"), the correct synonyms ("right answers"), and the incorrect synonyms ("wrong answers"). We specify a set of reference words consisting of the names of the days of the week. From Kungliga biblioteket (2022) we obtain the frequency with which each word of interest, as well as each reference word, was used in the newspaper *Dagens Nyheter* in each year from 1962 to 1993. For each word of interest we estimate a regression of the frequency of the word's use on the total frequency across all reference words, where the unit of observation is the word-year. We take the residual from this regression and compute, for each word of interest and each birth cohort 1962–1975, the average residual over the first 19 years of the cohort's life. We subtract the mean of this series and take the result as our measure of the cohort's exposure to the given word of interest. In each plot, each lighter line shows the exposure of each cohort to a given word of interest, and the darker line shows the median exposure of each cohort across all words of interest in the given category. We use example questions for Enlistment Battery 2000 because we are not aware of example questions for Enlistment Battery 1980 that are in the public domain.

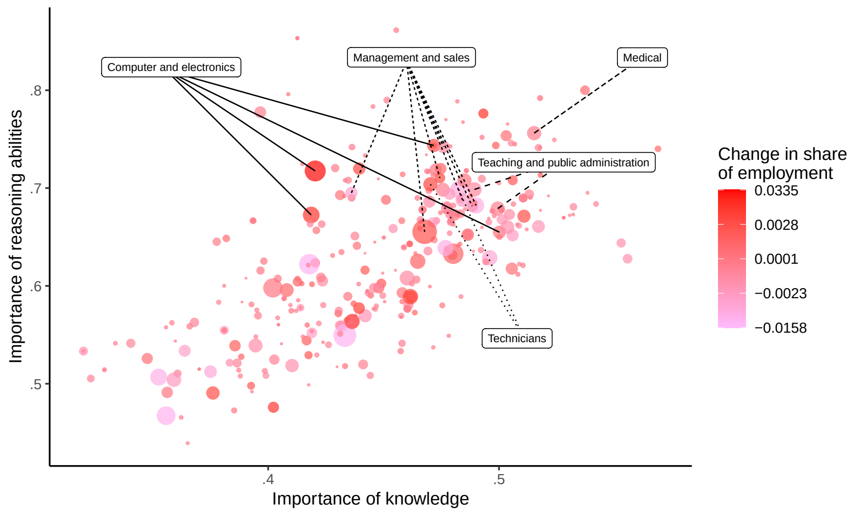
Figure 2.Q: Selected Word Families Related to Reasoning vs. Knowledge in Swedish Primary School Curricula



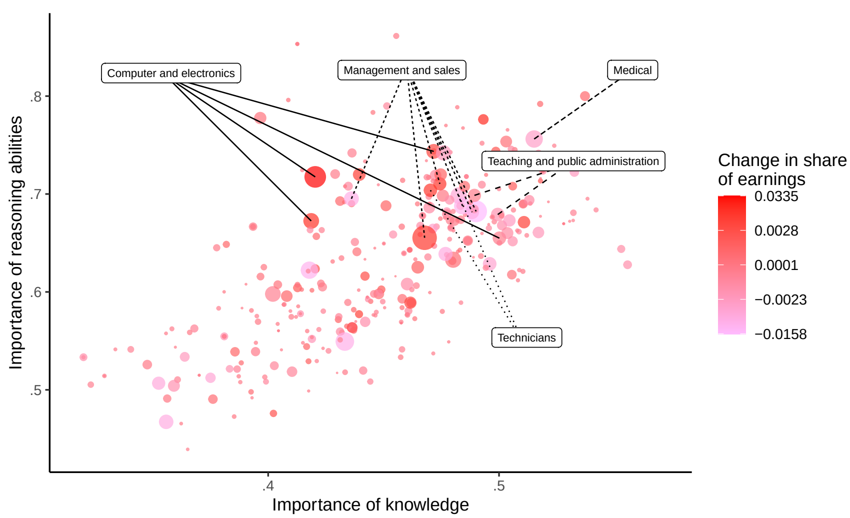
Notes: The plot shows the number of appearances of selected word families related to fluid intelligence ("Reasoning", left panel) and crystallized intelligence ("Knowledge", right panel) in the 1962, 1969, and 1980 revisions of the Swedish primary school Curricula (Läroplan for grundskolan; Skolöverstyrelsen 1962, 1969, 1980). Translations of word families are in parenthesis under the original Swedish. In both panels, word families are listed in ascending order based on their number of appearances in the 1962 Curriculum. The arrows point from the number of appearances in the 1962 Curriculum to the number of appearances in the 1980 Curriculum. We chose a set of keywords based on a close reading of the Curricula and categorized them in word families. We counted the number of appearances of word family as follows. For the "analysera," "granska," "ifrågasätta," "informationsläsning," "kunnande," "ordförråd," "pröva," "reflektera," "slutsats," "studieval," and "utvärdera" families, we searched for all words that start with the same characters. For the "fakta," "kritisk," "kunskaps," "memorer," "minne," "undersöka," and "vetande" families, we searched for all words that start with the same characters plus a few alternate forms. For the "lästräning" and "studieteknik" families, we searched for the exact word only. We conducted searches automatically. A Swedish speaker then reviewed search results and excluded cases, such as negations, where usage did not match our intent.

Figure 2.R: Growth of Occupations in Sweden by Their Reasoning and Knowledge Intensity

(a) Changes in Shares of Total Employment



(b) Changes in Shares of Total Earnings



Notes: The figure shows scatterplots of the knowledge and reasoning intensity well as growth of each occupation in the Swedish Occupational Register. Panel A measures occupation growth with the change in the occupation's share of total employment between cohorts 1962 and 1975, with marker sizes proportional to the occupation's share of total employment in cohort 1962. Panel B measures occupation growth with the change in the share of total earnings between cohorts 1962 and 1975, with marker sizes proportional to the occupation's share of total earnings in cohort 1962. In both panels, the y-axis depicts the total importance of reasoning abilities, and the x-axis depicts the total importance of knowledge. We measure the distribution of employment and earnings across occupations in the Swedish Occupational Register using data on employment histories from 2004 onwards from Statistics Sweden (2021), using 4-digit Swedish Standard Classification of Occupations 96 (SSYK 96) codes, and taking each individual's occupation to be the one observed in the available year closest to the year the individual turns 40. For each US Department of Labor, Employment and Training Administration (2020) occupation we define the total importance of reasoning abilities by summing the importance scores of Inductive, Deductive, and Mathematical Reasoning abilities and dividing by the highest possible sum. Similarly, we define the total importance of knowledge by summing the importance scores of all knowledge categories and dividing by the highest possible sum. We compute the total importance of reasoning abilities and knowledge of each SOC 2010 occupation by taking unweighted averages across all corresponding occupations in US Department of Labor, Employment and Training Administration (2020). We match the occupations in the Swedish Occupational Register to occupations in the Standard Occupational Classification 2010 (SOC 2010) by using the crosswalks from Statistics Sweden (2016c) and BLS (2015), manually excluding some matches to improve accuracy. We define the total importance of reasoning abilities and knowledge for each occupation in the Swedish Occupational Register by taking employment-weighted averages across all corresponding SOC 2010 occupations, using May 2018 OES employment estimates (BLS 2019) as weights. Heuristic descriptions, written by us, are applied to all occupations with total importance of reasoning above 0.65 and a share of earnings in 1962 of least 0.01. This figure includes information from the O*NET 25.0 Database by the US Department of Labor, Employment and Training Administration (USDOL/ETA). Used under the CC BY 4.0 license. O*NET® is a trademark of USDOL/ETA. We have modified all or some of this information. USDOL/ETA has not approved, endorsed, or tested these modifications.

CHAPTER 3

Collective Bargaining Networks, Rent-sharing, and the Propagation of Shocks

3.1 Introduction

Collective bargaining (CB, for short) establishes common provisions for workers, such as wage floors, across all firms covered in a bargaining unit. CB can therefore influence how wages respond to changes in the economic conditions of employers. In Europe, for example, differential responses of national labor markets to the Great Recession and the rise of import competition from China have been linked to differences in bargaining institutions (e.g., Barth et al., 2023; Ronchi & di Mauro, 2017). Understanding these responses is, in turn, important for the many public policies that seek to mitigate the effect of shocks on workers.[1] In the US, discussions about bargaining institutions have gained traction in recent years, and new bargaining policies have already been implemented.[2] These endeavors require an understanding of the trade-offs involved in different bargaining structures. While many papers examine how economic shocks, such as changes in product demand to firms, affect workers' wages (e.g., Autor, Dorn, & Hanson, 2013; Van Reenen, 1996) and propagate across firms and regions (e.g., Adão, Arkolakis, & Esposito, 2022; Giroud et al., 2021), there is little direct evidence on the role of CB in mediating these effects.

CB typically defines a network of firms, with two firms connected in the network if they fall under the same CB unit and therefore share the same commonly bargained provisions. The structure of the CB network differs widely across countries, with some characterized by large sectoral CB units, and others by employer-level bargaining (Bhuller et al., 2022). When economic shocks affect firms in a particular region or sector, these different forms of CB network seem likely to respond differently.[3] For instance, if firms in an affected region are connected to firms in another region via CB, then updated negotiations in the CB unit may result in wage changes in the unaffected region as well. This could be a cause for concern if it results

[1] See, for example, the US Trade Adjustment Assistance program or the EU Globalisation Adjustment Fund.

[2] See, for example, the recent California law granting bargaining power to fast-food workers. Washington Post (2022) notes that the "California law is widely seen as a step toward sectoral bargaining."

[3] As noted by Katz and Autor (1999, pp. 1540), "the same labor market shocks . . . may have different impacts on the wage structure depending on how unions and government regulations affect wage setting."

in low-wage regions covered by high wage floors that are not aligned with local conditions (Adamopoulou, Díez-Catalán, & Villanueva, 2022; Boeri et al., 2021). However, it could also be desirable if it dissipates the effects of concentrated shocks, effectively sharing the risk of shocks across workers and firms.

Studying the role of the CB network in determining the responsiveness of wages to economic shocks is challenging for several reasons. First, it requires detailed information on the CB network, which is not usually available in administrative datasets. Second, it requires measurable and exogenously determined shocks to employers. Finally, studying the effects of shocks under counterfactual CB networks requires an economic model where firms exposed to shocks are subject to common provisions negotiated by a CB network.

In this paper, I study how CB shapes the impact of economic shocks to firms. First, I leverage novel administrative data from Argentina to construct the network linking firms to CB units which, coupled with rich labor market data, allows me to study how shocks propagate through the CB network. Second, I construct plausibly exogenous product-demand shocks by exploiting changes in international demand for exported products, and compare the evolution of outcomes across firms that are shocked directly or via their CB unit. Finally, I develop and estimate a structural model where firms in different regions and economic sectors are connected by a CB network, and use it to study the incidence of shocks on wages under counterfactual networks.

The case of Argentina in the aftermath of the Great Recession provides an excellent case study for several reasons. First, CB is widespread in the country, with 93% of workers covered by some agreement in 2014 (Ministerio de Trabajo, Empleo y Seguridad Social, 2023). As in several other countries, agreements establish minimum working conditions for covered workers and firms.[4] Second, an unexpected shift in international demand for Argentina's exports in the aftermath of the Great Recession makes for a compelling natural experiment to study the role of CB in the propagation of shocks. Finally, due to the country's legal structure, CB units are not entirely determined by region or economic sector, resulting in an idiosyncratic network that allows me to compare otherwise similar firms that are nevertheless part of different CB units.

To construct product-demand shocks I leverage variation in world import demand arising from granular country-product pairs exported by Argentine firms in 2009–2013. Following Hummels et al. (2014), firms are exposed to changes in world import demand via the share of their value exported to each country-product, as reflected in Argentine Customs data. CB units are also exposed to product-demand changes via their employment shares in firms that export to each country-product. I define shocks to firms and CB units as the weighted average of the changes in world import demand, weighting by the appropriate exposure shares to each country-product.

[4] See Bhuller et al. (2022) for a recent review of collective bargaining institutions in OECD countries.

I use administrative data on firms' wages and employment and a difference-in-differences strategy to estimate the effects of firm and CB shocks. To identify the effect of *firm shocks* I compare the evolution of outcomes for firms in the same province and economic sector that are subject to a similar CB shock but a different firm shock. To identify the effect of *CB shocks* I proceed analogously, but comparing firms with a similar firm shock and a different CB shock. The key assumption is that, within province and sector cells, growth in world import demand (WID) affected firms because of changes in their product demand and in the outcomes of bargaining, and not because growing firms sorted into markets with trending WID or were affected by changes in WID through channels other than their CB unit. Following Garin and Silvério (2023), I see this as a plausible assumption given the nature of the changes in WID, which were driven by unexpected recessions and recoveries of varying magnitudes across countries following the Great Recession. In an appendix, I show that these assumptions can be cast in terms of conditional quasi-random assignment of product-demand shocks, as in recent work by Borusyak, Hull, and Jaravel (2022).

I start discussing the effects of shocks on wages and wage floors. A 10% increase in world import demand at the firm level increases wages by 0.13% (SE=0.07%), while a 10% increase in average world import demand at the CB unit level increases wages by 0.46% (SE=0.20%). Combining these results with evidence from a survey of businesses, I estimate that a shock equivalent to a 10% increase in average CB revenue would increase wages by 4.2% among covered firms. These effects are stronger for larger CB units. I use the administrative data to construct a novel dataset of wage floors, the main outcome of negotiations, by detecting bunching in the distribution of wages within CB units and occupation categories. I then estimate the models using the wage floor as outcome and find that wage floors increase by a similar magnitude as paid wages following a CB shock. This implies a pass-through of wage floors to wages close to one, larger than found by Card and Cardoso (2022), suggesting strong "wage norms" in the Argentine economy. I consistently find that wage floors respond to CB shocks but not to firm or sectoral shocks.

I then delve into the effects of shocks on employment. Importantly, theory suggests that a wage floor hike should result in heterogeneous employment responses across firms with different productivity (Ahlfeldt, Roth, & Seidel, 2022b; Berger, Herkenhoff, & Mongey, 2022). Low productivity firms are demand constrained by the wage floor, and so they should respond by decreasing employment as they move along their labor demand curve. Medium productivity firms are supply constrained implying that they should respond by increasing employment. High productivity firms should not be affected. As a proxy for productivity, I study heterogeneity of effects of the CB shock by the level of pre-period wages. I find that low wage firms in CB units that experience growth in world import demand decrease employment, consistent with this theory. I also find evidence of positive employment effects for medium wage firms, and null effects for high wage firms.

A potential threat to the view that CB matters is that shocks actually propagate through other networks that are correlated with CB units, such as geographic proximity or input-output linkages. Several additional exercises, however, suggest that the causality runs through CB units. First, I find robust estimates when varying controls for connections via local labor markets, suggesting that the effects are not driven by spillovers through proximity. Second, I find that CB shocks also affect non-exporting firms in the same CB unit, suggesting that exporter-specific unobservables are not responsible for the results. Finally, I exploit firms with workers under multiple CB units to construct a falsification test. Unlike the main analysis, where I assign the most common CB unit to each firm, in this test I compare workers bound by different CB units *within the same firm*. If a shock to a firm's main CB unit reflects spillovers through other networks, then we should not observe CB unit-specific effects within the firm. I find that workers' wages respond to shocks to their own CB unit, suggesting that the effects are driven by CB.

In the final part of the paper, I develop a general equilibrium model to investigate how the structure of the CB network affects the incidence of shocks on wages. The model assumes the existence of heterogeneous local labor markets, defined by region and economic sector, which are affected by wage floors. CB units, defined as partitions of local labor markets, bargain over a common wage floor anticipating the response of covered local markets. I use a "Nash-in-Nash" solution concept, first introduced by Horn and Wolinsky (1988), to model the equilibrium of the bargaining game between unions and employer associations. The key parameters that govern heterogeneity in the model are estimated by inversion of equilibrium conditions. In particular, I use the share of firms bunching at the wage floor to obtain an unobserved productivity parameter in each local labor market. Similarly, I invert first order conditions of the Nash bargaining problem to obtain bargaining power parameters of unions. I simulate the export shocks in the model, and verify that it is able to replicate the qualitative empirical patterns in the data.

The model allows me to assess the role of the CB network in the propagation of shocks. I define counterfactual networks that vary in the mean size of CB units, a proxy for the degree of centralization of bargaining, and simulate the effects of the export shocks. Countries with decentralized bargaining include the US and the UK, while Sweden or Norway are examples of more centralized bargaining systems (Bhuller et al., 2022). I find that the degree of shock propagation is hump-shaped in the mean size of CB units, meaning that shock propagation is larger in CB networks of intermediate size. This result can be explained by the extent of connections created by the CB network and the dispersion of productivity within CB units. Decentralized bargaining results in a high "bite" of wage floors, but trivially does not propagate shocks across local labor markets. Increasing the size of CB units leads to more connections, so shocks propagate more. It also increases the dispersion of productivity within CB units, which results in lower wage floors as unions want to prevent employment costs in low productivity local markets. Eventually, the second effect dominates, and as the wage floor "bite" declines changes in the wage floor as response to shocks become less impactful on wages. This result suggests that the degree of risk sharing of shocks between firms is higher in networks where CB units are of moderate size.

This article contributes to several strands of literature. First, to my knowledge this is the first paper to show direct evidence that the wages paid by a given firm are affected by shocks to other firms in the same CB unit. Rose (1987) and Abowd and Lemieux (1993) find wage responses to shocks to CB units in different contexts.[5] Gürtzgen (2009b) and Rusinek and Rycx (2013) find that wages are more responsive to firm shocks when bargaining is decentralized. Card and Cardoso (2022) study the association between changes in average value added in the CB unit and changes in wage floors in Portugal. None of these papers distinguish between the effects of shocks to a firm and to other firms in the same CB unit, nor do they explore the response of firms according to their productivity. Garin and Silvério (2023) study the effects of "common" and "idiosyncratic" export shocks in Portugal, but do not explore the role of CB.

My result of stronger wage responses to CB shocks than to firm shocks suggest that the different magnitudes of rent-sharing elasticities in the literature may be explained by the level at which shocks are aggregated. For example, findings in Abowd and Lemieux (1993) using contract wages from CB agreements suggest an elasticity of 0.2, twice as high as the average elasticities across worker-level specifications reported in reviews by Card et al. (2018) and Jäger et al. (2020).

Second, the paper shows that the effects of trade shocks are mediated by labor market institutions, pointing towards a novel channel for the regional contagion of shocks. This finding contributes to our understanding of the spatial effects of economic shocks, such as those arising from international trade (e.g., Autor, Dorn, & Hanson, 2013; Dix-Carneiro & Kovak, 2017; Topalova, 2010). Adão, Arkolakis, and Esposito (2022) study the role of spatial linkages in the propagation of shocks, and Felix (2022) discusses the role of local labor market concentration. Giroud and Mueller (2019) and García-Lembergman (2022) find that shocks propagate spatially through firms' internal networks. Borusyak, Dix-Carneiro, and Kovak (2022) note that the effects of regional shocks on migration depend on a region's shock and on the shocks of other regions as well. None of these papers discuss the role of CB, nor do they study how different CB networks affect the propagation of shocks.

Additionally, this is the first paper to incorporate bargaining and endogenous wage floors in a spatial economic model, allowing for varying degrees of centralization in bargaining. While prior work exists modelling

[5]Rose (1987) studies the response of wages in a deregulation episode in the US trucking industry. Abowd and Lemieux (1993) study the response of contract wages to changes in international prices in Canadian manufacturing.

bargaining institutions in different settings, it usually does not feature firm heterogeneity or spatial linkages.[6] In principle, the model can be used to study other questions in a spatial setting for which bargaining may be important, such as the connection between collective bargaining and regional inequality.

Finally, the paper contributes to a broad literature studying unions and collective bargaining (e.g., Card, 1990, 1996; Freeman & Medoff, 1984; Moene, Wallerstein, & Hoel, 1993). In particular, several papers have studied the properties of different bargaining regimes both theoretically and empirically (e.g., Boeri et al., 2021; Calmfors & Driffill, 1988; Cardoso & Portugal, 2005; Holden, 1988; Plasman, Rusinek, & Rycx, 2007). Bhuller et al. (2022) calls for "closing the gap between how economists tend to model wage setting and how wages are actually set." This paper takes a step in that direction by modeling the institutional structure of collective bargaining explicitly, and using an estimated model to evaluate the effects of different bargaining structures in a principled way.

The paper is organized as follows. Section 3.2 presents a stylized theoretical framework. Section 3.3 describes the context and data, Section 3.4 discusses the empirical strategy used to estimate the effects of shocks, and Section 3.5 dives into the results. I present a structural model of the labor market with collective bargaining in Section 3.6, discuss its estimation and validation in Section 3.7, and use it to conduct counterfactual exercises in Section 3.8. Section 3.9 concludes.

3.2 Theoretical Framework

This section presents a motivating theoretical framework with heterogeneous firms covered by a single CB unit. The model shows that shocks propagate to wages and employment through the wage floor, and that employment responses are heterogeneous across firms, yielding testable implications for the empirical analysis discussed in Section 3.4. While this section presents a stylized discussion of partial equilibrium effects under a single CB unit, the model of Section 3.6 will allow for multiple CB units and general equilibrium effects.

3.2.1 Firm heterogeneity and wage floors

Firms j face an upward-sloping labor supply curve $\ell_j(w)$ at wage $w = w_j$. In Section 3.6, ℓ_j will also depend on a measure of aggregate wages, capturing general equilibrium effects. Firms are heterogeneous in productivity φ_j, and the production function is $y_j = \varphi_j f\left(\ell_j(w_j)\right)$ for a concave $f(\cdot)$. For simplicity, the

[6] Several articles present theoretical models to study the role of bargaining. For example, Calmfors and Driffill (1988) study the relationship between centralized bargaining and the level of unemployment, and Corneo (1995) and Naylor (1998) study how the bargaining structure affects the response of national labor markets to trade integration.

output price is normalized to 1. A wage floor $\underline{w}$ sets a minimum to the wage that firms can pay. The firm's static decision problem is thus given by

$$\max_{w_j} \varphi_j f(\ell_j(w_j)) - w_j \ell_j(w_j) \quad \text{s.t.} \quad w_j \geq \underline{w}.$$

The solution (w_j^*, ℓ_j^*) is characterized by two thresholds that determine firms' behavior given their productivity (Ahlfeldt, Roth, & Seidel, 2022b), which I denote by $\underline{\varphi}$ and $\overline{\varphi}$. Firms with $\varphi_j > \overline{\varphi}$ are *unconstrained* by the wage floor. They optimally choose wages above the floor, so we can write $w_j^* = w_j(\varphi_j)$ and $\ell_j^* = \ell_j(w_j^*)$. The unconstrained wage exhibits a wage markdown over the marginal product of labor, as in monopsonistic labor markets (**Manning2011**; Card et al., 2018). The model implies that these firms will not be affected by marginal changes in the wage floor.

Firms with $\varphi_j \leq \overline{\varphi}$ are *constrained* by the wage floor, so that $w_j^* = \underline{w}$. These firms come in two varieties, depending on their productivity (Ahlfeldt, Roth, & Seidel, 2022b; Berger, Herkenhoff, & Mongey, 2022). When $\varphi_j \leq \underline{\varphi}$ firms are *demand constrained* and their optimal choice of labor is such that $d\ell_j(\underline{w})/d\underline{w} < 0$. These firms may also close down if not profitable, resulting in employment losses as well. When $\varphi_j \in (\underline{\varphi}, \overline{\varphi}]$ firms are *supply constrained* and their optimal choice of labor is such that $d\ell_j(\underline{w})/d\underline{w} > 0$.[7]

3.2.2 Nash bargaining problem

I incorporate a CB unit that determines wage floors and study the partial equilibrium effects of shocks to firms. Aggregate revenue and the wage bill are given by $R(\underline{w}) = \sum_{j \in \mathcal{J}} \varphi_j f\left(\ell_j(w_j)\right)$ and $WB(\underline{w}) = \sum_{j \in \mathcal{J}} w_j \ell_j(w_j)$, respectively, where $\mathcal{J}$ is the set of firms in the CB unit. The union objective is to maximize the wage bill $U(\underline{w}) = WB(\underline{w})$, and the employer objective to maximize aggregate profits $\Pi(\underline{w}) = R(\underline{w}) - WB(\underline{w})$. In this formulation, the union is concerned with maximizing total wages instead of worker welfare. In Argentina, unions receive a fee proportional to each payslip, so this is equivalent to assuming that unions try to maximize their income.[8]

Unions and employers within a CB unit Nash bargain over a wage floor $\underline{w}$. I assume that, in case of a floor $\underline{w}'$ that is not binding, aggregate profits are positive ($R(\underline{w}') > WB(\underline{w}')$). Additionally, the presence of firm heterogeneity implies that the wage bill is hump-shaped in the wage floor (Ahlfeldt, Roth, & Seidel,

[7]The response of constrained firms depends on the level at which their marginal revenue product of labor (MRPL) and marginal cost of labor (MCL) intersect in the equilibrium without a wage floor. For demand constrained firms the MRPL equates the MCL at a level lower than $\underline{w}$. Thus, as the MCL goes up to $\underline{w}$, these firms increase the MRPL to equate it to $\underline{w}$ by reducing employment. For supply constrained firms the MRPL equates the MCL at a level higher than $\underline{w}$. A wage floor induces these firms to lower the MRPL by increasing employment.

[8]Gürtzgen (2009a) theoretically explores the effect of different bargaining structures in an oligopoly model and similarly assumes that unions maximize the wage bill.

2022b), so there exists a $\underline{w}$ that maximizes the wage bill. Letting $\beta \in [0,1]$ be the bargaining power of the union, the Nash bargaining problem is

$$\max_{\underline{w}} U\left(\underline{w}\right)^{\beta} \Pi\left(\underline{w}\right)^{1-\beta}. \tag{3.1}$$

If $\beta = 1$ the problem amounts to selecting the wage bill-maximizing $\underline{w}$, whereas if $\beta = 0$ the problem amounts to selecting the profit-maximizing $\underline{w}$ (which will be non-binding). Now consider the case $\beta \in (0,1)$. In an internal solution the optimal wage floor $\underline{w}^*$ is implicitly given by

$$WB(\underline{w}^*) = \omega(\underline{w}^*)R(\underline{w}^*) \tag{3.2}$$

where $\omega \in (0,1)$ is a weight given by

$$\omega(\underline{w}^*) = \frac{\beta}{\beta + (1-\beta)\left(-\frac{d\Pi}{d\underline{w}}(\underline{w}^*)/\frac{dU}{d\underline{w}}(\underline{w}^*)\right)}. \tag{3.3}$$

The solution sets the wage floor so that the wage bill equals a fraction of aggregate revenue. If we divide (3.2) by total employment, and allow for an outside option for workers in the objective function U, then we would get the familiar result that average wages are a weighted average of revenue per worker and the outside option. The solution is thus analogous to the classical Nash bargaining problem in which unions set wages instead of wage floors. The main difference is the weight ω, which may differ from β. For instance, if the wage floor is more "painful" for firms relative to how "beneficial" it is for unions ($-d\Pi/d\underline{w} > dU/d\underline{w}$), then ω will be smaller.

3.2.3 Response to shocks

I now consider the response of wages and employment to shocks to firms, and how this response is mediated by the wage floor. I refer to productivity shocks, but the results apply to any shock that affects the revenue of firms, such as product demand shocks. A key feature that determines the response of wages and employment is firms' baseline productivity, which determines whether they are constrained or not. The following proposition shows how shocks affect the wage floor.

Proposition 11 (Response of wage floor to shocks)**.** *Assume a change in the productivity profile* $\{d\ln\varphi_j\}$*.*

Holding ω fixed, the resulting change in the wage floor is

$$d\ln \underline{w} = \frac{WB}{\tilde{WB}^{\text{co}}}\left[\sum_{j\in\mathcal{J}} s_j^R d\ln\varphi_j - (1-\omega)\sum_{j\in\mathcal{J}} \iota_j s_j^{WB} d\ln\varphi_j\right] \tag{3.4}$$

where $s_j^R = R_j/R_c$ is the share of revenue of firm j in the CB unit and $s_j^{WB} = WB_j/WB_c$ is the analogous wage bill share, $\tilde{WB}_c^{\text{co}}$ is the wage bill of constrained firms adjusted for its response to the wage floor, and ι_j is the elasticity of the wage bill to productivity φ_j. Allowing ω to change results in a third term that depends on the responses of the ratio $(-d\Pi/d\underline{w})/(dU/d\underline{w})$ to the changes in productivity and the wage floor.

Proofs are available in Appendix 3.A. Proposition 11 shows that $\underline{w}$ responds to a weighted average of changes in log productivity minus a weighted average of wage bill elasticities times changes in log productivity. This latter term reflects the fact that firms respond to the shock changing wages in proportion to revenue, yet revenue is down-weighted in the Nash split, so the wage bill increases "in excess." We also note that the response of $\underline{w}$ is decreasing in the adjusted constrained wage bill $\tilde{WB}_c^{\text{co}}$. As $\tilde{WB}_c^{\text{co}}$ is decreasing in β, stronger unions will result in more responsive wage floors.

The following proposition shows how wages and employment respond to shocks.

Proposition 12 (Response of wages and employment to shocks). *Assume a change in the productivity profile $\{d\ln\varphi_j\}$ that leads to the wage floor change given by (3.4).*

1. *For unconstrained firms with $\varphi > \overline{\varphi}$ we have $d\ln w_j = (dw_j(\varphi_j)/d\varphi_j)\,\varphi_j d\ln\varphi_j$ and $d\ln \ell_j = (d\ell_j(w_j)/dw_j)\,w_j d\ln w_j$. Wages and employment respond positively to shocks.*

2. *For demand constrained firms with $\varphi < \underline{\varphi}$ we have $d\ln w_j = d\ln\underline{w}$ and $d\ln\ell_j = (\partial\ell_j(\underline{w})/\partial w)\,\underline{w}d\ln\underline{w}$. Wages respond in the same direction as $\underline{w}$ and employment in the opposite direction.*

3. *For supply constrained firms $\varphi \in [\underline{\varphi}, \overline{\varphi}]$ the expressions are the same as for demand constrained firms, but the employment response is in the same direction as the wage floor.*

Proposition 12 establishes the response of wages and employment to the entire profile of shocks. The model implies that unconstrained firms should only be affected by their own shock.[9] For constrained firms, Proposition 12 implies that shocks that increase average CB revenue will unequivocally lead to higher wages. However, the effect of shocks on average employment is heterogeneous: the lowest productivity firms will respond to shocks reducing employment, those with intermediate productivity will increase employment, and the highest productivity firms will not be affected.

[9]Unconstrained firms may be affected by the wage floor if spillover effects are present. For example, Cengiz et al. (2019) in the US and Milgrom and Verdugo (2022) in Chile find evidence of spillovers from minimum wage policies.

In this section I showed that shocks propagate across firms via changes in the wage floor. The responses of firms to shocks via CB depend on their productivity, with employment responding in opposite directions for low and medium productivity firms. The empirical analysis that follows will test these predictions to establish whether the effects can be explained by the wage floor.

3.3 Context and Data

In this section I describe the labor market institutions in Argentina, the economic environment of the period under study, and the data. The goal is to provide context for the comparisons that I make in the empirical analysis and discuss the role of wage floors. I offer a succinct overview here, while Appendix 3.B provides more details.

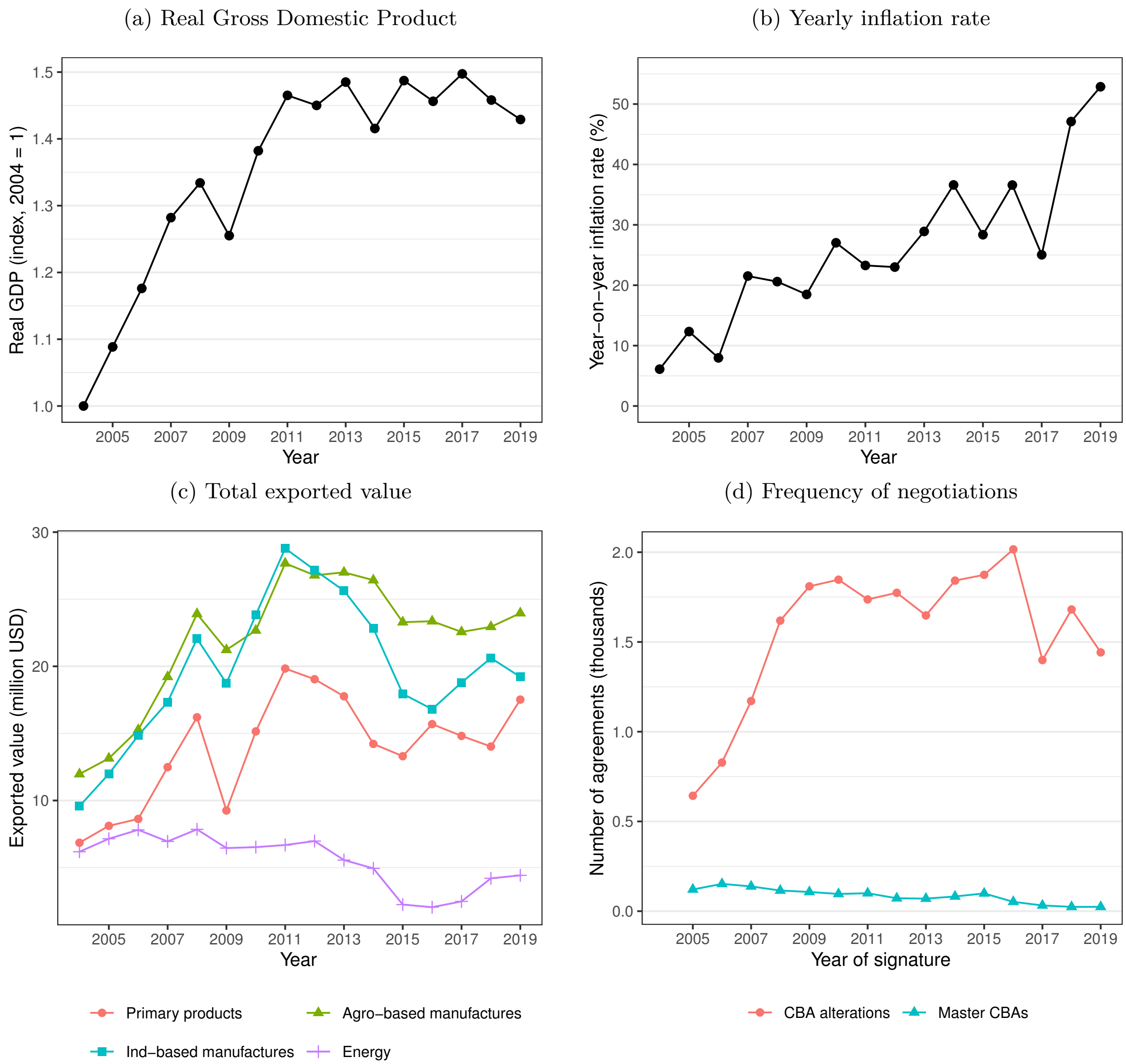
3.3.1 The socio-economic context

This subsection describes the socio-economic context surrounding the years 2009–2013, which is the period of interest for the construction of the export shocks. Figure 3.1 visualizes the evolution of the economy in 2004–2019. Panel (a) shows the real GDP and Panel (b) the inflation rate.

Exports experienced an increase in the aftermath of the Great Recession, partly due to a surge in international demand for Argentine products. Panel (c) shows that exports of manufactured products increased between 2009 and 2013, a period that—as I discuss in the next section—coincides with an increase in international demand. Exports declined afterwards due to deteriorating macroeconomic conditions such as the appreciation of the real exchange rate.

The increase in inflation prompted an increase in the frequency of negotiations to revise wages. Panel (d) shows that CBA alterations increased almost threefold between 2005 and 2010, and remained high afterwards. CBA alterations are agreements that modify some provisions in comprehensive "master CBAs," and tend to update wage floors. (I discuss the different types of CBAs in the next subsection.) The fact that nominal wage revisions are frequent turns out to be a feature of the empirical context, as *real* wage floors can potentially change significantly in response to changes in the economic conditions faced by firms within CB units.

Figure 3.1: Performance of Argentine economy, 2004–2019



Notes: The figure shows the evolution of key macroeconomic variables in Argentina for the period 2004–2019. Panel (a) shows the evolution of the real gross domestic product (GDP), using data from the National Institute of Statistics and Censuses (INDEC). The real GDP is measured in constant 2004 Argentine pesos and normalized to 1 in 2004. Panel (b) shows the yearly inflation rate constructed from INDEC, and using data from regional statistics offices for the period 2007–2015. The inflation rate is measured as the yearly percentage change in the consumer price index as of December of each year. Panel (c) shows the total exported value, using data from INDEC. The total exported value is measured in millions of current US dollars. Panel (d) shows the number of collective bargaining agreements (CBAs) signed each year, using data obtained from the public archive of Collective Bargaining Agreements.

3.3.2 Labor market institutions in Argentina

The Argentine law sets general regulatory standards for workers, and various CB agreements (or CBAs) establish additional regulations for specific worker subgroups. The law stipulates that individual employers cannot modify the terms of the CBAs to the detriment of workers, but can improve upon them. Consequently, CBAs establish minimum standards for covered workers.

The government grants bargaining privileges to a single union per "area of representation," defining the scope of CBAs. These areas may be defined by industry, occupation, geography, or a single employer. Only unions with privileges can sign either one of two types of agreements. First, a "master CBA" is a comprehensive contract that sets standards for all workers, and defines the CB units.[10] Second, "CBA alterations" act as amendments to master CBAs and are signed more frequently. Usually, national master CBAs allow for a proportional difference in wage floors across regions (most typically for southern provinces) that is determined in the master CBA and thus is constant over time. Additionally, the law allows a single employer to hire employees under multiple CBAs, a scenario that applies to around 16% of firms.

CBAs are extended to all workers and firms in a given area of representation, resulting in nearly universal CB coverage. Managerial positions and specific occupations are usually excluded. In 2014, of a total 6.4 million formal workers, 80% were covered by CBAs under the main private sector bargaining regime, 7% were not covered by any CBA, and the remaining workers were under special regimes (Ministerio de Trabajo, Empleo y Seguridad Social, 2023).

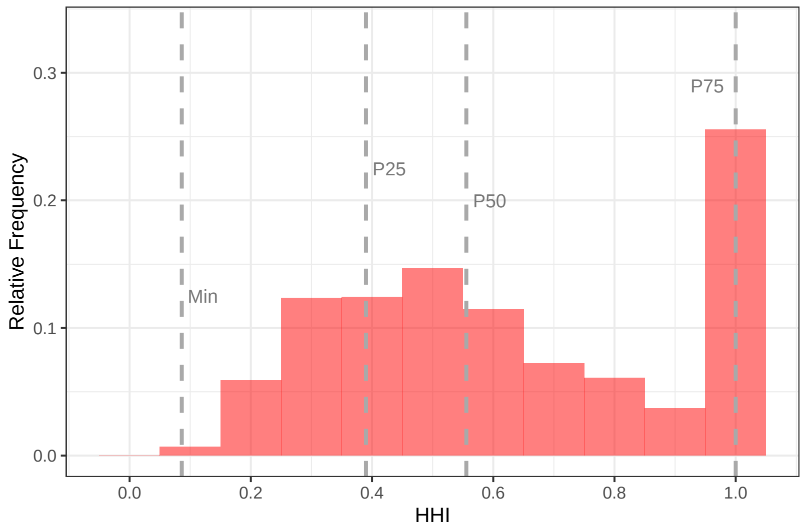
Heterogeneity in CB coverage. The legal structure results in an idiosyncratic CB network in which firms in the same economic sector and province usually operate under different CB units. One reason is that the definition of coverage of master CBAs, which follows from the union's area of representation, is based on inexact verbal descriptions. As an example, Appendix Table 3.B shows the description of three textile CBAs. It is hard to tell from the descriptions what CBA would apply to a given sector within textile manufacturing, such as "weaving of textiles" or "manufacture of wearing apparel." For instance, CBA 0500/07 covers "workers in the textile industry" and 0501/07 covers "workers ... from clothing industrial companies and related fields."

Figure 3.2 illustrates the heterogeneity in CB coverage, using the primary CBA in the firm (as defined later in this section). The figure shows the concentration of firms across different CB units within 4-digit sector and province cells using an HHI index. An exact alignment between CB units and sector-province cells

[10] A master CBA may be modified by a new master CBA that supersedes it, or a CBA alteration that updates some provisions in it. Expired master CBAs remain in force until renegotiated due to a clause known as "ultra-activity."

would result in an HHI of 1 for all cells. However, we observe a large dispersion of HHIs. 40% of cells have an HHI lower than 0.5, equivalent to 2 equal-sized CB units in the cell. As a concrete example, Appendix Figure 3.E shows the proportion of firms in different CB units in three textile-related economic sectors. The CB units described in Appendix Table 3.B appear in all sectors.

Figure 3.2: Concentration of firms in different CB units within 4-digit sector and province cells



Notes: Data include all firms that had positive employment in 2012 and declared an economic sector in "Manufacturing" or "Wholesale and Retail Trade". The figure shows the distribution of the Herfindahl-Hirschman Index (HHI) of the number of firms across different collective bargaining units within 4-digit sector and province cells. The index is computed as $HHI_\ell = \sum_c h_{c\ell}^2$, where $h_{c\ell}$ is the share of firms in CB unit c within the given sector by province cell ℓ. In case of a firm with multiple CB units I assign it to the one with the highest number of employees.

3.3.3 Data

I obtain information on labor market outcomes from two administrative registries. The first one is Argentina's matched employer-employee dataset, covering 2007–2020. The key variable of interest is total monthly compensation. The data also include the worker's gender and age, the hiring modality for the job, and the firm's fiscal location (postal code) and 6-digit economic sector (built from ISIC codes).[11] Importantly, the data do not include hours or full-time status, though I use the hiring modality to proxy for the latter. Second, I use data from *Simplificación Registral*, a national system introduced in 2008. The dataset contains information at worker hiring and termination dates, and includes the (master) CBA code, the category within the CBA, and an occupation code. I join these datasets using firm and worker identifiers.

Upon cleaning the CBA code variable, I assign a "primary CBA" to each firm, which defines its CB

[11] A firm's fiscal postal code may not coincide with its production site. However, fiscal provinces are likely to coincide, which is why I use this geography in my empirical analysis.

unit.[12] I do so by selecting the most common CBA code in the firm. Sometimes all workers in a firm have a missing CBA code. In this case I assign the primary CBA based on the firm's location and economic sector. All firms in the economy are assigned to a CB unit.

I use two additional data sources for the empirical analysis. First, to construct export shocks I rely on international trade flows data from BACI-CEPII (Gaulier & Zignago, 2010) for 2007–2020, and data from Argentine Customs for 2011–2020. I harmonize product and country codes to match the datasets. Second, I use a survey of businesses conducted jointly by the Ministry of Labor and the Ministry of Science to study the effect of shocks on firm revenue and expenditures.

Estimating wage floors. While the administrative data provides a CBA code and a within-CBA occupation code, it does not directly include wage floors.[13] I estimate wage floors by detecting bunching in the distribution of total wages within CBAs, categories, CBA-regions, and month cells, starting in 2011 when the CBA codes are more reliable. Then, I smooth the wage floors so that the percent difference across categories is constant and all categories experience the same growth over time, reflecting the usual structure of CBAs. I obtain $243,400$ monthly wage floors between 2011 and 2017, corresponding to 450 CBAs and $2,619$ CBA-occupations.[14]

The estimated wage floors align well with a sample of manually collected floors. The correlation between the levels of the estimated and manually collected wage floors is 0.87. However, the wage floors collected manually exhibit on average somewhat larger first differences. While measurement error is likely present, there is no reason to suspect it would be correlated with the export shocks. Appendix 3.C discusses the details of the estimation and validation of the wage floors.

Wage floors are strongly binding in the labor market. Appendix Figure 3.F shows the distribution of the wage to wage floor ratio for workers in the main hiring modality in 2012. The average ratio is 1.28 and 47.1% of workers are below the value of 1.1, though these numbers vary by CBA. Cardoso and Portugal (2005) pioneer the usage of the "wage cushion," defined as log ratio of the wage and the wage floor, to measure wage flexibility under CB. Card and Cardoso (2022) find an average wage cushion 0.20 in Portugal, and Bhuller et al. (2022) of 0.15 in Norway.

[12] I take several steps to impute missing values for the CBA code, increasing coverage from 70% to 85% after 2011. The most important step involves imputing the CBA to workers with missing values in a firm that only declares a single CBA. I also harmonized master CBA codes that change over time. Appendix 3.B.2.3 provides details.

[13] A dataset containing wage floors is not readily available. Acquiring this information typically requires reviewing the text of CBAs, usually available in scanned PDF format, making manual collection of wage floors impractical.

[14] I exclude June and December, as these months correspond to the 13th-month salary payments.

A yearly panel of firms. For my main analysis, I create a panel dataset at the firm-year level for the period 2007–2017, and for 2011–2017 for wage floors. I compute the average monthly real wage and wage floor for each firm-year, deflating monthly wages by the consumer price index. I also tally the number of workers that appear in a firm in the year and compute the share of workers in the main hiring modality (which proxies for the share of full-time workers). I detail the construction of the export shocks and the baseline analysis sample in the next section.

3.4 Empirical Strategy

In this section, I present the empirical strategy used to study the propagation of shocks through collective bargaining units. The strategy leverages fluctuations in world import demand for granular products to construct economic shocks at the firm and CB unit levels. Then, I use these shocks in a difference-in-differences strategy. Additionally, I discuss the construction of the baseline analysis sample and the identification assumptions underlying the analysis.

3.4.1 Overview

This paper aims to uncover the causal effects of a shock to the firm's product demand and to the product demand of a firm's peers in the CB unit. An ideal experiment to identify these effects would consist of randomly changing the demand for different granular products to generate exogenous variation in the total product demand to each firm. The effect of a product demand change at the firm could be estimated by comparing firms with different changes, conditional on the changes to the firm's peers. Section 3.2 suggests that bargaining outcomes are determined by average conditions across firms in the CB unit. As such, the experiment should generate variation in the average of the firm-level changes across CB units. Then, conditioning on the product demand change of the firm, the effect of the average product demand change at the CB unit could be estimated comparing firms across CB units.

To approximate this experiment I construct shocks to firm and CB units using variation arising from changes in world import demand for granular products exported by Argentine firms. I then rely on a difference-in-differences strategy to estimate the effects of these shocks on firm outcomes. I detail this strategy in the remainder of this section.

3.4.2 Identifying trade shocks

Building on Hummels et al. (2014) and recent shift-share literature (Borusyak, Hull, & Jaravel, 2022), I use variation in international demand for country-products exported by Argentine firms interacted with exposure shares to construct shocks at the firm and CB unit levels. Let WID_{pt} denote the world import demand of country-product $p \in \mathcal{P}$ from the world (excluding Argentina) in year t, where $\mathcal{P}$ is the set of country-products. The country-product level shock is

$$f_p = \frac{1}{2}\sum_{t=2012}^{2013} \ln WID_{pt} - \frac{1}{2}\sum_{t=2009}^{2010} \ln WID_{pt}.$$

I then define a shock to each firm j as

$$z_j = \sum_{p\in\mathcal{P}} s_{jp} f_p,$$

where $s_{jp} = EX_{pj}/\left(\sum_{p'\in\mathcal{P}} EX_{p'j}\right)$ and EX_{pj} is the sum of the value exported to country-product p in 2011 and 2012. Similarly, I define a shock to each CB unit c as

$$z_c = \sum_{j\in\mathcal{J}} s_{cj} z_j = \sum_{j\in\mathcal{J}} s_{cj} \left(\sum_{p\in\mathcal{P}} s_{jp} f_p\right) = \sum_{p\in\mathcal{P}} s_{cp} f_p.$$

where $s_{cj} = L^E X_{cj}/\left(\sum_{j'\in\mathcal{J}} L^E X_{cj'}\right)$ is the share of workers in exporting firm j in CB unit c, and $s_{cp} = \sum_j s_{cj} s_{jp}$ denotes the contribution of p to c's shock.[15] The CB shock z_c is the average product demand change across firms in the CB unit. As such, one would expect it to shift average revenue in the CB unit, which according to Section 3.2 should determine changes in wage floors.

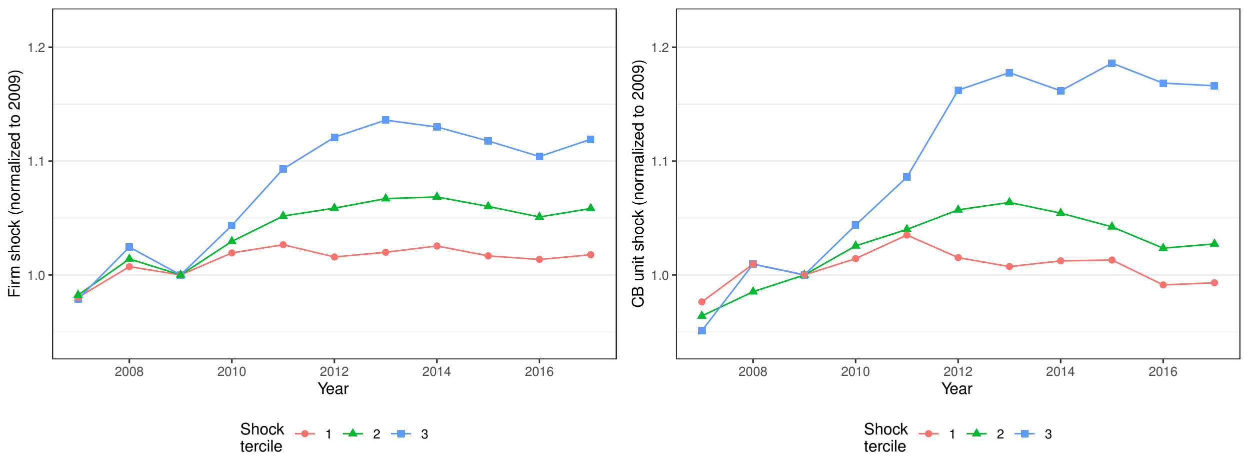
The data suggest an exogenous shift in international demand for different products following the Great Recession that translated into heterogeneous shocks to firms and CB units. To see this I compute time-varying versions of the shocks using changes in WID relative to 2009. Figure 3.3 visualizes the evolution of the average shock for different levels of z_j and z_c. Panel (a) presents the firm shocks and Panel (b) the CB shocks. We observe stable trends that start to diverge around 2010. I discuss evidence supporting shock exogeneity in the next subsection.

Baseline analysis sample. To ensure that outliers do not drive the results, I define a "baseline sample" of exporting firms that incorporates several restrictions. First, I keep firms that were operational in 2007 through 2009, had an average of 1 to 500 workers in the same period, and their value exported in 2011–2012

[15]Shocks are not defined for CB units without exporting firms, which are excluded from the analysis.

Figure 3.3: Evolution of export shocks to firms and collective bargaining units

(a) Shocks to firms (b) Shocks to CB units



Notes: Data are from the baseline sample of exporting firms. The figure illustrates the evolution of time-varying firm and CB shocks, for different levels of the static firm and CB shocks. Each line depicts the average of the time-varying shock in a given tercile of the distribution of the static shock. The time-varying versions of the shocks are constructed as averages in world import demand in a given year, weighting by appropriate exposure shares, and then normalized by the 2009 value. The static shocks are constructed from the average change in world import demand for a given country-product between 2009–2010 and 2012–2013, also weighted by exposure shares. Firm exposure shares are equal to the share of a firm's exports in a given country-product. CB exposure shares are defined as the sum across firms of the employment share of a firm in the CB unit times the firm's value exported share. The averages in Panel (b) are weighted by the number of firms in a given CB unit.

was within the 1st and 99th percentiles. I drop firms that experienced a change in average world import demand between 2007–2008 and 2009–2010 in the bottom 1% or top 1% of the distribution as well.[16] Second, I exclude firms in CB units with less than 5 exporting firms in 2011–2012, and CB units with shocks falling within the bottom or top 1% of the distribution. I discuss the robustness of the results to these restrictions.

Appendix Table 3.C shows cross-sectional statistics of the baseline sample. The sample contains $7,972$ firms, spanning 222 4-digit and 467 6-digit sectors. In the pre-period the average firm has 46.0 employees, and 23.5% of firms have less than 10 workers. Appendix Table 3.D shows cross-sectional statistics of the 174 CB units that cover these firms. Lastly, Appendix Table 3.E shows statistics of the main panel of firms used for estimation, spanning from 2007 to 2017.

Appendix Figure 3.G shows the distribution of both z_j and z_c in the baseline sample. The distribution of the firm shock is bell-shaped around a positive mean. The variation in CB shocks is somewhat larger, suggesting some within-CB unit correlation of firm shocks.

[16] As a result of matching imperfections between Argentine customs data and international trade flows data, 0.81 percent of exporting firms in 2011–2012 have less than 99 percent of their exporting value matched to a country-product (see Appendix 3.B.2.2 for details). In the terminology of Borusyak, Hull, and Jaravel (2022), these firms have "incomplete shares". I drop these firms from the analysis.

3.4.3 A difference-in-differences strategy

Let $I\{\cdot\}$ be an indicator function. The static difference-in-differences (DiD) model is

$$y_{jt} = \theta z_{c(j)} I\{t \geq 2012\} + \lambda z_j I\{t \geq 2012\} + \alpha_j + X_j' \psi_t + \delta_{\ell(j)t} + \varepsilon_{jt}. \tag{3.5}$$

where y_{jt} is firm j's outcome in year t, $c(j)$ indicates the CB unit of firm j, and z_c and z_j are the CB and firm shocks, respectively. The parameters of interest, λ and θ, can be interpreted as the effect of an increase in the average product demand at each level on the evolution of y_{jt}. I study the effect of the firm shock on revenue in a sample of firms observed in a survey of businesses to interpret the magnitude of these effects in terms of revenue as well.

The model controls for several potential confounders. First, I include firm effects α_j to control for time-invariant firm characteristics. Second, I include baseline characteristics X_j interacted with time to control for time-varying factors that may affect different firms differently, such as the increase in the federal minimum wage in 2007–2011. In particular, X_j consists of pre-period firm size and wage level dummies interacted with economic sector dummies.[17] Third, I include local labor market by year fixed effects $\delta_{\ell(j)t}$, where $\ell(j)$ indicates j's local market, to control for common trends in the local labor market. As baseline, I define local labor markets as the interaction between provinces and 4-digit ISIC sectors, though I vary this definition in robustness checks. I further show that the results are robust to different sets of controls.

To study treatment effect dynamics I use a specification analogous to (3.5) that includes interactions of year dummies with the shocks. Formally,

$$y_{jt} = \sum_{s \in \mathcal{S}} \theta_s z_{c(j)} I\{t = s\} + \sum_{s \in \mathcal{S}} \lambda_s z_j I\{t = s\} + \alpha_j + \delta_{\ell(j)t} + X_j' \psi_t + \varepsilon_{jt},$$

where $\mathcal{S}$ is the set of years from 2007 to 2017. Since Figure 3.4 suggests that effects start in 2011, I exclude 2010 from $\mathcal{S}$. I exclude 2011 for the wage floor as the wage floor data starts in that year. Throughout the paper, I cluster standard errors at the CB unit level.

Estimation of these models requires independent variation in each shock. Appendix Figure 3.H plots the firm shock versus the CB shock for each firm. Panel (a) focuses on the raw data, and Panel (b) shows the same plot after residualizing on local labor market fixed effects. There is a small positive correlation in the raw data, which is somewhat smaller after controlling for local labor market effects.[18] Overall, there is

[17] More precisely, I define two categorical variables using 2007–2009 averaged data: one for firm size (1–19, 20–124, 125–500) and one for the wage level (above or below the median wage).

[18] The raw correlation is slightly stronger when I focus on larger firms. This is to be expected given that shocks to CB units

significant independent variation in each of the shocks.

Identification. Borusyak, Hull, and Jaravel (2022) demonstrate that a shift-share model can be cast in terms of the shifting variable and present assumptions on the assignment process of the "shifts" (in my case, world import demand shocks for each country-product pair) that are sufficient for identification. Appendix 3.D develops the identification argument from Borusyak, Hull, and Jaravel (2022) in my setting, extending it to a scenario with two shift-share variables.

The key assumption is that changes in world import demand for each country-product p are quasi-randomly assigned with respect to average residuals of firms that export in that country-product, both when weighted by exposure via firms and via CB units.[19] This assumption would be violated if, for example, firms that are growing select into products in which world import demand is growing. The assumption that shocks are quasi-randomly assigned implies that shocks should not be correlated with pre-period observables.

Is the assumption of quasi-random assignment of shocks plausible? As argued by Garin and Silvério (2023), the 2009 international crisis generated unexpected changes in world import demand for different products that should be uncorrelated with pre-determined economic conditions. To explore this idea, Appendix Figure 3.I shows estimates of firm-level regressions of a pre-period outcome on the firm shock (from years 2007–2009). Using the baseline sample and the firm controls I find that the firm shock does not predict the pre-period firm shock, the firm size, the mean wage among workers, the share of workers in the main hiring modality, and an indicator for the retail CB unit 0130/75. These results support the quasi-randomness assumption. Results are similar when using all firms and no controls, except for the pre-period firm shock. Panel (a) of Appendix Figure 3.J shows that the apparent negative auto-correlation in the firm shock is mainly driven by mean reversion at the tails of the distribution, in particular the left tail.[20] Panel (b) shows that this correlation disappears once I drop firms with extreme pre-period shocks in the baseline sample.

The quasi-randomness assumption implies that CB-level shocks should not be correlated with pre-determined CB-level outcomes. Appendix Figure 3.K shows that the CB shocks are not correlated with the pre-period CB shock, the number of firms in the CB unit, and the share of employment in exporting firms.

Panel event-study. To estimate the effects of firm shocks on revenue I rely on the survey of businesses, which has fewer observations only in the years 2010–2012 and 2014–2016, and a panel event-study design in

are defined as employment-weighted averages of firms shocks.

[19]A condition for this interpretation to hold is that the shares in the definition of the shift-share variables sum to 1 (Borusyak, Hull, & Jaravel, 2022). By construction, this holds for both z_j and z_c.

[20]Using similar data for Portugal, Garin and Silvério (2023, Figure A.4) also finds evidence of mean reversion at the left of the distribution of export shocks.

the spirit of Freyaldenhoven et al. (forthcoming). This estimation strategy has two advantages. First, it uses variation in world import demand beyond the change between 2009 and 2013, leveraging more observations in the smaller survey sample of firms. Second, it estimates pre-period coefficients asking whether future shocks are correlated with current outcomes, meaning that outcome data from the pre-period are not needed to estimate pre-period coefficients.

3.5 Empirical Results

This section presents the main empirical results of the paper. First, I explore the effects of both the CB shock and the firm shock on wages and wage floors. Second, I explore the effect of shocks on employment, and use heterogeneity analysis to study whether responses to CB shocks can be rationalized by a wage floor mechanism. Third, I compare my estimates with the literature. Finally, I present a set of falsification tests and robustness checks.

3.5.1 Effects on wages and wage floors

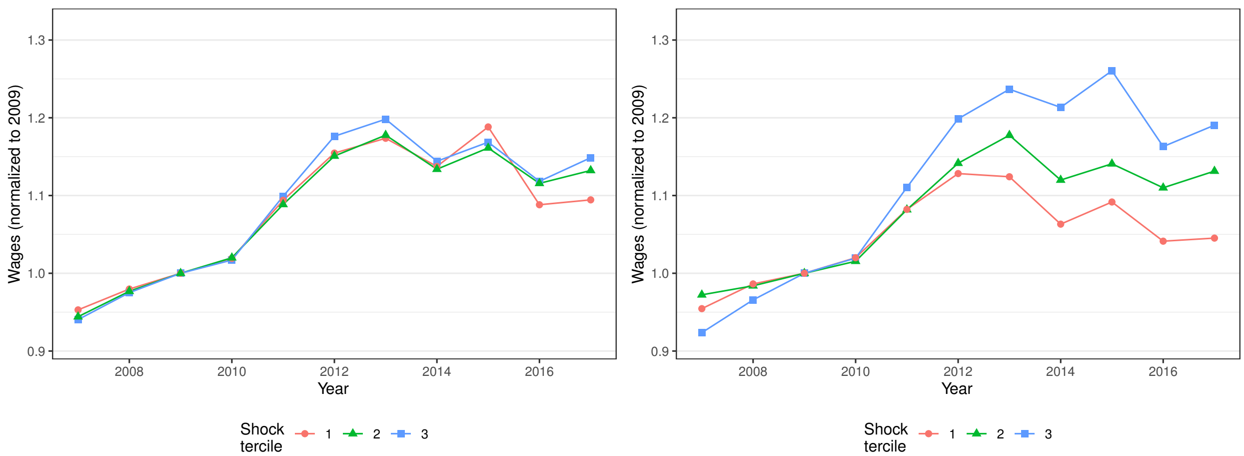
The raw data suggest stronger effects of CB shocks on wages than of firm shocks. Figure 3.4 visualizes the average evolution of wages by level of the CB shock and firm shock, relative to 2009. CB units with larger shocks consistently experience larger wage increases, whereas there seems to be a small increase in wages, if any, following a firm shock. The figure also shows small divergences between firms before the Great Recession, suggestive of parallel trends before the shocks. Next, I ask whether these patterns are robust to controls using the DiD model.

The estimates of the dynamic DiD model indicate that both mean wages and mean wage floors increase in response to a CB shock, and that mean wages increase in response to a firm shock. Figure 3.5 shows the estimates. Panel (a) shows a strong and stable increase in wages following a CB shock, and a smaller and marginally significant increase as response to a firm shock. We cannot reject that pre-trends are zero in anticipation of either shock, consistent with the parallel trends assumption. Panel (b) shows estimates of the same model but using the log mean wage floor as outcome. As discussed in Section 3.3.3, the wage floor is only available since 2011, so this model uses 2011 as omitted year. We observe a strong increase in wage floors following a CB shock, of a similar magnitude as the increase in wages, and a precisely estimated null effect of the firm shock. This is reassuring as firm shocks are not supposed to affect wage floors.

I summarize the magnitude of the estimates using a static DiD model. Column (1) of Table 3.1 shows

Figure 3.4: Evolution of wages by level of CB unit and firm shock, baseline sample

(a) Shocks to firms (b) Shocks to CB units



Notes: Data are from the baseline sample of exporting firms. The figure shows the average evolution of mean wages for firms in different terciles of the distribution of the firm shock (Panel a) and the CB shock (Panel b), relative to 2009. The firm and CB shocks are defined as the average changes in world import demand between 2009–2010 and 2012–2013, weighting by appropriate exposure shares.

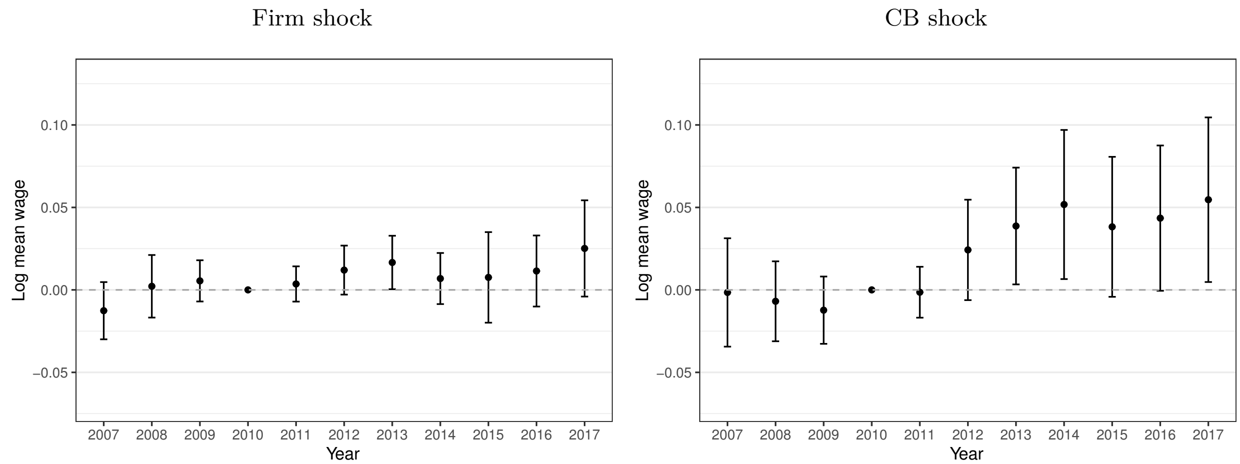
that an estimate of the elasticity of wages to the average world import demand in the CB unit of 0.0458, and to the average world import demand in the firm of 0.0134. While the coefficient on the CB shock is strongly significant ($t = 2.27$), the coefficient on the firm shock is only marginally so ($t = 1.92$). Column (2) of Table 3.1 shows an effect of the CB shock on log mean wage floors of 0.0541 ($t = 2.54$), using only observations with a valid wage floor value. Column (3) of Table 3.1 shows the effect of the CB shock on the "wage cushion" (the log ratio between the wage and wage floor), which is indistinguishable from zero.

The fact that wages and wage floors respond similarly to a CB shock suggests a pass-through of wage floors to wages close to 100%. Card and Cardoso (2022) estimate the effect of changes in wage floors on wages in Portugal, finding a smaller pass-through of about 50%. The authors do not use exogenous variation in wage floors. My estimates suggest that, in Argentina, wage negotiations in a CB unit serve as a reference point for wage-setting even from firms that pay wages higher than the floor. Another possibility is that other components of pay are affected by the CB shock, such as productivity bonuses, leading to the impression of a large pass-through.

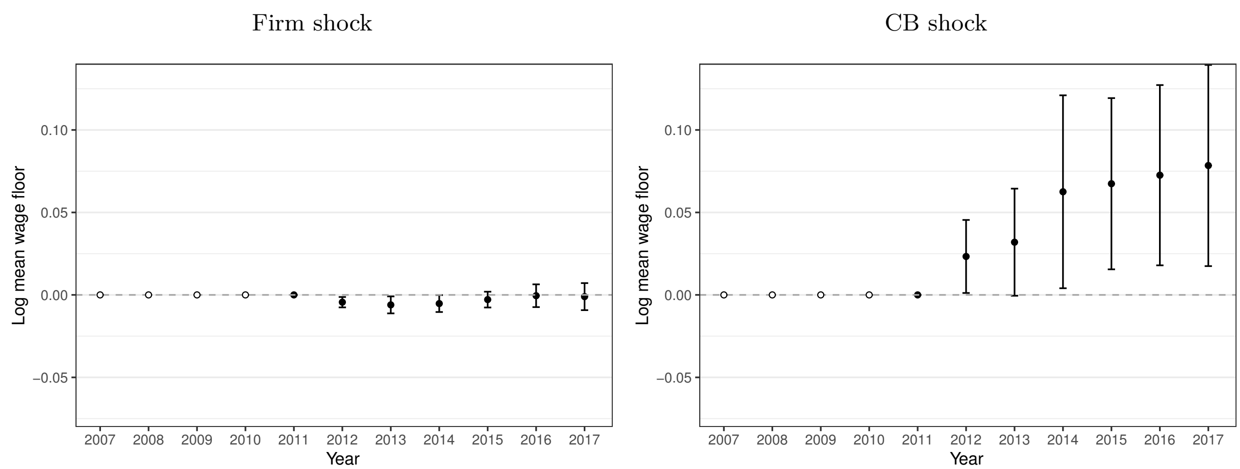
The magnitude of effects. To better interpret the magnitude of these effects, I scale them by the effect of the firm shock on revenue. Appendix 3.E discusses estimates of the firm shock on revenue and expenditures using a panel event-study estimation strategy. I obtain that a 10% increase in average world import demand raises product market sales by 1.13%. The results show null pre-trends, consistent with the identification assumption. I also show estimates of the effect of the firm shock on labor expenditures, which also increase

Figure 3.5: Effect of export shocks on mean wages and mean wage floors

(a) Log mean wages



(b) Log mean wage floors



Notes: Data are from the baseline sample of exporting firms. The figure shows the dynamic effects of firm and CB shocks on log mean wages and log mean wage floors, interacting the shocks with year dummies. The regression omits the year 2010 for the wage variable, and the year 2011 for the wage floor variable. The regression includes firm fixed effects, 4-digit economic sector by province by year fixed effects, time-varying firm controls, and a similar CB shock for the pre-period interacted with year dummies. Firm controls consist of a firm size indicator (categories 1-9, 10-24, 25-99, and 100-500) interacted with 2-digit sector and year and the pre-period share of workers in the main hiring modality interacted with year. The firm and CB shocks are defined as the average changes in world import demand between 2009–2010 and 2012–2013, weighting by appropriate exposure shares. Standard errors are clustered at the CB unit level.

but by a smaller magnitude.

Following these results, assume that a 10% change in z_j translates into a 1.1% increase in firm revenue. Then, a 10% CB shock can be interpreted as a 1.1% increase in (weighted) average firm revenue in the CB unit. Using the results in Table 3.1, this implies that a 10% increase in revenue of all firms in the CB unit increases wages by $10 \times (0.0458/0.11) \approx 4.2\%$. Similarly, a 10% increase in firm revenue increases wages by $10 \times (0.0134/0.11) \approx 1.2\%$.

Table 3.1: Static difference-in-differences estimates

	Log mean wage	Log mean wage floor	Log wage cushion	Log employment	Sh. main modality	Firm exit
	(1)	(2)	(3)	(4)	(5)	(6)
CB shock × Post	0.0458	0.0541	-0.0001	-0.0233	0.0162	-0.0050
	(0.0202)	(0.0213)	(0.0171)	(0.0351)	(0.0118)	(0.0077)
Firm shock × Post	0.0134	-0.0036	0.0100	0.0297	0.0027	-0.0008
	(0.0070)	(0.0021)	(0.0079)	(0.0210)	(0.0071)	(0.0040)
Firm FE	Y	Y	Y	Y	Y	Y
Local market-year FE	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y
Pre-period CB shock	Y	Y	Y	Y	Y	Y
Num. firms	7,972	7,654	7,654	7,972	7,972	7,972
Num. fixed effects	27,976	19,860	19,860	27,976	28,031	28,234
Num. observations	85,777	50,703	50,699	85,777	86,238	87,692
Adjusted R^2	0.8480	0.9253	0.8266	0.8965	0.5851	0.3212

Notes: Data are from the baseline sample of exporting firms. The table show regression coefficients on the firm and CB shocks variables interacted with an indicator for year greater than or equal to 2012. The firm and CB shocks are defined as the average changes in world import demand between 2009–2010 and 2012–2013, weighting by appropriate exposure shares. The regression includes firm fixed effects, 4-digit economic sector by province by year fixed effects, firm controls, and a similar CB shock for the pre-period interacted with year dummies. Firm controls consist of the following fixed effects interacted: firm size categories (categories 1-19, 20-124, 125-500) by an indicator for above median wages in 2007–2009 by a 4-digit economic sector by year. Standard errors are clustered at the CB unit level.

Heterogeneity by CB unit size. A natural question is whether the effects differ across CB units. Estimates in Table 3.2 interact the CB shock with an indicator for a large CB unit, defined as having more firms than the median number of firms in the 4-digit sector by province cell. These estimates exclude firms in cells with less than one CB unit. Columns (1) through (4) show that the wage effects of CB shocks are driven by larger CB units, suggesting that they are more effective at raising wages in response to shocks.

3.5.2 Effects on employment

Trends in the raw data are not as clear for employment as they are for wages. Appendix Figure 3.L shows the evolution of employment by level of the CB shock and firm shock, relative to 2009. We observe lower employment growth in firms that experienced a low CB shock, and stronger growth in firms with a high firm shock.

Figure 3.6 shows the dynamic DiD estimates. Panel (a) shows a clear increase in employment following a firm shock, while Panel (b) reveals a noisy, statistically indistinguishable from zero, response of employment to the CB shock. The theory discussed in Section 3.2 suggests that this is to be expected, as firms with different productivity levels will systematically respond differently to a wage floor. Table 3.1 shows estimates of the static DiD model. Column (4) shows the response of employment, which is positive and marginally

Table 3.2: Static difference-in-differences estimates, by size of CB unit

	Log mean wage		Log mean wage floor		Log employment	
	(1)	(2)	(3)	(4)	(5)	(6)
CB shock	0.0477		0.0385		0.0072	
	(0.0222)		(0.0241)		(0.0358)	
CB shock × Small CB		-0.0011		0.0023		-0.0266
		(0.0206)		(0.0256)		(0.0419)
CB shock × Large CB		0.1063		0.0911		0.0702
		(0.0265)		(0.0290)		(0.0521)
p-value equality		0.0021		0.0235		0.1671
Firm FE	Y	Y	Y	Y	Y	Y
Firm shock	Y	Y	Y	Y	Y	Y
Local market-CB size-year FE	Y	Y	Y	Y	Y	Y
Pre-period CB shock	Y	Y	Y	Y	Y	Y
Num. firms	7,013	7,013	6,741	6,741	7,013	7,013
Num. fixed effects	23,772	16,125	17,047	12,316	23,772	16,125
Num. observations	75,529	75,529	44,709	44,709	75,529	75,529
Adjusted R^2	0.8533	0.8519	0.9322	0.9351	0.8979	0.8979

Notes: Data are from the baseline sample of exporting firms, excluding 4-digit sector by province cells with less one CB unit. The table show regression coefficients on the CB shocks variable interacted with an indicator for year greater than or equal to 2012. Columns (1), (3), and (5) replicate the baseline results. Columns (2), (4), and (6) further interact the CB shock variable with an indicator for CB unit size. A "small" CB unit is defined as one with fewer than the median number of firms in the 4-digit sector by province cell. A "large" CB unit is defined as one with more than the median number of firms in the 4-digit sector by province cell. The regression controls for the firm shock, and includes firm fixed effects, CB size by 4-digit economic sector by province by year fixed effects, and a similar CB shock for the pre-period interacted with year dummies. Standard errors are clustered at the CB unit level.

significant for the firm shock ($t = 1.42$), and indistinguishable from zero for the CB shock ($t = -0.66$).[21] Columns (5) and (6) show that neither shock seems to affect the share of workers in the main hiring modality nor firm exit.

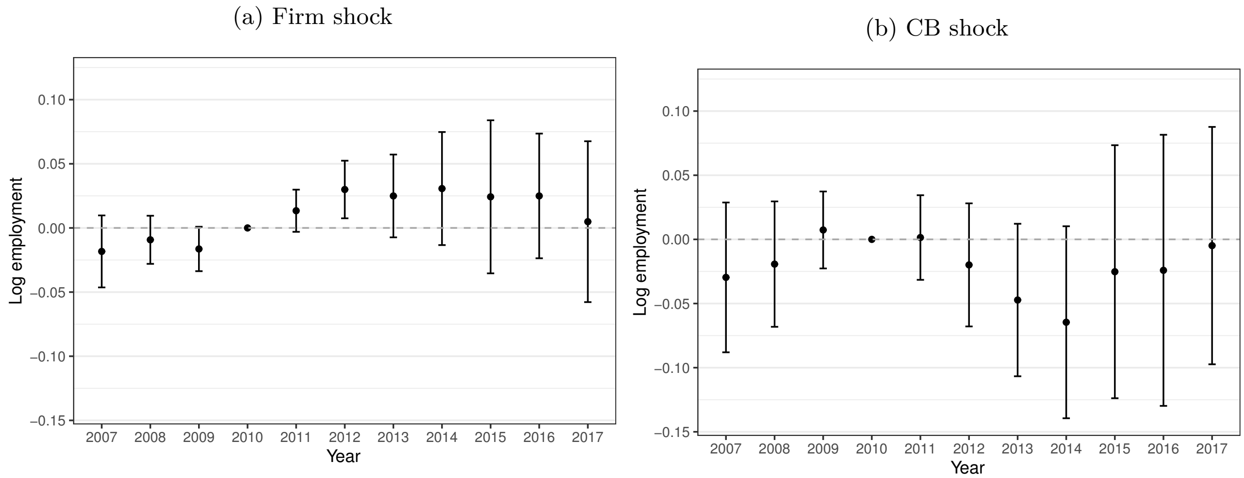
Heterogeneity by CB unit size. Columns (5) and (6) of Table 3.2 show that the effect of CB shocks on employment is not statistically different from zero for either small or large CB units.

3.5.3 Responses to CB shocks by firm productivity

A threat to the interpretation that CB negotiations are responsible for observed CB effects is that these are actually driven by unobserved shocks to product demand that are correlated with CB shocks, such as a concurrent increases in local demand. If this is the case, then a positive CB shock should increase both wages and employment across different types of firms. However, as discussed in Section 3.2, a positive CB shock that affects wage floors should cause low-productivity firms, which are constrained by the wage floor,

[21] The non-significant effect of the firm shock on employment is driven by the fact that coefficients start to increase in 2010. The years 2010–2011, which are in the pre-period in the static model, bias the estimate towards zero.

Figure 3.6: Effect of export shocks on employment



Notes: Data are from the baseline sample of exporting firms, including only firm-years for which wage floors are available. The figure shows the dynamic effects of firm and CB shocks on log employment, interacting the shocks with year dummies and omitting the year 2010. The regression includes firm fixed effects, 4-digit economic sector by province by year fixed effects, time-varying firm controls, and a similar CB shock for the pre-period interacted with year dummies. Firm controls consist of a firm size indicator (categories 1-9, 10-24, 25-99, and 100-500) interacted with 2-digit sector and year and the pre-period share of workers in the main hiring modality interacted with year. The firm and CB shocks are defined as the average changes in world import demand between 2009–2010 and 2012–2013, weighting by appropriate exposure shares. Standard errors are clustered at the CB unit level.

to decrease employment.

To differentiate these hypotheses I explore the response of firms with varying productivity levels to CB shocks. I employ a static DiD model as in (3.5), but I interact the treatment variables with a proxy for firm productivity. In particular, to account for regional differences in cost of living I construct quartiles of the distribution of average wages in 2007–2009 within each province, excluding provinces with less than 6 firms. Appendix Figure 3.M shows that the estimates are similar when using a different definition of quartiles. I interact the local labor market effects with quartile indicators, as a result I compare firms with similar pre-period level of wages.

The evidence of the wage effects of CB shocks is consistent with the hypothesis that they operate mainly through wage floors. Table 3.3 shows the results. Because I drop firms in local labor markets with less than 6 firms and I increase the number of fixed effects, the estimates are noisier. Column (1) shows that the effects of the CB shock on wages are declining in the pre-period level of wages, and column (2) shows that this is not the case for the wage floor. This is consistent with a wage floor that is more binding for low-wage firms. Column (3) shows suggestive evidence that the wage cushion increases in low-wage firms and decreases in high-wage firms, although the coefficients are not statistically different from zero.

The evidence on employment effects is consistent with the presence of wage floors as well. Column (4) shows that the effect of the CB shock on employment is negative and statistically significant for firms in the lowest quartile of pre-period wages, and positive and marginally significant for firms in the third quartile. This is consistent with low-wage firms moving along the labor demand curve and with reductions in monopsony power for firms in the third quartile. The point estimate for the second quartile is positive and for the fourth quartile is negative, but neither is statistically significant. The hypothesis of joint significance of coefficients can be confidently rejected ($p = 0.0003$). Column (5) shows positive and significant effects of the CB shock on the share of workers hired in the main hiring modality for firms in the second and third quartiles.

Table 3.3: Static difference-in-differences estimates, heterogeneity by pre-period mean wage

	Log mean wage	Log mean wage floor	Log wage cushion	Log employment	Sh. main modality
	(1)	(2)	(3)	(4)	(5)
CB shock × Pre wage 1	0.1509	0.0884	0.0624	-0.1857	-0.0134
	(0.0400)	(0.0215)	(0.0414)	(0.0944)	(0.0370)
CB shock × Pre wage 2	0.1433	0.1112	0.0018	0.0516	0.0616
	(0.0375)	(0.0260)	(0.0306)	(0.1037)	(0.0302)
CB shock × Pre wage 3	0.0871	0.0729	0.0068	0.0926	0.0659
	(0.0253)	(0.0272)	(0.0330)	(0.0591)	(0.0265)
CB shock × Pre wage 4	0.0483	0.1167	-0.0531	-0.0719	-0.0199
	(0.0352)	(0.0564)	(0.0644)	(0.0667)	(0.0448)
p-value joint significance	0.0000	0.0000	0.6086	0.0003	0.0049
p-value equality $1 = 2$	0.8414	0.4579	0.2777	0.0832	0.0421
p-value equality $1 = 3$	0.1704	0.5468	0.2408	0.0033	0.0271
p-value equality $1 = 4$	0.0849	0.6655	0.1752	0.3577	0.8736
Firm shock	Y	Y	Y	Y	Y
Local market-Pre wage-Size-year FE	Y	Y	Y	Y	Y
Pre-period CB shock	Y	Y	Y	Y	Y
Num. firms	7,145	6,937	6,937	7,145	7,145
Num. fixed effects	24,894	17,853	17,853	24,894	24,947
Num. observations	76,911	46,104	46,100	76,911	77,308
Adjusted R^2	0.8591	0.9246	0.8373	0.9000	0.5946

Notes: Data are from the baseline panels of exporting firms excluding CB unit by province cells with fewer than 6 firms. The figure shows regression coefficients on the CB shocks interacted with an indicator for year greater than or equal to 2012. The CB shock variable is further interacted with an indicator for whether the average firm wage in 2007–2009 is in the first ("Pre wage 1"), second ("Pre wage 2"), third ("Pre wage 3") or fourth ("Pre wage 4") quartile of the distribution of firm wages within the province. The first p-value tests the joint significance of the four coefficients presented in the table, and the remaining p-values test the null hypothesis that the coefficient on the first quartile is equal to the coefficient on the second, third, and fourth quartiles, respectively. The regression models controls for the firm shock, local labor market by year by pre wage fixed effects, and a similar CB shock for the pre-period interacted with year dummies. Standard errors, included for the hypothesis testing, are clustered at the CB unit level.

The estimates suggest that employment elasticity to the firm wage differs across the productivity distribution. Taking the point estimates at face value, for the first quartile I obtain $-0.1857/0.1509 \approx -1.23$ and for the third quartile $0.0926/0.0871 \approx 1.06$. For comparison, Cengiz et al. (2019) estimates an employment elasticity to own wage of 0.41. However, the elasticity estimates are quite noisy, making it hard to draw strong conclusions about their magnitude.

3.5.4 Comparison with the rent-sharing literature

The estimates of the effects of firm shocks are comparable to those in the rent-sharing literature. The most closely related paper is Garin and Silvério (2023), who use a similar estimation strategy in Portugal. The authors find that an "idiosyncratic" export shock to a firm increases log sales by 0.143 (Panel A of Table 4) and log monthly wages by 0.022 (Panel A of Table 6). The implied elasticity is 0.15, comparable to my estimate of 0.12. The authors also study the effects of "common" export shocks to firms, but do not study CB shocks. More generally, Jäger et al. (2020, Figure II) review the rent-sharing literature and find that elasticity estimates using worker-level micro-data are on average 0.099, which is comparable to my estimates as well. Card et al. (2018, Table 1) find an average rent-sharing elasticity of studies using worker-level specifications of 0.08.

The larger estimates of effects of CB shocks on wages suggest that the magnitude of rent-sharing elasticities might depend on the level of aggregation of the shocks. Jäger et al. (2020, Figure II) also find that industry-level specifications, and calibrations of bargaining power in macro models, tend to find larger elasticity estimates. Similarly, industry-level specifications in Card et al. (2018, Table 1) also tend to find larger elasticities. For example, Abowd and Lemieux (1993) use contract wages from CB agreements and shocks to international prices in Canadian manufacturing. Their estimates suggest a rent-sharing elasticity of around 0.2, twice as high as the average worker-level estimates. I find that the response to a change in average product demand at the CB unit is larger than the response to a comparable change in demand at the firm, implying stronger wage responsiveness to CB shocks. Card and Cardoso (2022) find that the mean change in value added in the CB unit is associated with the change in wage floors in Portugal, but do not compare the effects of shocks to firms and CB units.[22]

3.5.5 Falsification tests and robustness checks

In this subsection I present additional empirical exercises to assess the robustness and validity of the empirical results.

Firms with multiple collective bargaining agreements. A minority of firms declare workers under multiple CB units, presenting an interesting case study to determine whether the relevant factor is the CB

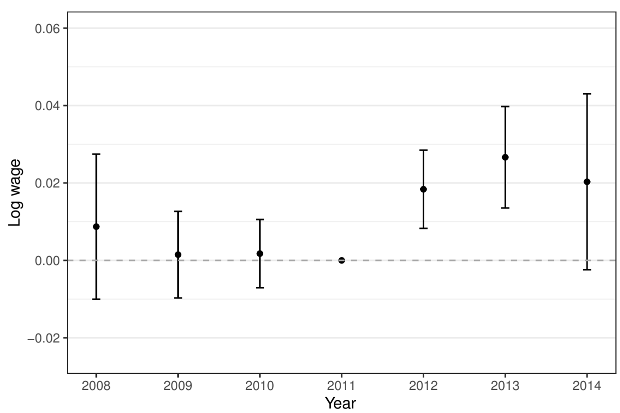
[22] Card and Cardoso (2022) do not rely on quasi-random variation, and find an implied elasticity of around 0.1. These estimates are hard to compare to my results as I cannot compute the effect of CB shocks on value added.

unit covering the worker or other unobserved channels that may propagate the shocks. If the shocks to firms in the same CB unit propagate through other channels, we should not observe CB unit-specific effects within the firm. However, if the shock propagates through CB, we should observe an effect on wages of workers declared under that CB unit.

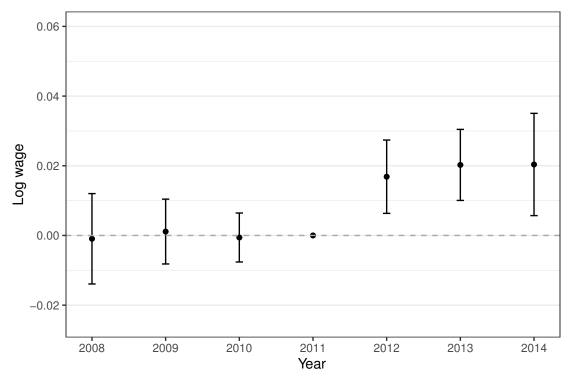
I conduct a worker-level analysis and find that wages respond to shocks to their own CB unit, even when controlling for firm by year fixed effects. To abstract from the extensive margin, I focus on a sample of workers that are observed in an exporting firm before and after 2011.[23] Figure 3.7 shows the estimates. Panel (a) replicates the baseline results: a positive shock to the primary CB unit in the firm increases wages. Panel (b) estimates a model that uses the CB shock of the worker's CB unit, instead of the primary CB unit in the firm, as treatment variable. Importantly, as it controls for firm by year fixed effects, this specification allows for arbitrary time-varying unobservables at the firm level that should capture any spillover effects of the CB shock through other channels. The figure shows a consistent positive effect of the CB shock on wages.

Figure 3.7: Effect of export shocks to CB units, worker-level estimates

(a) Shock to firm's primary CB



(b) Shock to worker's CB unit, controlling for firm-year effects



Notes: Data are from a panel of workers that worked in 2008, 2011, and 2014 in a firm in the baseline sample. The figures show estimates of the effect of CB shocks on mean monthly wage. Panel (a) estimates a difference-in-differences model using the primary CBA of the firm to define the treatment. It includes controls for the firm shock, worker by firm ("match") fixed effects, 6-digit economic sector by province by year fixed effects, hiring modality by year fixed effects, and 2-digit economic sector by an indicator for whether the worker's CB is the primary CB unit in the firm by year fixed effects. Panel (b) estimates a difference-in-differences model as well, but instead uses the CB shock that is specific to each worker and controls for worker fixed effects and firm by year fixed effects. Standard errors are clustered at the CB unit level.

Appendix Table 3.F provides detailed regression results using static DiD models. Columns (1) through (3) show consistent positive effects of CB shocks to the primary CB unit of the firm when varying local labor market controls, with column (3) corresponding to the specification in Panel (a) of Figure 3.7. Column

[23]Specifically, I focus on workers employed in the same exporting firm in 2008, 2011, and 2014, and with a non-missing CB agreement code.

(4) shows that the effect of the primary CB shock is driven by workers that are actually declared under the primary CB unit in the firm. The effect on workers declared under a different CB unit is smaller in magnitude and not statistically significant. Finally, Column (5) replicates the within-firm model in Panel (b) of Figure 3.7.

Controls for local-labor-market effects. A key concern is that the results may be driven by spillovers between firms through proximity instead of the CB network. I explore this possibility in Appendix Table 3.G, where I show estimates that use three alternative specifications to control for local labor market trends: the baseline specification that interacts 4-digit economic sector with province and year fixed effects; the same specification but using 6-digit economic sector instead; and a third specification that uses 2-digit sector and additionally controls for an export shock computed similarly to the CB shock but at the 6-digit sector level. The effects of firm and CB shocks are similar across specifications, suggesting that spillovers through proximity are not driving the results. Columns (3), (6), and (9) indicate that 6-digit sector shocks affect wages and employment, but do not affect wage floors, consistent with spillovers through proximity.

Effects on non-exporters. I have so far focused on exporting firms, but CB shocks should affect non-exporting firms as well. To test for this, in Appendix Figure 3.N I estimate the effect of CB shocks on wages and the wage floor using only firms that do not export. I find a similarly-sized effect of CB shocks on mean wages, implying that non-exporting firms are also affected by the CB shock. Given that the effect on wages seems to start in 2011, the effect on wage floors computed relative to 2011 appears somewhat smaller in this sample.

Placebo CB units. To assess the validity of the design I estimate the DiD model using several placebo CB shocks. To do so, I construct placebo CB networks by randomly shuffling the CB unit code across all firms, across firms within 1-digit sector and province cells, and across firms within 2-digit sector and province cells. Appendix Figure 3.O shows estimates using the placebo CB shocks, and reveals no evidence of effects of the placebo shocks on wages or wage floors.

Other robustness checks. A final concern is that regression controls or sample restrictions may be driving the results. Appendix Table 3.H varies the set of controls included in the regression, and shows very similar results when dropping the firm-level controls, controlling for the pre-period firm shock directly, and excluding the control for the pre-period CB shock. Appendix Figure 3.P replicates the dynamic estimates on wages and employment keeping firms with extreme values of the pre-period firm shock, relaxing one of the sample restrictions. The effects of the CB shock and those of the firm shock on employment are very similar to the baseline estimates. However, for the effect of the firm shock on wages there is a significant pre-period coefficient in 2007, and no effect of the firm shock after 2011. While I find the exclusion of these firms to be justified, it is important to note how the results change when they are included.

Appendix Table 3.I varies the set of CB units included in the sample. Column (2) shows that keeping CB units with less than 5 exporting firms in the sample does not affect the results. Column (3) shows that dropping CB units with less than 30 exporting firms seems to increase the effect of the CB shock. Consistent with Table 3.2, larger CB units seem to respond more strongly to CB shocks. Column (4) shows results of the static model when excluding the retail CB unit. Columns (5) through (8) replicate the finding of non-significant effects of the CB shock on employment.

3.6 Structural Model

While the empirical estimates indicate that shocks propagate through the existing CB network in Argentina, they cannot inform us about the role of the network in shaping the effects of shocks. To study this question I develop a spatial economic model of the labor market with collective bargaining. Unlike in Section 3.2, the model in this section allows for multiple local labor markets, explicitly incorporates workers' labor supply decisions, adds formal structure to the firm's problem to obtain tractability, and introduces multiple CB units. The model is estimated and used to analyze how shocks propagate under counterfactual CB networks in the upcoming sections.

3.6.1 Set-up

There is a fixed population of N_r in each region $r \in \mathcal{R}$, each with a separate labor market. Regions are divided into local labor markets $g \in \mathcal{G}$, each characterized by a single economic sector $k \in \mathcal{K}$. I denote by $\mathcal{K}1$ the broadest grouping of economic sectors. A given sector and region cell may be partitioned into multiple local markets. Each local market contains a continuum of firms, indexed by j, and the measure of firms is given by M_g. The collective bargaining network is a partition of local labor markets denoted by $\mathcal{C}$. Within each CB unit $c \in \mathcal{C}$ a union and an employer association bargain over a single wage floor $\underline{w}_c$ binding for all local markets in the unit.[24]

Different actors take decisions sequentially. First, CB units play a simultaneous-move game to determine wage floors $\{\underline{w}_c\}_{c \in \mathcal{C}}$. Second, firms j in each g draw productivities $\varphi(j)$ from a known distribution $F_g(\varphi)$.

[24] As discussed in Section 3.3.2, there may be constant regional differences in wage floors within a CB unit. These differences do not respond to shocks, os CB units effectively bargain over a single wage floor.

Third, workers in each region r decide, first, whether to enter the formal labor market or not, and second, their supply of labor to each firm. Simultaneously, firms decide employment and wages under monopsonistic competition. Wage indexes adjust to clear regional labor markets. Finally, production takes place and workers earn the wage $w(j)$ in their firm j.

To focus on the role of the CB network, I abstract from the goods market. I will model economic shocks as changes in the productivity distributions of local labor markets.

3.6.2 Solving the model

I solve the model backwards starting with the decisions of workers and firms, and then moving into the bargaining game between CB units.

3.6.2.1 Labor supply

Conditional on formal labor market entry, a worker i in region r has an indirect utility of working for firm j given by $V_{ri}(j) = A_{k1(j)} w(j) \xi_i(j)$. $A_{k1(j)}$ is an amenity value specific to j's broad sector, $k1(j)$, and $w(j)$ is j's wage. I assume that the idiosyncratic component $\xi_i(j)$ follows a Fréchet (or type-2 extreme value) distribution with scale parameter η, as is standard in the literature of discrete choice models. Then, it can be shown that labor supply to a firm located in r is

$$\ell(j) = \left[\frac{A_{k1(j)} w(j)}{W_r}\right]^{\eta} \tag{3.6}$$

where W_r is a region-specific aggregate wage index. Derivations are available in Appendix 3.F.1. Note that η can be interpreted as the elasticity of labor supply to the firm.

Since $\xi_i(j)$ follows a Fréchet distribution, the expected utility of formal employment in region r, V_r, is proportional to the regional wage index. Let $\Gamma(\cdot)$ denote the gamma function, then

$$V_r = \Gamma\left(\frac{\eta - 1}{\eta}\right) W_r. \tag{3.7}$$

Before choosing a firm, workers decide whether to work formally or not. Workers' preferences for formal employment in r depend on the value of formal employment V_r, the value of non-formal employment b_r, and an idiosyncratic shock that follows a Gumbel (or type-1 extreme value) distribution with shape parameter

ζ. The resulting formal employment share is given by

$$\mu_r = \frac{V_r^\zeta}{V_r^\zeta + b_r}. \tag{3.8}$$

A larger value of ζ implies a larger elasticity of the employment share to changes in V_r.

3.6.2.2 Labor demand

Each firm j draws a productivity $\varphi(j) \geq 0$ from a local labor market-specific distribution $F_g(\varphi)$ defined over $[\varphi_{g0}, \infty)$. Firms in local labor market g maximize profits with a linear technology:[25]

$$(\ell(j)^*, w(j)^*) = \arg\max_{\ell, w} \left\{ \varphi(j)\ell - w\ell \;\middle|\; \ell = \left(\frac{A_{k1(j)}w}{W_{r(j)}}\right)^\eta,\ w \geq \underline{w}_c \right\}. \tag{3.9}$$

The wage floor may be binding or not. If not, we say that the firm is *unconstrained*, in which case the solution to the firm's problem for a firm with productivity $\varphi(j)$ is $w(\varphi(j)) = \mu\varphi(j)$, where $\mu = (\eta/(\eta+1))$ is the markdown factor. If the wage floor is binding and the firm is *constrained* we have that $w(\varphi(j)) = \underline{w}_c$. The quantity of labor can be obtained by replacing the wage in the labor supply curve (3.6). Appendix 3.F.1 shows exact expressions for employment and profits.

If the wage floor is high enough, the threshold that determines whether firms are constrained in g is given implicitly by $w(\overline{\varphi}_g) = \underline{w}_c$, which results in

$$\overline{\varphi}_g = \mu^{-1}\underline{w}_c. \tag{3.10}$$

Firms with $\varphi \leq \overline{\varphi}_g$ will pay exactly the wage floor. Similarly, firms leave the market if ex-post they experience negative profits $\pi(\underline{\varphi}_g) < 0$, implying thresholds

$$\underline{\varphi}_g = \underline{w}_c. \tag{3.11}$$

Consistent with Section 3.2, employment may decline following wage floor increases when firms with $\varphi < \underline{\varphi}_g$ exit. Unlike in Section 3.2, the model does not feature demand-constrained firms, so the only reason for employment declines is firm exit. If $\underline{w}_c < \mu\varphi_{g0}$, then no firm will be constrained.

The distance between the thresholds depends on η. A higher η implies lower heterogeneity in idiosyncratic worker-level preferences $\xi_i(j)$, and so more responsiveness to wages. In the extreme case of $\eta \to \infty$, the markdown vanishes and the wage floor is only binding for the lowest productivity firm. A lower η implies

[25] I assume a linear technology for analytical convenience. The main results will be qualitatively similar under alternative specifications.

more heterogeneity in $\xi_i(j)$, and so lower responsiveness to wages. If $\eta \to 0$, the markdown goes to 0 and so the bargained wage floor becomes binding for all firms.

Parametrization and the share of firms bunching. I assume that productivities in local labor market g are drawn from a Pareto distribution with shape $\alpha > 1$ and cdf given by

$$F_g(\varphi) = 1 - \left(\frac{\varphi_{g0}}{\varphi}\right)^{\alpha} \tag{3.12}$$

for $\varphi \geq \varphi_{g0}$ and zero otherwise. The conditional cdf of productivity given a minimum value of $x > \varphi_{g0}$ is given by $F_g(\varphi|x) = 1 - (x/\varphi)^{\alpha}$. The fraction of "possible" productivities that will be observed is given by $1 - F_g(\varphi^{\min})$, where $\varphi^{\min} = \max\{\varphi_{g0}, \underline{\varphi}_g\}$. The total measure of firms in the market is then $M_g\,(1 - F_g(\varphi^{\min}))$. Note that this quantity is non-increasing in the wage floor (if $\underline{w}_c > \varphi_{g0}$, then the measure of firms is decreasing in the floor).

The share of observed firms paying the wage floor, or "bunching," can be computed using the conditional cdf $F_g(\varphi|\varphi^{\min})$. If $\underline{w}_c < \mu\varphi_{g0}$ the share is zero. If $\underline{w}_c > \varphi_{g0}$ then $\varphi^{\min} = \underline{w}_c$ and the share of firms bunching takes its maximum value of $1 - \mu^{\alpha}$. Otherwise, the share varies between 0 and this maximum value. Appendix Figure 3.Q illustrates the productivity distribution with a binding wage floor, for a situation where the share of firms bunching is in the intermediate case. Panel (a) shows the pdf. Panel (b) illustrates the wage paid at each productivity level.

Computing aggregate quantities. To compute aggregate quantities I integrate over the distribution of firms in g. To obtain average wages across active firms, or the share of firms bunching, I integrate over "observed" productivities. The minimum value of this integration might be φ_{g0}, $\underline{\varphi}_g$, or $\overline{\varphi}_g$ depending on the value of $\underline{w}_{c(g)}$. For example, with $\underline{\varphi}_g < \varphi_{g0} < \overline{\varphi}_g$ the average wage is given by $w_g = \int_{\varphi_{g0}}^{\overline{\varphi}_g} \underline{w}_c f_g(\varphi)d\varphi + \int_{\overline{\varphi}_g}^{\infty} w(\varphi) f_g(\varphi)d\varphi$, where $f_g(\varphi)$ is the pdf of the Pareto distribution and $w(\varphi) = \mu\varphi$. To compute aggregate labor demand I integrate over the distribution of "possible" productivities in g, so the minimum value will always be φ_{g0}. Formally, $L_g = M_g \int_{\varphi_{g0}}^{\infty} \ell(\varphi) f_g(\varphi)d\varphi$. I proceed analogously to determine revenue and the wage bill. These are the relevant quantity for Nash bargaining, as the measure of firms in the market will then respond to the wage floor. Appendix 3.F.1 shows closed form expressions for these quantities.

3.6.2.3 Nash bargaining

Each CB unit negotiates over wage floors $\{\underline{w}_c\}_{c\in\mathcal{C}}$. I assume that both unions and employer associations are risk neutral, and that both parties have rational expectations in the sense that they know the distributions

$\{F_g(z)\}_{g\in\mathcal{G}}$ and correctly anticipate the outcomes following their choice of $\underline{w}_c$. As a result, I do not use expectations below.

Following the framework in Section 3.2, I assume that the objective function of the union is given by $U_c\left(\underline{w}_c, \underline{\mathbf{w}}_{-c}\right) = \sum_g WB_g$, where WB_g is the wage bill in g, and the objective of the employer is $\Pi_c\left(\underline{w}_c, \underline{\mathbf{w}}_{-c}\right) = \sum_g \left(R_g - WB_g\right)$, where R_g is revenue in g. Preferences depend on the wage floor $\underline{w}_c$ by altering the share of firms bunching and the cost of labor. They also depend on the wage floor of other CB units $\underline{\mathbf{w}}_{-c}$, which affects the equilibrium wage index in each region.

Solving the Nash bargaining problem. Letting β_c denote the bargaining power of the union, the Nash bargaining problem can then be written as in (3.1). Denoting $WB_c = \sum_g WB_g$ and $R_c = \sum_g R_g$, the solution to a single Nash problem is given by the split rule $WB_c\left(\underline{w}_c, \underline{\mathbf{w}}_{-c}\right) = \omega_c R_c\left(\underline{w}_c, \underline{\mathbf{w}}_{-c}\right)$, where $\omega_c \in (0,1)$ is a weight that depends on the wage floors, given in (3.3). Importantly, the weight ω_c is affected by the derivatives of U_c and Π_c with respect to $\underline{w}_c$, which incorporate the effect of the wage floor on the aggregate wage index.

Nash-in-Nash solution. Horn and Wolinsky (1988) introduced this solution concept in the analysis of bilateral monopolies.[26] In a Nash-in-Nash solution, each individual bargaining problem results in a Nash equilibrium given that the wage floors of other CBAs are in equilibrium as well. The solution assumes that players in a given CB unit do not take into account the effect of their decision on the choice of other CB units.[27] In the setting of this model, the wage floor profile $\{\underline{w}_c^*\}_{c\in\mathcal{C}}$ is a Nash-in-Nash solution if

$$\underline{w}_c^* = \arg\max_{\underline{w}} U_c\left(\underline{w}, \underline{\mathbf{w}}_{-c}^*\right)^{\beta_c} \Pi_c\left(\underline{w}, \underline{\mathbf{w}}_{-c}^*\right)^{1-\beta_c} \tag{3.13}$$

for all $c \in \mathcal{C}$.

3.6.3 Equilibrium

Given a collective bargaining network $\mathcal{C}$ and a set of parameters, an equilibrium is defined as a set of wage floors $\{\underline{w}_c^*\}_{c\in\mathcal{C}}$, regional wage indexes $\{W_r^*\}_{r\in\mathcal{R}}$, and employment shares $\{\mu_r^*\}_{r\in\mathcal{R}}$ such that: (1) the Nash-in-Nash bargaining game is solved, (2) labor markets clear in each region. Appendix 3.F.2.1 formally defines the equilibrium. In general, there is no closed form solution for the vector of equilibrium wage floors and

[26] Davidson (1988) studies a two-union bargaining game and uses a similar solution concept.

[27] This assumption is debatable if a single union takes part in multiple CB units. While I assume separate unions across CB units, Collard-Wexler, Gowrisankaran, and Lee (2019) show that the Nash-in-nash solution can be micro-founded in a fully non-cooperative environment where players internalize the interdependence of their potentially multiple bargains.

regional wage indexes. Appendix 3.F.2.2 discusses the algorithm I use to compute the equilibrium for a given set of parameters.

3.7 Estimation and Validation of Structural Model

In this section I estimate and validate the model with aggregate data. To do so, I first construct a dataset at the local labor market level and use it to estimate the model parameters. Second, I show that the model is able to replicate several features of the data. I postpone the counterfactual exercises, which rely on the estimated model, to the next section.

3.7.1 Local labor markets data

To keep the number of local labor markets and CB units manageable, I simplify the economic sectors and the CB network. I specify sectors $k \in \mathcal{K}$ as a coarsening of the 4-digit sectors, and regions $r \in \mathcal{R}$ as a grouping of provinces in Centro (center, including Buenos Aires), Cuyo (west), Norte (north), and Patagonia (south).[28] I further divide the economic sector by region cells using the exporting status of firms, the CB unit codes, and (if the cell is large enough) provinces. To make sure that all local markets have a wage floor, I create "local" CB units by grouping CB units that have less than 200 employees or fewer than 1% of workers with a valid wage floor. Importantly, the definition of local CB units does not use the exporting status, so local CB units may include both an exporter and a non-exporter local labor market.

Once the spatial economy is defined, I aggregate the firm-level data to the local labor market level. To estimate the share of firms constrained by the wage floor I use the workers that were assigned a wage floor. To account for part-time work and measurement error, I define a worker as a "buncher" (i.e., with a deviation of zero) if the wage is between 90 and 105% of the floor, and between 40 and 60% if the worker is classified as part-time. Then, the share of firms constrained is simply the share of firms where all workers are bunchers. To estimate wage floors for each CB unit, I take the mean wage floor across local labor markets weighting by the share of workers with an assigned wage floor in each local labor market. I allow for regional wage floors within a CB unit only if the region's average wage floor is sufficiently different from the rest. I also compute the average wage and total employment using the firm-level data, adjusting for (estimated) part-time work. Appendix 3.F.3 provides details on the construction of the local labor markets and the adjustments made to the aggregate data to correct for possible measurement error.

[28] I drop several broad economic sectors, including agriculture, education and government, in which bargaining is regulated by different regimes. Additionally, the agricultural sector represents a small share of employment.

Appendix Table 3.L provides summary statistics. There are 894 CB units, of which 456 are local. 63.5% of local labor markets are in the region "Centro," 13.6% are covered by a local CB unit, and 14.9% are covered by the retail CB unit. There are a few small local labor markets where the mean wage is lower than the wage floor. This is caused by two things: (1) merging different CB units together, and (2) small local labor markets relying on part-time work. Using the part-time adjusted mean wage solves the problem, but introduces some missing values. The measure of firms in each local labor market M_g is normalized so that $\frac{1}{|\mathcal{G}|}\sum_g M_g = 1$. The table also shows model-estimated minimum productivities, which are discussed in the next subsection.

3.7.2 Calibration and Estimation

I use several strategies to estimate the model parameters, which are summarized in Table 3.4 and detailed in this section. Appendix 3.F.4 provides details.

Worker problem. To estimate the preference heterogeneity parameter η and the amenity values $\{A_{k1}\}_{k1\in\mathcal{K}1}$, I leverage the relationship between firm size and firm wages implied by the labor supply to the firm (3.6). In particular, I estimate a regression of log mean wage on log firm size, controlling for region and broad sector fixed effects.[29] I find a value of $\eta \approx 4.10$, which is in line with the literature. This value implies a markdown factor of $\mu \approx 0.80$. I calibrate the parameter indicating heterogeneous preferences for formal employment using evidence on the extensive-margin labor supply elasticity from Chetty et al. (2011) and data on the share of formal employment in the labor market. This results in $\zeta \approx 0.28$.

Table 3.4: Estimation strategy for different model parameters

Parameter	Description	Source
ζ	Elasticity of formal employment	Literature
η	Elasticity of labor supply to the firm	OLS estimation
$\{A_{k1}\}$	Amenity values of 1-digit sectors	OLS estimation
α	Curvature of productivity processes	Calibration
$\{\varphi_{g0}\}$	Minimum productivity of Pareto distributions	Model inversion
$\{b_r\}$	Outside option of workers in each r	Model inversion
$\{\beta_c\}$	Bargaining power parameters	Model inversion

Notes: The table shows the parameters of the structure model and the approach used to estimate them.

Productivity distributions. I calibrate the common shape parameter α to 5.50. This value, which is in line with estimates calibrated in the literature, corresponds to the model admitting all shares below the

[29] While the model implies an exact fit of this regression, it is straightforward to incorporate hours to allow for an error term, as discussed in the appendix.

98.8th percentile of the distribution of observed shares of firms bunching at the wage floor. Shares above this threshold are set to the maximum feasible value in the model. To estimate the minimum productivities $\{\varphi_{g0}\}_{g\in\mathcal{G}}$, I invert the expression for the share of firms bunching at the wage floor implied by the model. I use the wage floor in each local labor market and the values of η and α from above. Appendix Table 3.L shows summary statistics of the estimated $\{\varphi_{g0}\}_{g\in\mathcal{G}}$, in ARS. We observe that the φ's are on average larger than the minimum wage, which per equation (3.11) is the lower threshold that defines whether firms exit the market.

Outside options. Using estimates from above, the share of formal firms in each region from household survey data, and the market-clearing condition in each region, I compute $\{W_r\}_{r\in\mathcal{R}}$ and $\{V_r\}_{r\in\mathcal{R}}$. Then, I invert equation (3.8) to obtain the outside option parameters $\{b_r\}_{r\in\mathcal{R}}$.

Bargaining power. I invert equation (3.2) to obtain β_c for each c. This condition will hold in any interior equilibrium. The key variation used to pin down these parameters comes from the ratio of the derivatives of U and Π with respect to the wage floor, evaluated at the equilibrium. If the ratio is low, the union is relatively closer to its optimal wage floor (where $dU/d\underline{w}_c = 0$), suggesting stronger bargaining power. The distribution of the ratio of derivatives is shown in Panel (a) of Appendix Figure 3.R. After estimating the parameters I check whether they correspond to an interior equilibrium or not, and find that this is not the case for the retail CB unit. This unit is so large that it has significant general equilibrium effects. As a result, the objective function is not strictly concave, and the bargaining power actually corresponds to a local maximum. To deal with this, I hold the wage floor of this CB unit fixed when computing counterfactual equilibria. Panel (b) of Appendix Figure 3.R shows the estimated bargaining power parameters. There is a great deal of heterogeneity that cannot be accounted for by characteristics of CB units.

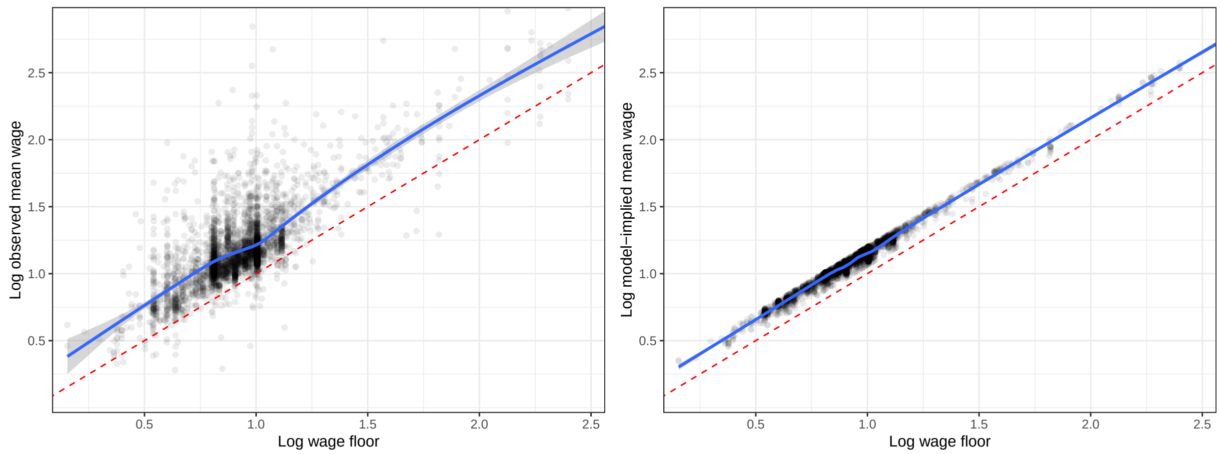
3.7.3 Validation

I validate the model comparing data moments with model-based predictions. To do so, I use the parameters estimated before to compute the model-implied mean wages in each local labor market in 2011–2012. I start by comparing the correlation between mean wages and wage floors in the data and the model. Panel (a) of Figure 3.8 shows that the model does a good job at replicating the correlation between mean wages and wage floors, where log mean wages are roughly a constant shift of log wage floors. Unsurprisingly, given that the production technology is very simple in the model, the data show more variation in average wages than the model. As discussed before, the data construction results in a minority of local labor markets where the mean wage is lower than the wage floor. Appendix Figure 3.S shows that the model does a good job at replicating the correlation between the share of firms bunching at the wage floor and the mean wage.

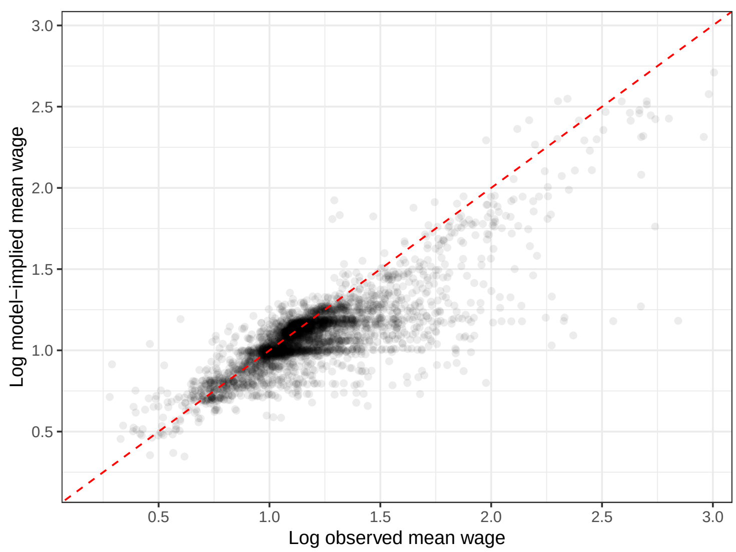
Figure 3.8: Summary of model fit to the data

(a) Wage floor versus average wage

In the data　　　　In the model



(b) Wages in the data versus in the model



Notes: This figure illustrates the model fit to the local labor market data. Panel (a) shows the correlation between the log wage floor and the log mean wage. The left column shows the data, the right column shows the model. Panel (b) shows a scatter plot of observed mean wages versus model-implied mean wages in each local labor market. The measure of mean wages used in these figures is not adjusted by part-time employment.

The second validation exercise is to compare the distribution of wages in the data and the model. Importantly, mean wages in the data were not used in the estimation of the model parameters. Additionally, mean wages are weakly correlated with the share of firms bunching at the wage floor (as shown in Appendix Figure 3.S), which is the key input used to estimate the minimum productivities. Panel (b) of Figure 3.8 shows that the observations cluster around the 45-degree line, indicating that the model does a good job at replicating the mean wage in each local labor market. Once again, mean wages in the data are more volatile than in the model.

3.7.4 Replicating the effects of CB shocks

To construct shocks in the model I rely on the estimated effects of shocks on local labor market revenue. To do so, I first estimate the effect of trade shocks on aggregate revenue using data from the survey of businesses. Appendix Table 3.M shows that aggregate revenue increases by about 22% in exporting local labor markets, and this conclusion is similar when excluding the largest CB units in the data. There is also a non-significant effect on non-exporting local labor markets of about 6%. Then, I take the estimated model in 2011–2012 and simulate shocks to minimum productivity that would result in the same changes in aggregate revenue as suggested by the survey data. I re-compute the model equilibrium using the new minimum productivities, and estimate the effect of the shocks on wages using a shift-share DiD strategy.

The model is able to replicate the effects of CB shocks on wages observed in the data. Table 3.5 shows the estimates using the aggregate data and the model-generated data. CB shocks affect wages in the aggregate data, just like in the firm-level estimates, although the results are noisier. The model is able to replicate the effects of CB shocks on wages. However, the magnitudes of the effects are smaller in the model, which understates the degree of spillovers. The main reason is that the model does not account for spillover effects of the wage floor on unconstrained firms, which the heterogeneity analysis of Table 3.3 suggests can be quite important. There is no reason to think that this bias would be different for counterfactual CB networks, suggesting that the model is useful to explore how shocks propagate under different bargaining networks even if the magnitudes are not exactly correct.

Overall, these results suggest that the estimated model does a good job at capturing the main features of the data. In the next section I use the model to explore the propagation of shocks under different bargaining networks.

3.8 Model-Based Counterfactuals

In this section, I use the estimated model to explore how shocks propagate through the labor market under different CB networks. First, I illustrate how shocks propagate through the labor market using a simple simulated economy to visualize the mechanisms at play. Second, I define the counterfactual networks used in the Argentine data. Third, I explore how the different networks affect the propagation of shocks. Finally,

Table 3.5: Effects of CB shocks on wages and wage floors, aggregate data vs model estimates

	Data, 2011 vs 2014			Model	
	Log wage (adj. part-time)	Log wage	Log wage floor	Log wage	Log wage floor
CB shock	0.0619 (0.0313)	0.0369 (0.0493)	0.0411 (0.0374)	0.0203 (0.0016)	0.0086 (0.0073)
Share exporting empl. CB unit	Y	Y	Y	Y	Y
Region by 4d by exporter FE	Y	Y	Y	N	N
Region by exporter FE	N	N	N	N	N
Observations	1,390	1,492	1,404	1,079	1,079

Notes: The table shows the effect of CB shocks on log wages and log wage floors, in the aggregate data and the model-generated data. The model is estimated using 2011–2012 data. I simulate shocks in the model so that changes in minimum productivities mimic the effects of exporting shocks on revenue at the local labor market level, and then re-compute the model equilibrium using the new minimum productivities. Then, I regress the change in an outcome on the average local labor market shock at the CB level, using pre-period employment shares to weight the shocks. Both the aggregate data and the model-based data exclude local labor markets covered by the retail CB unit, and those in CB units with less than 5 percent of employment in exporting firms. Standard errors are clustered at the CB level.

I discuss the implications of the findings and their relationship to the literature.

3.8.1 Illustration of model mechanisms

I simulate a simple economy to illustrate how shocks propagate in the model. I define 3 local labor market types (L, M, H), 3 regions (L, C, R), and 15 economic sectors within each region and type cell. The elasticities η, ζ, and α are the same as in the main estimates, and the bargaining power parameters are defined to $\beta_c = 0.35$ for all CB units. I allow the baseline minimum productivity to differ by local market type and region, and I shock a selection of local markets in regions L and C.[30] Then, I observe how the shock propagates to wages in different CB networks.

The main takeaway from this exercise is that the response of CB units to shocks depends on the distribution of productivity of covered local markets, suggesting that productivity dispersion within CB units may be an important determinant of the response of wages to shocks. Figure 3.9 shows the results, focusing only on 4 local labor markets in each type by region cell. Panels (a) and (b) show baseline productivities and the changes in productivity.[31] Panel (c) shows how shocks affect wage floors for a network in which CB units connect local labor markets with the same sector and type across regions. Consider the CB units labelled H0, M0, and L2. While they received a similar shock, they differ in the dispersion of productivity. Wage floors do not change in H0, which has the highest dispersion, and they change relatively more in L2, which

[30]A few more parameters need to be defined for the simulation. I assume $N_r = 100$ and $b_r = 1$ for all regions, and $A_{k1} = 1$ for all local labor markets. I assume $\varphi_{g0} = 1$ for local markets of type L in region L and all local markets in region C, $\varphi_{g0} = 1.025$ for those of type M in region L and those of type R, and $\varphi_{g0} = 1.05$ for those of type H in region L. I let $M_g = 3$ for local markets of type L, $M_g = 2$ for type M, and $M_g = 1$ for type H.

[31]Local labor markets omitted from the figure are not shocked directly.

has the lowest dispersion. Wage floor responses in M0 are in between, as is the dispersion of productivity. Panel (e) shows the response of average wages to shocks.

Figure 3.9 illustrates a couple more interesting features of the model. First, the exercise illustrates that CB units that cross regions will result in cross-regional propagation of shocks. Comparing across networks in Panels (c) and (d), we note that the cross-type network does not lead to regional contagion of shocks. Second, the response of wage floors will depend on whether the low- or high- productivity local labor markets are shocked. Consider the second network, depicted in Panels (d) and (f), and the CB units labelled L1 and L2, both of which have two local markets shocked. In L1 the high-productivity local market is shocked, while in L2 the low-productivity one is shocked. Wage floors respond more strongly in L2 relative to L1. Finally, there may be wage floor responses in CB units that are not directly affected by the shocks. For instance, Panel (d) shows that CB unit L3 increases its wage floor even though none of its local labor markets are directly affected by the shock.

3.8.2 Counterfactual CB networks

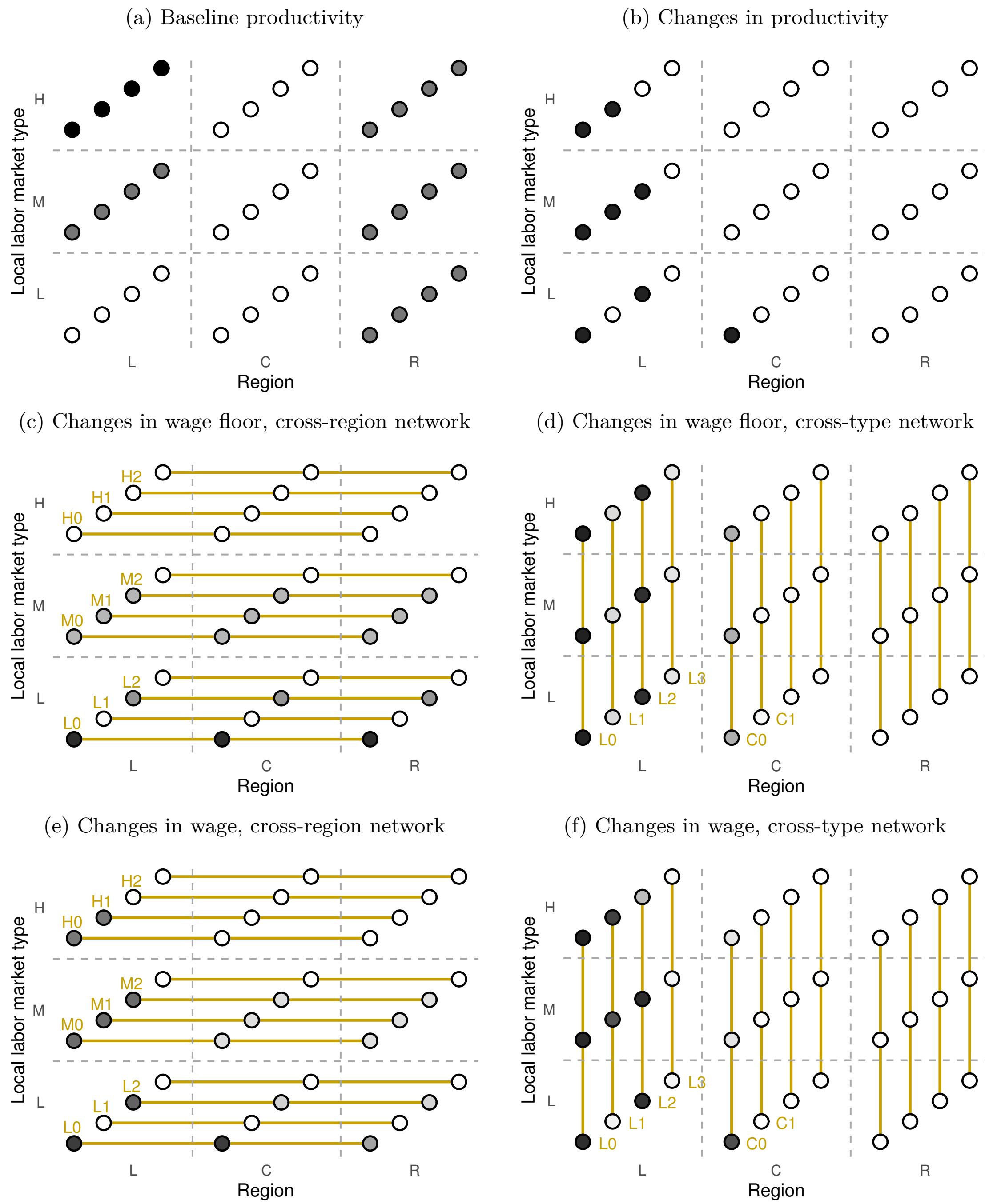
I manipulate the CB network observed in the data to create counterfactual networks. The first counterfactual, which I call Baseline Simple network, is defined by assigning some local labor markets with a local CB unit to the most common non-local CB unit in the region by economic sector cell.[32] I define a Baseline Region network by splitting the CB units in the Baseline Simple network by region, and a 2-d Sector and 3-d Sector networks by splitting it by 2-digit and 3-digit economic sector, respectively. Finally, I define a Local network, which consists of a separate CB unit for each local labor market, and a non-bargaining economy, for which I simply compute the market clearing regional wage indexes with non-binding wage floors. In order to hold the average economy-wide level of bargaining power constant, I assign bargaining power parameters to the new CB units by averaging the parameters of the baseline CB network across local labor markets, weighting by the measure of local labor market size M_g.[33]

These networks differ in the degree of centralization of bargaining or, in terms of the classification in Bhuller et al. (2022), of "vertical coordination" of bargaining. Examples of countries with mostly sectoral bargaining are Italy, Portugal, or France, while examples of countries with mostly local bargaining are the US, the UK, or Japan. I classify the counterfactual networks based on the average number of local labor markets per CB unit. The Baseline and Baseline Region networks have an average of about 4 local markets

[32] Specifically, I replace local CB units in a region by province by coarse 4-digit sector cell if the most common CB unit in the cell is non-local.

[33] A second consideration is the regional differences in wage floors within CB units. I preserve these differences in the new CB units and calculate them similarly to the bargaining power parameters to ensure their average remains constant. As stated before, these regional differences do not respond to shocks.

Figure 3.9: Illustration of collective bargaining networks and shock propagation, simulated data



Notes: The figure illustrates the propagation of productivity shocks to wages across different collective bargaining (CB) networks using simulated data. Each dot represents a local labor market, and its color shows the level of a variable in the pre-shock equilibrium or the percent change between the pre-shock and post-shock equilibria. Color palettes are defined within each outcome, and darker colors indicate larger values. Line segments connect local labor markets in the CB unit. Panel (a) shows the baseline productivity level, and Panel (b) shows the change in productivity. Panel (c) and Panel (d) show the change in the wage floor in each local labor market for two different CB networks, respectively. Panel (e) and Panel (f) show the change in the average wage for the same two CB networks, respectively. The model is simulated with 15 local labor markets in each of the nine Productivity by Region cells, of which only 4 are shown. There are no productivity changes in the omitted local labor markets.

per CB unit. The Baseline Simple network is more centralized, with an average of about 6. The 2-d and 3-d Sector networks are more decentralized, with an average of about 2 to 3. Finally, the Local network is the most decentralized.

These networks also differ in the degree of productivity dispersion within CB units. To capture this dimension I compute, for each network, the average log ratio of the 90th to the 10th percentile of minimum productivity across local labor markets within CB units. The ratios oscillate between about 0.02 and 0.06. By definition, the local network gets a value of 0, as each local labor market is its own CB unit and there is no dispersion.

3.8.3 Shock propagation under counterfactual CB networks

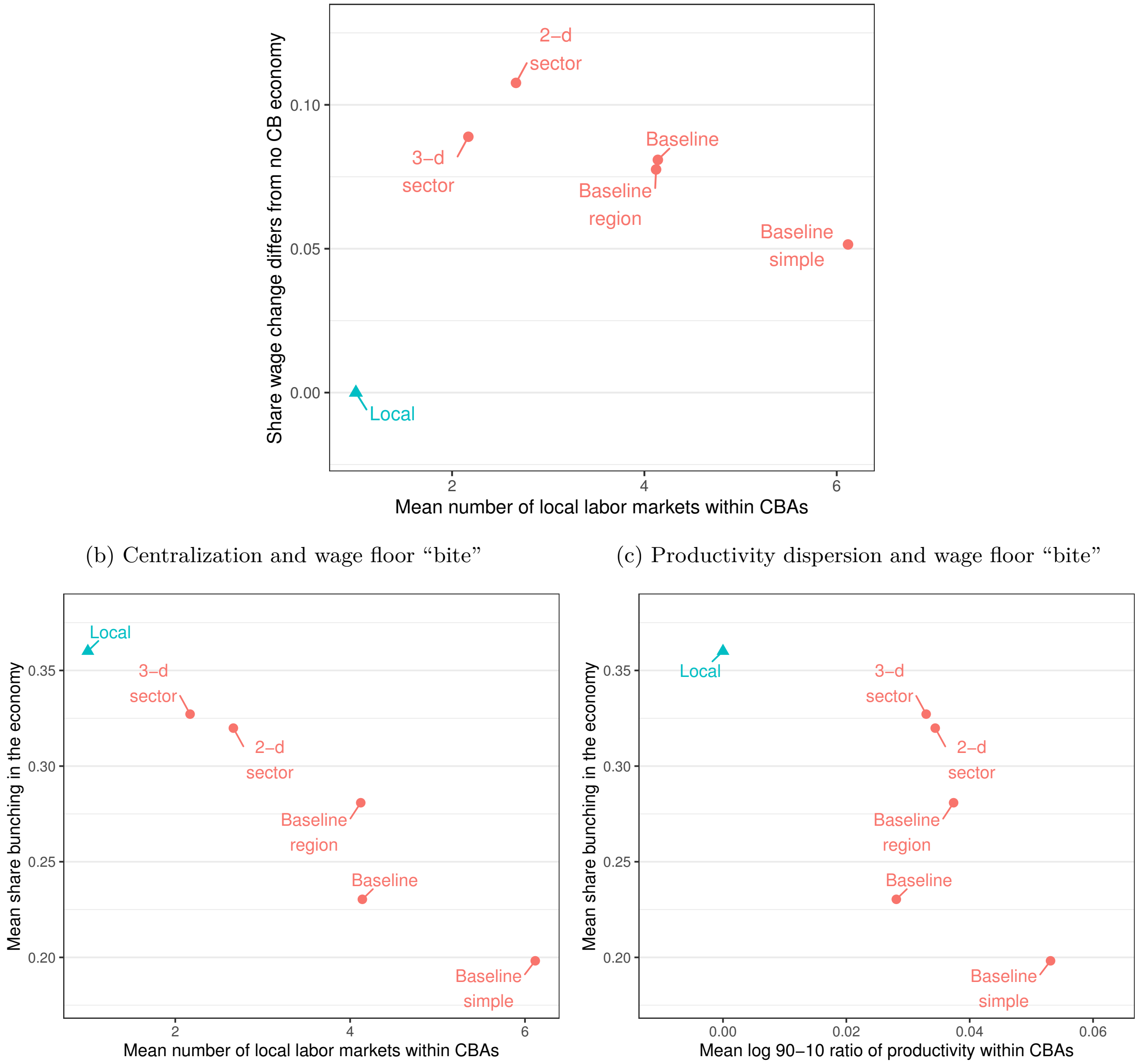
I use the counterfactual CB networks and the simulated shocks to compute the equilibrium of the model pre- and post-shock. As discussed in Section 3.7.4, the simulated shocks consist of a change in revenue that mimics the effect of the export shocks on aggregate revenue. Then, for each network I compute the wage floors and mean wages pre- and post-shock, and their change (in logs) as a result of the simulated shocks. I also simulate the effect of shocks in a non-bargaining economy, where wage floors are not binding in any local labor market.

I compare wage changes as response to shocks to the non-bargaining economy to summarize the extent of shock propagation in a given CB network. Specifically, I compute the share of local labor markets for which the absolute difference in the change in wages in a given network and the non-bargaining economy is more than 0.25%. If this share is large, then the economy is resulting in a large degree of shock propagation, so wage changes are mostly determined by factors outside the local labor market. This means that the effects of shocks are being shared across connected local labor markets instead of being fully absorbed by the directly shocked local labor markets.

The counterfactual estimates reveal a hump-shaped relationship between the degree of shock propagation and the degree of centralization of bargaining. This result is shown in Panel (a) of Figure 3.10. Both highly decentralized and highly centralized bargaining result in low degree of shock propagation. On the other hand, bargaining with medium levels of centralization results in relatively higher levels of shock propagation. Overall, there is significant variability in the degree of shock propagation across networks.

This pattern can be explained by extent to which wage floors are binding in the economy and the extent of the connections generated by CB. Panel (b) of Figure 3.10 shows that more centralized networks result in a lower share of firms bunching at the wage floor on average. In decentralized networks the bite of the

Figure 3.10: Centralization of bargaining and shock propagation across CB networks



(a) Centralization and shock propagation

(b) Centralization and wage floor "bite"

(c) Productivity dispersion and wage floor "bite"

Notes: Data are from model simulations pre- and post-export shocks under different CB networks. Panel (a) shows the degree of shock propagation against the level of bargaining centralization. Panel (b) shows the average share of firms bunching (wage floor "bite") against bargaining centralization. Panel (c) shows the average share of firms bunching against the level of productivity dispersion. The degree of shock propagation is measured as the share of local labor markets with an absolute average wage change following the shock more than 0.25% different from the counterfactual wage change in an economy without CB. This computation excludes local labor markets that correspond to the retail CBA (0130/75) at baseline and to CBAs with less than 5% of employment in exporting firms. Bargaining centralization is measured as the average number of local labor markets per CBA. The productivity dispersion is measured as the average ratio of the 90th to the 10th percentile of the productivity distribution within CBAs. The average share of firms bunching is measured as the simple mean across local labor markets.

wage floor is high, but since there are not many connections across local labor markets, the shock does not propagate. As the network becomes more centralized there are more connections across local markets, leading to more propagation. However, the wage floor has less bite, so shocks propagates less. At some point the bite becomes too low, and even though the network connects many local markets, the degree of shock propagation declines.

The key driver of wage floor bite is the average dispersion of productivity within CB units. Panel (c) of Figure 3.10 shows that the bite of the wage floor is decreasing in the dispersion of productivity within CB units. More centralized bargaining networks tend to result in CB units that are very heterogeneous. In order to avoid negative employment consequences in low-productivity local markets, unions in these networks set wage floors that are not as high. As a result, the bite of the wage floor is lower in these networks, and changes in wage floors following shocks are less impactful on wages.

The degree of bargaining centralization has consequences for the responses of employment to shocks as well. Appendix Figure 3.T shows a similar hump-shaped relationship between the degree to which employment responses to shocks differ from the non-bargaining economy and bargaining centralization. Because the share bunching is far from the maximum, employment responses to the wage floor are generally positive. As a result, the model implies that CB networks that lead to more propagation of shocks also relocate employment towards the positively shocked CB units.

3.8.4 Discussion

This paper shows that the degree of propagation of shocks across CB units is hump-shaped in the degree of centralization of bargaining. This suggests that labor markets in countries with moderate degrees of centralization may be more resilient to shocks, as their effects are shared across firms connected by the CB network. The key mechanism at play is the endogenous level of wage floors across different networks: moderate levels of centralization result in wage floors that have significant bite and also connect many local labor markets.

This paper contributes to a recent literature studying the effects of bargaining centralization on the labor market adjustment to shocks. Barth et al. (2023) find that the effect of import competition from China on employment was stronger in European countries with uncoordinated wage bargaining. The authors point out that wage coordination may work as an insurance device against the risk of shocks. My model does not allow for cross-union wage coordination, but it does incorporate a risk-sharing mechanism as local shocks are not fully transmitted to wages. Countries with coordinated bargaining also tend to have more centralized bargaining, so it is possible that the mechanisms proposed in this paper explain part of Barth et al.'s (2023) result.[34] Ronchi and di Mauro (2017) find that firms in countries with more centralized bargaining faced

[34] The authors use a measure of wage coordination from Visser (2019). This dataset also provides a measure of the degree of centralization of bargaining that is positively correlated with wage coordination.

stronger wage rigidity during the Great Recession, resulting in employment losses. This finding seems to contradict Barth et al. (2023), who find better employment performance in countries with more centralized bargaining. Studying the effects of these shocks in a unified framework, such as the one proposed in this paper, may help reconcile these results.

This paper also contributes to literature studying the effects of bargaining centralization on unemployment and the spatial allocation of labor. Calmfors and Driffill (1988) argue that extreme degrees of centralization of bargaining perform best in terms of unemployment rates.[35] The evidence for this theoretical prediction has been mixed (see Moene, Wallerstein, and Hoel 1993 for a review). Boeri et al. (2021) compare wage-setting institutions in Italy and Germany, and find that the more centralized Italian system results in higher unemployment in low-productivity regions. In light of these findings, the results in this paper highlight the potential beneficial effects of centralization in dissipating the effects of shocks across regions, leading to risk sharing among firms connected by the CB network. However, if the level of centralization is too high the extent of risk sharing will be lower, as the bite of wage floors is endogenously lower.

3.9 Conclusions

This paper studies the role of collective bargaining (CB) in mediating the effects of shocks. I find that firms respond to product-demand shocks to other firms in the same CB unit, indicating that shocks propagate through CB. The results are consistent with collective rent-sharing: when average economic conditions among covered firms improve, unions negotiate higher wage floors that affect wages and employment among all firms in the CB unit. Additionally, I find that wages respond more strongly to CB shocks than to comparable firm shocks. Although this result is robust across several specifications, it may not readily generalize to other settings. In particular, the strength of unions in Argentina and the frequent nominal wage adjustments during the study period could be factors that contribute to the strong response of wages to CB shocks.

The empirical findings suggest that CB may play an important role in mediating the effects of shocks. However, they are uninformative about the effects of shocks under alternative CB networks. To study this question, I develop and estimate a structural model of the labor market with CB. The model illustrates how the CB network can affect the propagation of shocks, leading to a sharing of risk across firms. Interestingly, I find a hump-shaped relationship between the degree of shock propagation and the degree of centralization of bargaining, suggesting that wage responsiveness to local shocks is lower with intermediate levels of cen-

[35]Their key insight is that medium degrees of decentralization will generate negative externalities in other sectors that are ignored by unions, whereas these negative effects will be internalized by the competitive forces of decentralized bargaining or by the bargains of a monopolistic union.

tralization. This can be explained by the extent to which firms are constrained by the wage floor across CB networks.

This article opens up several avenues for future research. First, the article does not speak to the value for workers of decreasing the risk of shocks. This might be important to quantify the welfare contribution of the insurance provided by sectoral CB. Second, the analysis abstracts from several features of labor markets that may be important to determine the role of CB in wage inequality, such as worker heterogeneity. Third, the analysis ignores potential effects of CB on informality, which may be particularly important in developing countries. Addressing these questions can provide a more comprehensive understanding of the impacts of different CB structures on the labor market.

APPENDIX

3.A Proofs for Theoretical Framework

Proof of Proposition 11. The Nash split equation is given by

$$\sum_{j\in\mathcal{J}} w_j \ell_j = \omega \sum_{j\in\mathcal{J}} \varphi_j f\left(\ell_j\right),$$

where $w_j = \underline{w}$ and $\ell_j = \ell_j(\underline{w}, \varphi_j)$ for constrained firms and $w_j = w(\varphi_j)$ and $\ell_j = \ell_j(w_j)$ for unconstrained ones.

Let $\mathcal{J}^{\text{co}}$ be the non-empty set of constrained firms, and $\mathcal{J}^{\text{uco}} = \mathcal{J} \setminus \mathcal{J}^{\text{co}}$ the set of unconstrained firms. Assume that ω is fixed. Differentiating with respect to φ_j and $\underline{w}$, and reordering terms yields

$$\begin{aligned} \sum_{j\in\mathcal{J}^{\text{co}}} \left(\ell_j + \underline{w}\frac{d\ell}{dw_j} - \omega\varphi_j f_\ell \frac{d\ell_j}{dw}\right) d\underline{w} &= \omega \sum_{j\in\mathcal{J}} f(\ell_j) d\varphi_j \\ + \sum_{j\in\mathcal{J}^{\text{co}}} \left(\omega\varphi_j f_\ell - \underline{w}\right) \frac{d\ell_j}{d\varphi} d\varphi_j &+ \sum_{j\in\mathcal{J}^{\text{uco}}} \left(\varphi_j f_\ell \frac{d\ell_j}{dw} - \ell_j + w_j \frac{d\ell_j}{dw_j}\right) \frac{dw_j}{d\varphi_j} d\varphi_j. \end{aligned} \tag{3.14}$$

where $L = \sum_{j'\in\mathcal{J}} \ell_{j'}$ is aggregate employment in the CBA.

From the first order conditions of the firm problem we know that $\ell_j + w_j \frac{d\ell_j}{dw_j} = \varphi_j f_\ell \frac{d\ell_j}{dw_j}$ for all $j \in \mathcal{J}^{\text{uco}}$, and that $\underline{w} = \varphi_j f_\ell$ for all $j \in \mathcal{J}^{\text{co}}$. I will add and subtract terms to drop the terms involving f_ℓ. I also substitute $d\underline{w} = \underline{w} d\ln\underline{w}$ and $d\varphi_j = \varphi_j d\ln\varphi_j$, and construct elasticities by multiplying and diving by appropriate terms. This yields

$$\sum_{j\in\mathcal{J}^{\text{co}}} \underline{w}\ell_j \left(1+(1-\omega)\eta_j\right) d\ln\underline{w} = \omega \sum_{j\in\mathcal{J}} \varphi_j f(\ell_j) d\ln\varphi_j$$
$$- (1-\omega) \sum_{j\in\mathcal{J}^{\text{co}}} \underline{w}\ell_j\rho_j^{\ell} d\ln\varphi_j - (1-\omega) \sum_{j\in\mathcal{J}^{\text{uco}}} w_j\ell_j \left(1+\eta_j\right) \rho_j^{w} d\varphi_j,$$

where $\eta_j = \frac{d\ell_j}{dw}\frac{w}{\ell_j}$, $\rho_j^{\ell} = \frac{d\ell_j}{d\varphi}\frac{\varphi_j}{\ell_j}$, and $\rho_j^{w} = \frac{dw_j}{d\varphi_j}\frac{\varphi_j}{w_j}$ are elasticities. Let us now define the adjusted wage bill

$$\tilde{WB}^{\text{co}} = \sum_{j\in\mathcal{J}^{\text{co}}} \underline{w}\ell_j \left(1+(1-\omega)\eta_j\right). \tag{3.15}$$

Using this definition, recalling that $R_j = \varphi_j f(\ell_j)$ and $WB_j = w_j\ell_j$, and dividing and multiplying appropriately to obtain shares, we can write

$$d\ln\underline{w} = \frac{WB}{\tilde{WB}^{\text{co}}} \sum_{j\in\mathcal{J}} s_j^R d\ln\varphi_j - (1-\omega_c)\frac{WB}{\tilde{WB}^{\text{co}}}\left[\sum_{j\in\mathcal{J}^{\text{co}}} s_j^{WB}\rho_j^{\ell} d\ln\varphi_j - \sum_{j\in\mathcal{J}^{\text{uco}}} s_j^{WB}\left(1+\eta_j\right)\rho_j^{w} d\varphi_j\right].$$

Defining the elasticity of the j's wage bill to the productivity shock φ_j as $\iota_j = \rho_j^{\ell}$ for constrained firms and $\iota_j = \left(1+\eta_j\right)\rho_j^{w}$ for unconstrained firms, we can use the previous expression to obtain equation (3.4).

Now, if ω is not fixed, then (3.14) will have an extra term on the right hand side, namely

$$R\left(\frac{d\omega}{d\underline{w}_c} + \sum_j \frac{d\omega}{d\varphi_j}\right) = R\omega(1-\beta)\left(\frac{d\left[\left(-\frac{d\Pi}{d\underline{w}}/\frac{dU}{d\underline{w}}\right)\right]}{d\underline{w}} + \sum_j \frac{d\left[\left(-\frac{d\Pi}{d\underline{w}}/\frac{dU}{d\underline{w}}\right)\right]}{d\varphi_j}\right), \tag{3.16}$$

where $R = \sum_j \varphi_j f(\ell_j)$ is the aggregate revenue. After the corresponding algebraic manipulations, the final expression will resemble equation (3.4) but with an additional term that corresponds to (3.16) divided by $\tilde{WB}^{\text{co}}$.

□

Proof of Proposition 12. The results follow directly from the effects of the wage floor on firms discussed in Section 3.2.1. □

3.B Details on Context and Data

3.B.1 Labor market institutions in Argentina

The Law of Labor Contracts (N° 20.744) sets the general standards for all labor relations. Above this base, a set of CB agreements (CBAs) establishes standards that are binding for subgroups of workers in different industries, occupations, and firms. Private-sector CBAs are governed by the regime in Law N° 14.250, first sanctioned in 1953. Different regimes regulate CBAs for government employees and educators.

The CBAs are negotiated between unions and employer associations, and sometimes they are adhered to by other unions that did not participate directly in the negotiations. The government serves as a mediator and legal validator of these agreements. The terms established in CBAs set minimum standards for workers, which individual firms cannot alter to the workers' detriment.

Types of unions. The law enables any group of workers to form a union. Unions exist in 3 legal forms: basic unions (*sindicatos*), which directly represent workers, are the most common; federations (*federaciones*), which are groups of unions; and confederations (*confederaciones*), which agglomerate federations and basic unions.

Despite this freedom of association, not all unions are legally allowed to negotiate CBAs. Only one union per "area of representation" is endowed with "bargaining privileges." The government grants privileges to the union that meets certain requirements, such as being the one with more affiliates among the workers it aims to represent.[36] It is not uncommon to find unions with bargaining privileges that simply adhere to existing CBAs. An example is the retail sector (*comercio*): its most important CBA (0130/75) is adhered by many regional basic unions. Furthermore, a single union can participate in multiple CBAs and a single CBA can have multiple adhering unions. In the paper I focus on the role of CB units, abstracting from these complexities.

Areas of representation and coverage. An area of representation can be determined by industry, occupation, geographical location, or even a single employer, and is formally defined when the government grants bargaining privileges to a union. Areas of representation effectively delimit the scope of CB units. The government has the authority to change these areas by granting new bargaining privileges or revoking existing ones. However, areas of representation have been stable in the recent past, especially in mature

[36] Unions that are registered but do not have bargaining privileges are known as *sindicatos simplemente inscriptos* (roughly, "unions simply registered"), while those with privileges are *sindicatos con personería gremial* ("unions with legal recognition"). The criteria for assigning *personería gremial* to unions are outlined in Law N° 23.551.

sectors exposed to international trade. CBAs signed within these areas of representation are binding for all workers and firms within them. This is so because the law establishes "universal coverage," meaning that CBAs are automatically extended to workers not affiliated with the union, and "automatic extension," meaning that CBAs are automatically extended to all firms in the area of representation.

CBAs, CBA alterations, and the negotiation process. A union with bargaining privileges and an employer association will typically negotiate a comprehensive CBA that outlines labor regulations applicable to the workers they represent. I refer to these as "master CBAs," or simply CBAs when the context is clear. Procedural rules for the negotiation process, established by law, define protocols for unions to formally request meetings with employers, facilitate information exchange (e.g., employers providing details about labor costs and organizational structures), among other considerations.[37] The use of strikes, as well as the procedure in case of employer crisis, is regulated by law as well. If an agreement is not reached, the government can issue an arbitral award to determine the regulations for the labor contracts of the involved workers.

New agreements are legally validated by the Ministry of Labor in a process termed *homologación*. The government archives these master CBAs under unique codes, which align with the CBA codes I observe in the data. A master CBA may be modified by either a new master CBA that supersedes it, or a "CBA alteration" that simply updates some provisions within it.[38] While master CBAs have an expiration date, even if they are not renegotiated they remain in force until a new master CBA replaces them due to a clause known as "ultra-activity." CBA alterations act as amendments to the master CBA and are negotiated more frequently. They typically relate to updates in wage scales, although they may also entail modifications in other provisions.

The dynamics of negotiations. Panel (d) of Figure 3.1 shows the dynamics of collective negotiations in 2005–2019. After a period of low activity in the 90s, the number of negotiations reignited in the early 2000s.[39] The recovery from the 2001-2002 crisis triggered government interventions affecting private sector wages, such as minimum wage increases and even wage supplements by decree. These developments revitalized the negotiations, which at first incorporated these provisions into existing CBAs. The new ruling party, elected in 2003, introduced legislation that further encouraged negotiations, galvanizing unions into signing new master CBAs. These factors account for the peak of 150 new master CBAs signed in 2006. By 2014, 52% of active

[37]The procedural rules for collective bargaining are established in Law N° 23.546.

[38]If the CBA is completely revised the code of the CBA in the data changes as well. I reviewed these cases so that a constant code appears in my data for the same CB unit.

[39]The 90s negotiations took place in a context of pro-market reforms that weakened traditional unions. See Palomino and Trajtemberg (2006) for a discussion of the dynamics of negotiations in 1991–2006.

master CBAs had been signed in 2003 or later (**TrajtembergPontoni2017**). However, soaring inflation since 2007 prompted unions and employers to meet nearly annually to revise wages via CBA alterations, resulting in a steep increase in the number of CBA alterations.[40]

3.B.2 Data

3.B.2.1 Main labor market data

The primary source of information on the formal labor market is Argentina's matched employer-employee dataset. The data are collected by the tax authority for social security purposes under a system known as *Sistema Integrado Previsional Argentino* (SIPA). I have gained access to a version of this data maintained by the Ministry of Labor (**MinisterioTrabajo2022sipa**), covering 2007–2020. The data contain worker identifiers, worker monthly compensation, and worker characteristics such as age and gender. I also observe firm identifiers, fiscal province and postal code, and their 6-digit economic sector which corresponds to a custom version of the ISIC classification system, version 4.[41] While the dataset does not contain information on hours or full-time status, it does contain a "hiring modality." This variable contains tens of categories, but the most common one (number 8) usually corresponds to full-time workers under a permanent contract. However, as suggested by Appendix Figure 3.F, part-time workers are oftentimes declared under the main hiring modality as well.

I use a second administrative dataset to obtain additional information on labor relations (Ministerio de Trabajo, Empleo y Seguridad Social, 2022). This dataset, collected also by the tax authority under a system known as *Simplificación Registral*, is constructed from employer's online declarations during hiring or termination of workers. The system was introduced progressively since 2008. Appendix Table 3.A lists the size thresholds that determined whether firms were required to or had the option of entering the system at different times. The idea was to accommodate larger firms who might need more time to adjust to the new system.

The goal of the new system was to simplify the process of registering workers and to collect information that could be used to, for example, determine eligibility for government programs or family allowances. The dataset contains the CBA code, the within-CBA category, and an occupation code, among other variables that I did not get access to for the paper.

[40]The 2017 dip in the number of CBA alterations can also be attributed to inflation-related developments. Encouraged by the government, some CB units introduced a "trigger clause" that would automatically update wages in case of unanticipated inflation, thus reducing the need to negotiate.

[41]From this sector code I compute 2- through 4-digit codes as needed. The 1-digit code I use in the paper corresponds to the "letter" of the ISIC codes, and is the broadest category available.

Importantly, both datasets contain the same worker and firm identifiers. As a result, I can use *Simplificación Registral* to add information on the CBA code to the matched employer-employee dataset. I describe the processing of the CBA code variable later in this appendix.

Table 3.A: Mandatory usage of *Simplificación Registral* by firm employment

Period	**Threshold (employment)**
January 2008 to July 2012	10
August 2012 to March 2013	25
April 2013 to March 2014	100
April 2014 to March 2015	200
April 2015 to April 2016	300
May 2016 to July 2017	400
August 2017 to November 2017	600
December 2018 to July 2018	2000
August 2018 onwards	Any

Source: Resolución general AFIP 4265/2018.

3.B.2.2 Other data sources

First, I collected data from the publicly available BACI-CEPII dataset (Gaulier & Zignago, 2010), which contains yearly trade flows between any pair of countries in each 6-digit product from the Harmonized System (HS) of product classification. In particular, I use the data coded with the 2007 version of the HS system, which covers 2007–2020. Second, I obtained data from Argentine customs (*Dirección General de Aduanas*) which details the value exported to each country and product for every Argentine firm. As a member of Mercosur, Argentina's product classification system is based on the *Nomenclatura Común del Mercosur* (NCM), which is an 8-digit code that is compatible with the HS. Using concordance tables from Liao et al. (2020), I convert NCM codes into 6-digit 2007 HS codes.[42] I use names to match country codes between the Customs data and BACI-CEPII. Then, I join the datasets using country and HS codes. Because this matching is imperfect 0.81 percent of exporting firms in 2011–2012 have less than 99 percent of their exporting value matched to a country-product. I drop these firms from the analysis.

To study how a firm's accounting outcomes respond to economic shocks, I use survey data from a national business survey. This survey, known as *Encuesta Nacional de Dinámica de Empleo e Innovación* (ENDEI), was conducted jointly by the Ministry of Labor and the Ministry of Science and Technology in two waves, in 2012 and 2016. The survey asked about the firm's situation in regard to innovation in the 3 previous years. Fortunately, the survey also asked about revenue and expenditures. Some firms were sampled twice, and are

[42]To convert codes I proceed as follows. First, I convert NCM 8-digit to NCM 6-digit by keeping the first 6 digits. These 6 digits directly correspond to the HS, although not necessarily the 2007 version. I then convert codes in different years to HS 6-digit version 2007 using the appropriate concordance table. I impute a handful of codes that are not present in the concordance manually to minimize missing values.

thus observed for 6 years. I identify 1,800 firms that participated in the survey at least once and exported in 2011–2012, which accounts for 22.6% of the firms in my primary estimation sample (as seen in Appendix Table 3.C).

3.B.2.3 Imputation of CBA codes

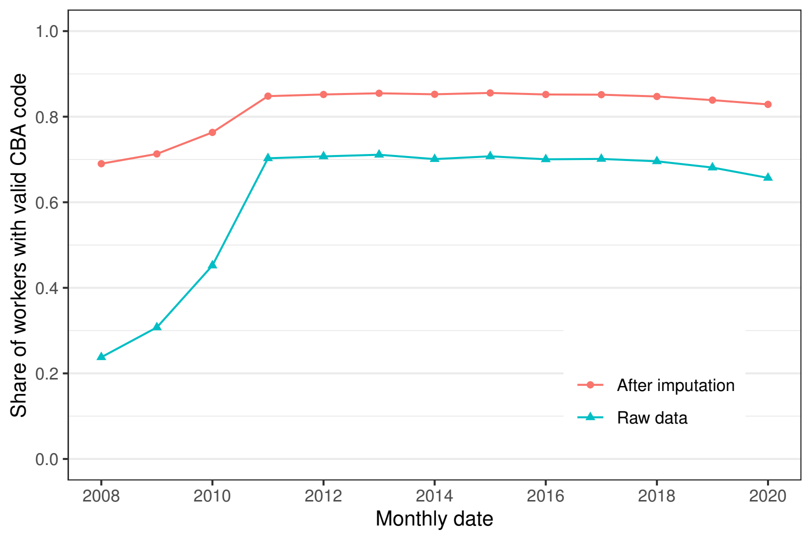
Cleaning CBA codes at worker-firm level. Due to the progressive introduction of *Simplificación Registral* many workers in SIPA do not have a CBA code. Additionally, sometimes the CBA code is outdated because a new master CBA was signed that supersedes the old one.

To increase the number of workers with a CBA code and update the codes so that they reflect a constant CB unit, I proceed in three steps. First, in the early years of the system most workers are observed by their termination date only. I use this information to fill the declared CBA code backwards, which increases coverage quite a bit for 2008 through 2010. Second, I impute the CBA code to workers with a missing code in a firm-year cell if a single code is observed. If a single CBA code is observed in increasingly large cells (such as firm-occupation, 6-digit ISIC-postal code, and occupation-postal code), I impute that code to workers with a missing code in that cell. Finally, I update the CBA codes forward to take into account updates to master CBAs that result in a new code. To do so, I scraped data from an online search engine of CBAs constructed by the Ministry of Labor. The search engine tells the user whether a master CBA updates a previous one. Results were manually reviewed to ensure that the information was correct.

Appendix Figure 3.A shows the share of workers in the employer-employee dataset with a non-missing CBA code in the raw data and after the imputation described below. The imputation increases coverage significantly. The most important steps in the imputation are the backwards filling, which raises coverage in the yearly years, and the imputation using firm-year cells, which increases coverage by around 13% after 2010.

Defining CB units. For firms employing workers across multiple CBAs, I assign as primary CBA code the modal CBA code. These primary CBA codes correspond to CB units. If a firm does not have any workers associated with a CBA code, I assign the most frequent CBA code in the postal code and 4-digit economic sector. A few postal codes and 4-digit sector cells do not have any worker with a declared CBA code. In such cases I use wider cells defined by province and 4-digit economic sector. About 25% of codes across the economy are imputed. Among firms with a non-imputed CBA code, the average share of workers with the primary CBA code is 97%.

Figure 3.A: Share of workers with non-missing CBA code, raw data



Notes: The figure shows the share of workers in the employer-employee dataset that can be matched to a valid CB agreement code.

3.C Computation of Wage Floors

The data lacks information on the wage floors set by the CBAs. I therefore use the distribution of wages within a CBA, worker category, CBA-region, and month to infer the wage floors.[43] I use workers declared in the main hiring modality and exclude the first and last month of a spell as well as the months of June and December, as these months correspond to the 13th-month salary payments. Then, I smooth the resulting time series of wage floors to reduce noise.

Figure 3.B shows the distribution of wages for workers in the main hiring modality within two categories of a given CBA, CBA-region, and month. We observe a clear bunching of wages at the wage floor. There is a significant mass of workers earning below the wage floor, which is to be expected as we do not control for hours worked and simply observe the total monthly wage received by the worker.[44] We also observe a smaller bunching point at around half the wage floor, which corresponds to workers that actually work part-time.

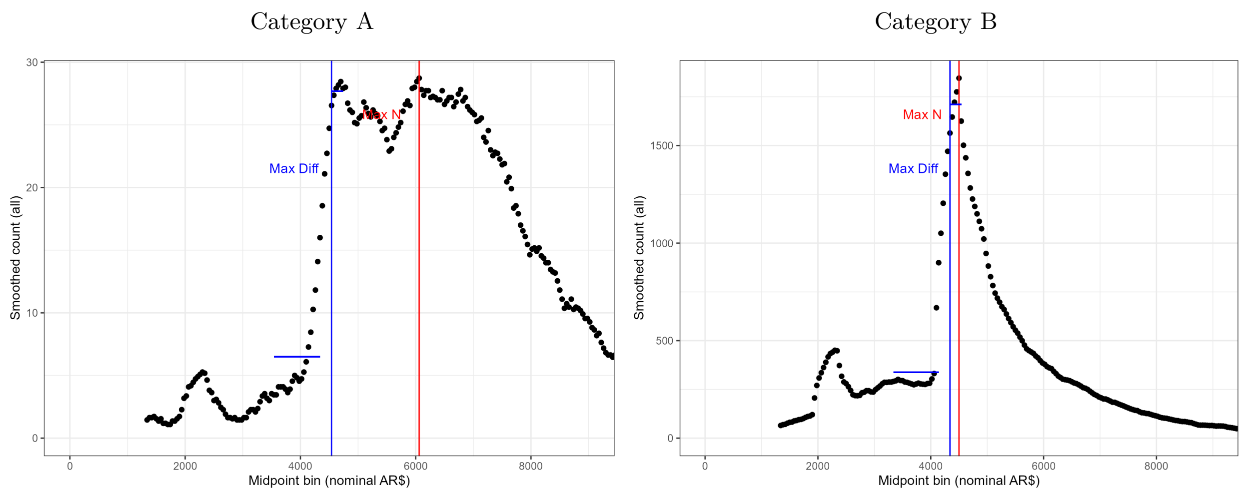
The goal is to identify the bunching point. One possible way is to follow the approach suggested by Cardoso and Portugal (2005) and pick the mode of the distribution. However, as shown by the left panel of Appendix Figure 3.B, the mode may be higher than the wage floor. A second option is to pick the point at which the distribution increases more rapidly. I implement this approach by selecting the bin at which the

[43] Card and Cardoso (2022) observe wage floors but not within-CBA categories. They rely on a lengthy matching process of within-CBA categories to occupations in the administrative data to assign wage floors to workers.

[44] Some reasons people may earn less than the wage floor are part-time workers declared in the main hiring modality or workers on vacation not receiving the usual pay supplement for attendance.

difference between a 20-bin average and the subsequent 5-bin average is maximized. This is the preferred approach as it seems to capture the actual wage floor in a variety of cases in which the mode is not a good approximation.

Figure 3.B: Distribution of wages within a CBA and CBA-region, Jan. 2012



Notes: The figures show the distribution of wages within a CBA, category, CBA region, and month. The bins are equal to 20 pesos in 2012, but change with the inflation rate in subsequent years. The bins were smoothed using a moving average with a window of 5 bins at each side. The blue line shows the "maximum difference" between a 20-bin average with respect to the subsequent 5-bin average. The red line shows the mode.

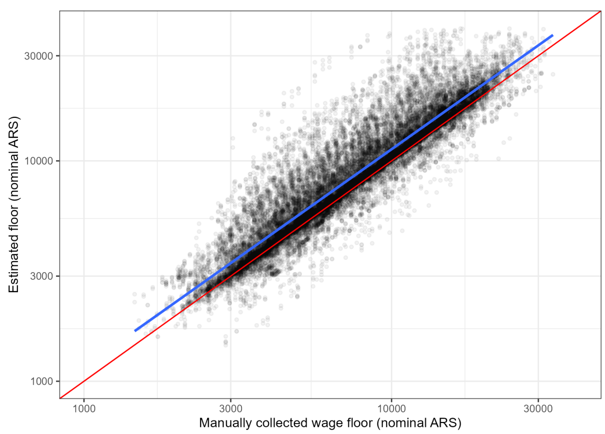
Now, after identifying the wage floors, I end up with a monthly time series of wage floors for each CBA, category, and CBA region cell. I start by dropping cells that appear for less than 3.5 years. I also drop categories that show an "implausible" behavior, i.e., for which the wage floor decreases for a period of time and then increases again. These restrictions attempt to drop the noisy estimated wage floors from CBAs with few workers. Finally, I smooth the time series of wage floors using a fixed effects model that imposes that log wage floors are a linear function of CBA by category by CBA region fixed effects and CBA-region by month fixed effects. The structure of most contracts is such that the relative difference between categories is constant over time and all wage floors increase at the same rate. The fixed effects model captures this structure, reducing the noise in the data. I conclude by smoothing each series with a 1-month moving average.

Appendix Figure 3.C shows a comparison between manually-collected wage floors and the wage floors inferred from the distribution of wages. The manual collection is challenging as it requires reading the actual agreements, which are usually in PDF format and do not follow a consistent structure over time. Furthermore, it is common for agreements to include non-compensatory payments, or one-time payments, that are hard to identify. Panel (a) shows that the levels of the wage floors are similar, with the data-inferred wage floors being slightly higher (suggesting that the manual collection may be missing some mandatory non-compensatory payments). The comparison is similar when using the average wage floor in each semester, as shown in Panel (b). Finally, Panel (c) compares the changes in the wage floors across semesters. The manually-collected wage floors exhibit a higher volatility than the data-inferred wage floors. This is likely due to the fact that the manual collection is missing some form of compensation. For example, if the manual collection missed the update of the wage scales in a given period, then it would appear to jump by a larger amount in the next wage update.

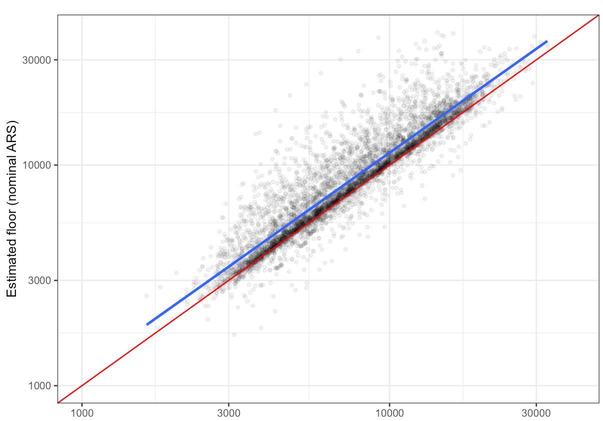
These data sources are in approximate agreement, suggesting that the data-inferred wage floors serve as a good approximation to the actual floors set by CBAs. I acknowledge that they may be noisy. However, this noise is unlikely to be correlated with the CB shocks, and therefore should not affect the empirical results of the paper.

Figure 3.C: Validation of wage floors with manually-collected wage floor data

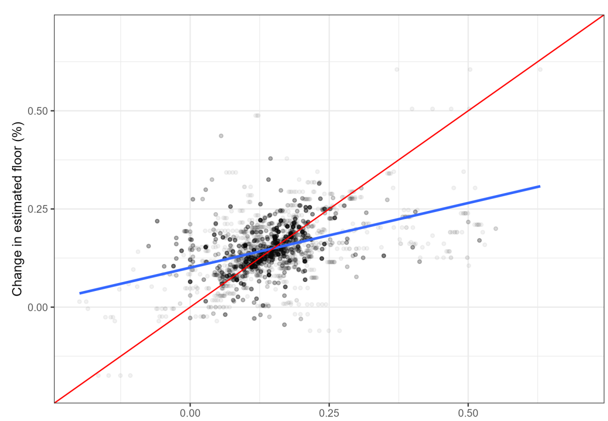
(a) Monthly comparison



(b) Semester average comparison



(c) Changes in semester average



Notes: The figure compares the manually-collected wage floors and the data-inferred wage floors. Panel (a) shows a monthly comparison of the wage floors. Panel (b) shows a similar comparison but using the average wage floor in each semester. Panel (c) compares the changes in the semester average wage floor.

3.D Shift-share Identification with Two Treatments

In this section I will show that identification of equation (3.5) can be cast in terms of country-product shocks, following Borusyak, Hull, and Jaravel (2022). For simplicity, I consider two time periods and assume a single intercept for all firms. The reasoning would go through with more time periods and local labor market intercepts, but the notation would be more cumbersome.

Consider the following causal model for the change in outcome y_j

$$\Delta y_j = \beta_1 z_{j1} + \beta_2 z_{j2} + \varepsilon_j, \tag{3.17}$$

where Δy_j is the demeaned change in y for firm j and z_{j1} and z_{j2} are defined as

$$z_{jn} = \sum_{p \in \mathcal{P}} s_{jpn} f_p$$

for $n \in \{1, 2\}$. s_{jpn} are exposure shares (with $\sum_{p \in \mathcal{P}} s_{jpn} = 1$) and f_p are common shifts. This setting aligns with the main paper since we have two shift-share variables (the firm and CB shocks) that differently weight the same set of country-product demand shocks. Given this non-*iid* setting, I consider identification of β_1 and β_2 by the full-data moment conditions

$$\mathrm{E}\left[\frac{1}{|\mathcal{J}|}\sum_{j \in \mathcal{J}} z_{j1}\varepsilon_j\right] = 0 \quad \text{and} \quad \mathrm{E}\left[\frac{1}{|\mathcal{J}|}\sum_{j \in \mathcal{J}} z_{j2}\varepsilon_j\right] = 0 \tag{3.18}$$

where ε_j is the residual from (3.17) and $\mathcal{J}$ is the set of firms. When these conditions hold the parameters are identified (provided the shift-share variables are not perfectly collinear).

To show that an assumption on shocks f_p allow identification of the parameters β_1 and β_2, rewrite the moment conditions as

$$\begin{aligned} 0 &= \mathrm{E}\left[\frac{1}{|\mathcal{J}|}\sum_{j \in \mathcal{J}} z_{jn}\varepsilon_j\right] = \mathrm{E}\left[\frac{1}{|\mathcal{J}|}\sum_{j \in \mathcal{J}} \left(\sum_{p \in \mathcal{P}} s_{jpn} f_p\right)\varepsilon_j\right] = \mathrm{E}\left[\sum_{p \in \mathcal{P}} f_p \frac{1}{|\mathcal{J}|}\sum_{j \in \mathcal{J}} s_{jpn}\varepsilon_j\right] \\ &= \mathrm{E}\left[\sum_{p \in \mathcal{P}} s_{pn} f_p r_{pn}\right], \end{aligned}$$

where $n \in \{1, 2\}$, $s_{pn} = (1/|\mathcal{J}|)\sum_{j \in \mathcal{J}} s_{jpn}$ are shock-level weights (where $\sum_p s_{pn} = 1$ as well), and $r_{pn} = \frac{(1/|\mathcal{J}|)\sum_{j \in \mathcal{J}} s_{jpn}\varepsilon_j}{(1/|\mathcal{J}|)\sum_{j \in \mathcal{J}} s_{jpn}}$ are average residuals for each category p weighted by appropriate shares. These conditions define a p-level GMM problem.

The key sufficient assumption for identification is $\mathrm{E}\left[f_p|s,r\right]=\mu$, which amounts to a quasi-randomness assumption on the shocks f_p with respect to the shares $s=\{s_{pn}\}_{p,n}$ and shocks $r=\{r_{pn}\}_{p,n}$, and is analogous to Assumption 1 in Borusyak, Hull, and Jaravel (2022).[45] This assumption guarantees that the previous conditions hold since

$$\mathrm{E}\left[\sum_{p\in\mathcal{P}} s_{pn} f_p r_{pn}\right]=\mathrm{E}\left[\sum_{p\in\mathcal{P}} s_{pn}\,\mathrm{E}\left[f_p|s,r\right] r_{pn}\right]=\mu\,\mathrm{E}\left[\sum_{p\in\mathcal{P}} s_{pn} r_{pn}\right]=0.$$

The first equality follows from the law of iterated expectations, and the second from the fact that $\mathrm{E}\left[\sum_{p\in\mathcal{P}} s_{pn} r_{pn}\right]=0$.[46] Given that the moment conditions of the p-level problem are equivalent to the moment conditions (3.18), the parameters β_1 and β_2 in (3.17) are identified.

The interpretation of the quasi-randomness assumption is that the shifts f_p have the same mean μ regardless of the realization of the unobservables r_{pn} (and shares s_{pn}). As discussed by Borusyak, Hull, and Jaravel (2022), this assumption can be relaxed by the inclusion of controls. First, adding controls in (3.17) allows one to remove variation in the error term that may be correlated with the shift-share variables. For instance, if one thinks that firms in some industries tend to both grow faster and also experience increases in world demand, then controlling for industry fixed effects will mean that shocks f_p must be quasi-random with respect to the within-industry residuals. Second, controlling for average exposure to clusters of p's allows the average of f_p to differ by cluster, relaxing the quasi-randomness assumption as well. I take these two points as a motivation for the inclusion of controls in the main paper. We see that the logic of Borusyak, Hull, and Jaravel (2022) applies to the case of two shift-share variables that use the same set of shocks but weighted by different shares.

3.E The effects on shocks on revenue in an event-study design

This section describes the estimates of the effects of firm shocks on firm revenue and employment. This section uses data from a survey of businesses (ENDEI) conducted jointly by the Ministry of Labor and the

[45] A second assumption, in the spirit of Assumption 2 in Borusyak, Hull, and Jaravel (2022), is required for consistency. Namely, that $\mathrm{E}[s_{pn}^2]\to 0$ for $n\in\{1,2\}$ and $\mathrm{Cov}\left(f_p,f_{p'}|s,r\right)=0$ for all (p,p') with $p\neq p'$. This assumption requires a large effective number of shocks, and that shocks are uncorrelated given the unobservables and shares. The authors discuss ways in which these assumptions can be relaxed.

[46] To show this result recall that $\sum_{p\in\mathcal{P}} s_{jpn}=1$ and $\mathrm{E}\left[(1/|\mathcal{J}|)\sum_{j\in\mathcal{J}}\varepsilon_j\right]=0$ by construction. Then, $\mathrm{E}\left[\sum_{p\in\mathcal{P}} s_{pn} r_{pn}\right]=\mathrm{E}\left[\sum_{p\in\mathcal{P}}(1/|\mathcal{J}|)\sum_{j\in\mathcal{J}} s_{jpn}\varepsilon_j\right]=\mathrm{E}\left[(1/|\mathcal{J}|)\sum_{j\in\mathcal{J}}\varepsilon_j\left(\sum_{p\in\mathcal{P}} s_{jpn}\right)\right]=0.$

Ministry of Science and Technology in the years 2010–2016 (excluding 2013).

3.E.1 Empirical Strategy

Consider the following panel model:

$$y_{jt} = \theta z_{c(j)t} + \beta z_{jt} + \alpha_j + \delta_{\ell(j)t} + \varepsilon_{jt}$$

where variables are defined as in equation (3.5). This model imposes that only the period-t shocks affect the outcome. However, it is possible that the effect of the shock takes time to fully materialize. To account for this, I follow Freyaldenhoven et al. (forthcoming) and add a specific transformation of the leads and lags of the shock variables into the model:

$$\begin{aligned} y_{jt} = \beta_{-\underline{R}-1}\left(-z_{j,t+\underline{R}}\right) + \sum_{r\in\mathcal{R}} \beta_k \Delta z_{j,t-r} + \beta_{\overline{R}}\left(z_{j,t-\overline{R}}\right) \\ + \theta_{-\underline{R}-1}\left(-z_{c(j),t+\underline{R}}\right) + \sum_{r\in\mathcal{R}} \theta_k \Delta z_{c(j),t-r} + \theta_{\overline{R}}\left(z_{c(j),t-\overline{R}}\right) \\ + \alpha_j + \delta_{\ell(j)t} + \varepsilon_{jt}. \end{aligned} \tag{3.19}$$

$\mathcal{R}$ is a set of "relative years" running from $-\underline{R}$ to $\overline{R}-1$. Given that the end-points and the first-differences of each z sum to 0, a normalization is required to avoid perfect multicollinearity.

The parameters of this model are identified by different comparisons relative to equation (3.5). The outcome at each year t is affected by a set of event-study coefficients of each treatment, which are identified by the conditional covariance of the period-t change in outcome to the period-t change in the treatment, relative to the normalized period. Future values of the treatment (i.e., forward values up to $\overline{R}$ years) pin down the pre-period coefficients. If there is no correlation between future shocks and current outcomes, then these coefficients should be zero.[47] Similarly, past values of the z's (i.e., lagged values up to $\underline{R}$ years) identify the post-period coefficients.

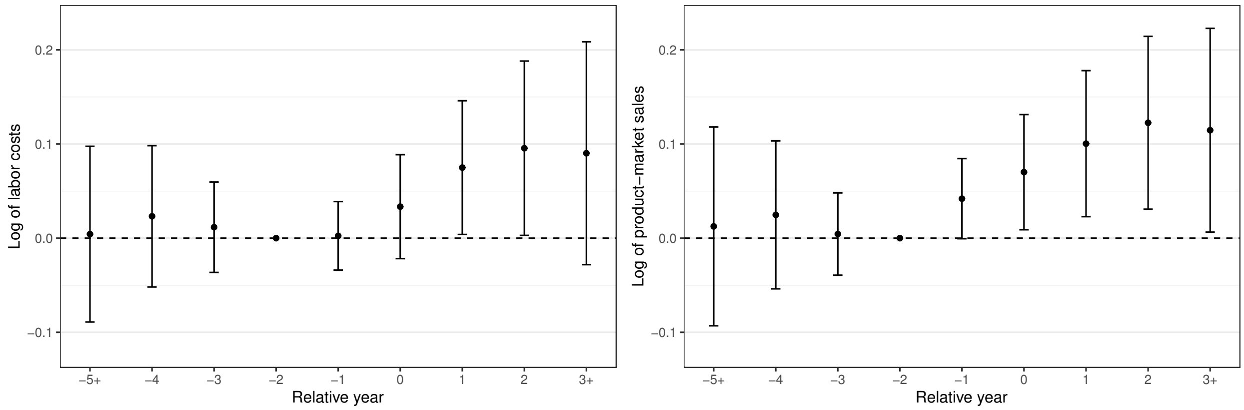
I set $\underline{R} = 4$ and $\overline{R} = 3$. Since the shock variables are observed in all years 2007–2020, this allows me to compute an entire set of event-study coefficients for any firm in the survey data in 2010–2012 or 2014–2016. For those observed in 2010, I compute post-coefficients using data starting from $2010 - \overline{R} = 2007$, and for those observed in 2016, I compute pre-coefficients using data up to $2016 + \underline{R} = 2020$. A second decision concerns the period to normalize to 0. Considering that the treatment variables exhibit some AR(1)

[47] Contrast this with the DiD model where zero pre-trends for those treated at t result from no correlation between *current* shocks and *past* outcomes.

autocorrelation, I normalize the relative year -2 for both sets of event-study variables.

Figure 3.D: Effect of firm shocks on firm's accounting outcomes, panel event-study design

(a) Log wage expenditures (b) Log product market sales



Notes: Data are from a sample of firms that exported in 2011–2012 that were surveyed in the first or second waves of the *Encuesta Nacional de Dinámica del Empleo y la Innovación* (ENDEI). The figure shows coefficients on the effect of firms shocks on a firm accounting outcomes, using a panel event-study design as described in Appendix 3.E. The time-varying firm shock z_{jt} is defined as the average world import demand across the country-products exported by the firm in 2011–2012, weighting by the share of the value exported to each country-product. The regressions include controls for firm fixed effects and 4-digit economic sector by province by year fixed effects. Standard errors are clustered at the firm level.

3.E.2 Results

Appendix Figure 3.D shows effects of the firm shock on product-market sales and labor expenditures labor using data from the survey of businesses. We observe an increase in both variables following the shock. A 10% increase in average world import demand rises product-market sales by 1.13%, and labor costs by 0.87%, on average in relative years 1–3. Both panels show stable pre-trends, supporting the underlying parallel-trends assumption of the model.

3.F Details on Structural Model

3.F.1 Derivations

3.F.1.1 Labor supply: supply to the firm decision

The cdf and pdf of a Fréchet distribution with shape η, scale equal to one and location equal to zero are given by

$$F(\xi) = e^{-\xi^{-\eta}}, \quad f(\xi) = \eta \xi^{-\eta-1} e^{-\xi^{-\eta}}.$$

Let Ω_r be the set of firms operating in region r. The share of formal-sector workers in region r who optimally choose firm j is given by

$$\begin{aligned}
\ell(j) &= \int_0^1 \Pr\left(V_{ir}(j) \geq V_{ir}(j') \forall j' \neq j\right) di \\
&= \int_0^1 \int_0^\infty f(\xi(j)) \prod_{j' \in \Omega_r \setminus \{j\}} F\left(\frac{w(j) A_{k1(j)} \xi(j)}{w(j') A_{k1(j')}}\right) d\xi(j) di \\
&= \int_0^1 \int_0^\infty f(\xi(j)) \prod_{j' \in \Omega_r \setminus \{j\}} F\left(\frac{w(j) A_{k1(j)} \xi(j)}{w(j') A_{k1(j')}}\right) d\xi(j) di \\
&= \int_0^1 \int_0^\infty \eta \xi(j)^{-\eta-1} e^{-\xi(j)^{-\eta}} \prod_{j' \in \Omega_r \setminus \{j\}} e^{-\left(\frac{w(j) A_{k1(j)} \xi(j)}{w(j') A_{k1(j')}}\right)^{-\eta}} d\xi(j) di \\
&= \int_0^1 \int_0^\infty \eta \xi(j)^{-\eta-1} e^{\int_{j' \in \Omega_r} -\left(\frac{w(j) A_{k1(j)} \xi(j)}{w(j') A_{k1(j')}}\right)^{-\eta} dj'} d\xi(j) di \\
&= \int_0^1 \int_0^\infty \eta \xi(j)^{-\eta-1} e^{-\left(w(j) A_{k1(j)} \xi(j)\right)^{-\eta}} e^{\int_{j' \in \Omega_r} \left(w(j') A_{k1(j')}\right)^{\eta} dj'} d\xi(j) di.
\end{aligned}$$

Defining $s = \frac{\left[\int_{j' \in \Omega_r} \left(w(j') A_{k1(j')}\right)^{\eta} dj'\right]^{1/\eta}}{w(j) A_{k1(j)}}$ we can rewrite as

$$\ell(j) = \int_0^1 \int_0^\infty \eta \xi(j)^{-\eta-1} e^{-\left(\frac{\xi(j)}{s}\right)^{-\eta}} d\xi(j) di,$$

and further manipulations yield

$$\ell(j) = s^{-\eta} \int_0^1 \int_0^\infty \frac{\eta}{s} \left(\frac{\xi(j)}{s}\right)^{-\eta-1} e^{-\left(\frac{\xi(j)}{s}\right)^{-\eta}} d\xi(j) di,$$

Note that, as workers are homogeneous, the integral over i is irrelevant. Also, the expression inside the integrals is the pdf of a Fréchet distribution with shape η and scale s. Thus, the double integral above equals 1, and we have

$$\ell(j) = \left(\frac{w(j) A_{k1(j)}}{\int_{j' \in \Omega_r} \left(w(j') A_{k1(j')}\right)^{\eta} dj'}\right)^{\eta}.$$

The expression for expected utility can be obtained following similar steps. See Appendix D in Parente (2022).

3.F.1.2 Labor supply: Formal employment decision

Derivations are available in Appendix D.2.1 of Ahlfeldt, Roth, and Seidel (2022b).

3.F.1.3 Labor demand: solution to firm problem

For *unconstrained* firms the solution to the firm's problem is

$$w(\varphi) = \left(\frac{\eta}{\eta+1}\right)\varphi, \quad \ell(\varphi) = W_r^{-\eta}\left(A_{k1}\left(\frac{\eta}{\eta+1}\right)\varphi\right)^{\eta}, \quad \pi(\varphi) = \frac{\eta^{\eta}}{(\eta+1)^{\eta+1}}\varphi^{\eta+1}A_{k1}^{\eta}W_r^{-\eta}.$$

For *constrained* firms we have

$$w(\varphi) = \underline{w}_c, \quad \ell(\varphi) = W_r^{-\eta}\left(A_{k1}\underline{w}_c\right)^{\eta}, \quad \pi(\varphi) = \left(\varphi - \underline{w}_c\right)\underline{w}_c^{\eta}A_{k1}^{\eta}W_r^{-\eta}.$$

3.F.1.4 Local labor markets: derivations

Recall that the cdf of productivity is $F_g(\varphi) = 1 - (\varphi/\varphi_{g0})^{\alpha}$ if $\varphi > \varphi_{g0}$ and zero otherwise. The conditional cdf on some value x can be found by simply using x instead of φ_{g0} in the expression above. Also recall the value of the thresholds is $\overline{\varphi}_g = \underline{w}_c(\eta+1)/\eta$ and $\underline{\varphi}_g = \underline{w}_c$.

Share of firms bunching at the wage floor First, if $\varphi_{g0} > \overline{\varphi}_g > \underline{\varphi}_g$ all firms pay above the wage floor, thus $S_g = 0$. Second, if $\overline{\varphi}_g > \varphi_{g0} > \underline{\varphi}_g$, then the minimum productivity is φ_{g0}. The share of firms bunching at the wage floor is

$$\begin{aligned} S_g &= F_g\left(\overline{\varphi}(\underline{w}_{c(g)})\right) - F_g\left(\varphi_{g0}\right) = \left[\left(\frac{\varphi_{g0}}{\varphi_{g0}}\right)^{\alpha}\right] - \left[\left(\frac{\varphi_{g0}}{\overline{\varphi}(\underline{w}_{c(g)})}\right)^{\alpha}\right], \\ &= 1 - \left(\frac{\varphi_{g0}}{\underline{w}_{c(g)}}\right)^{\alpha}\left(\frac{\eta}{\eta+1}\right)^{\alpha}. \end{aligned} \tag{3.20}$$

Finally, if $\overline{\varphi}_g > \underline{\varphi}_g > \varphi_{g0}$, then the minimum productivity is $\underline{\varphi}_g = \underline{w}_c$. Thus,

$$\begin{aligned} S_g &= F_g\left(\overline{\varphi}(\underline{w}_{c(g)})\right) - F_g\left(\underline{\varphi}(\underline{w}_{c(g)})\right) = \left[\left(\frac{\underline{w}_c}{\underline{\varphi}(\underline{w}_{c(g)})}\right)^{\alpha}\right] - \left[\left(\frac{\underline{w}_c}{\overline{\varphi}(\underline{w}_{c(g)})}\right)^{\alpha}\right] \\ &= 1 - \left(\frac{\eta}{\eta+1}\right)^{\alpha}. \end{aligned}$$

This is the maximum share of observed firms bunching that is compatible with the model.

Average wage For any threshold x, the average wage of *unconstrained* firms is given by

$$\begin{aligned}\bar{w}_g^u &= \int_x^\infty w(z) f_g(z|x) dz \\ &= \int_x^\infty \left(\frac{\eta}{\eta+1}\right) z\alpha x^\alpha z^{-\alpha-1} dz \\ &= \left(\frac{\eta}{\eta+1}\right)\left(\frac{\alpha}{\alpha-1}\right) x\end{aligned}$$

Then, if $\varphi_{g0} \geq \overline{\varphi}_g$ there are no constrained firms, so that

$$\bar{w}_g^u = \left(\frac{\eta}{\eta+1}\right)\left(\frac{\alpha}{\alpha-1}\right) \varphi_{g0}.$$

Now, if $\overline{\varphi}_g > \varphi_{g0} > \underline{\varphi}_g$, the minimum productivity of unconstrained firms is $\overline{\varphi}_g$, implying

$$\bar{w}_g^u = \left(\frac{\alpha}{\alpha-1}\right) \underline{w}_c.$$

This holds whether $\varphi_{g0} > \underline{\varphi}_g$ or not.

The average wage of *constrained* firms is given by $\underline{w}_c$ if there are any constrained firms. If $\varphi_{g0} \geq \overline{\varphi}_g$ no firm is constrained so this quantity is not defined.

Then, the average wage in a local labor market is given by a weighted average of the unconstrained and constrained wages, where the weights are given by the share of firms bunching. Going over the computations for each case, we get

$$\bar{w}_g = \begin{cases} \left(\frac{\eta}{\eta+1}\right)\left(\frac{\alpha}{\alpha-1}\right)\varphi_{g0} & \text{if } \varphi_{g0} > \overline{\varphi}, \\ \left(1 + \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^\alpha \left(\frac{1}{\alpha-1}\right)\left(\frac{\eta}{\eta+1}\right)^\alpha\right)\underline{w}_c & \text{if } \overline{\varphi} > \varphi_{g0} > \underline{\varphi}_g, \\ \left(1 + \left(\frac{1}{\alpha-1}\right)\left(\frac{\eta}{\eta+1}\right)^\alpha\right)\underline{w}_c & \text{if } \overline{\varphi} > \underline{\varphi}_g > \varphi_{g0}. \end{cases}.$$

Aggregate quantities To compute aggregate labor demand in g we need to solve $L_g = M_g \int_{\varphi_{g0}}^\infty \ell(\varphi) f_g(\varphi) d\varphi$. Solving the integral for each case we get:

$$L_g = M_g W_r^{-\eta} A_{k1}^\eta \begin{cases} \left(\frac{\eta}{\eta+1}\right)^\eta \left(\frac{\alpha}{\alpha-\eta}\right)\varphi_{g0}^\eta & \text{if } \varphi_{g0} > \overline{\varphi}_g, \\ \underline{w}_c^\eta \left\{1 + \left(\frac{\eta}{\alpha-\eta}\right)\left(\frac{\eta}{\eta+1}\right)^\alpha \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^\alpha\right\} & \text{if } \overline{\varphi}_g > \varphi_{g0} > \underline{\varphi}_g, \\ \underline{w}_c^\eta \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^\alpha \left\{1 + \left(\frac{\eta}{\alpha-\eta}\right)\left(\frac{\eta}{\eta+1}\right)^\alpha\right\} & \text{if } \overline{\varphi}_g > \underline{\varphi}_g > \varphi_{g0}, \end{cases}$$

and the partial derivative with respect to the wage floor, holding constant the wage index, is given by

$$\frac{\partial L_g}{\partial \underline{w}_c} = M_g W_r^{-\eta} A_{k1}^{\eta} \begin{cases} 0 & \text{if } \varphi_{g0} > \overline{\varphi}_g, \\ \underline{w}_c^{\eta-1} \eta \left\{ 1 - \left(\frac{\eta}{\eta+1}\right)^{\alpha} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \right\} & \text{if } \overline{\varphi}_g > \varphi_{g0} > \underline{\varphi}_g, \\ \underline{w}_c^{\eta-1} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} (\eta - \alpha) \left\{ 1 + \left(\frac{\eta}{\alpha-\eta}\right) \left(\frac{\eta}{\eta+1}\right)^{\alpha} \right\} & \text{if } \overline{\varphi}_g > \underline{\varphi}_g > \varphi_{g0}. \end{cases}$$

These equations imply that the effect of the wage floor on employment is hump-shaped, a result first derived by Ahlfeldt, Roth, and Seidel (2022b) in a more general model where there are also firms that can ration employment.

To compute the aggregate wage bill in g we need to solve $WB_g = M_g \int_{z^{\min}}^{\infty} w(z)\ell(z) f_g(z) dz$. The result is given by

$$WB_g = M_g W_r^{-\eta} A_{k1}^{\eta} \begin{cases} \varphi_{g0}^{\eta+1} \left(\frac{\eta}{\eta+1}\right)^{\eta+1} \left(\frac{\alpha}{\alpha-\eta-1}\right) & \text{if } \varphi_{g0} \geq \overline{\varphi}_g, \\ \underline{w}_c^{\eta+1} \left\{ 1 + \left(\frac{\eta+1}{\alpha-\eta-1}\right) \left(\frac{\eta}{\eta+1}\right)^{\alpha} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \right\} & \text{if } \overline{\varphi}_g > \varphi_{g0} \geq \underline{\varphi}_g, \\ \underline{w}_c^{\eta+1} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \left\{ 1 + \left(\frac{\eta+1}{\alpha-\eta-1}\right) \left(\frac{\eta}{\eta+1}\right)^{\alpha} \right\} & \text{if } \varphi_{g0} < \underline{\varphi}_g, \end{cases}$$

and the derivative of WB_g with respect to the wage floor, holding constant the wage index, is

$$\frac{\partial WB_g}{\partial \underline{w}_c} = M_g W_r^{-\eta} A_{k1}^{\eta} \begin{cases} 0 & \text{if } \varphi_{g0} \geq \overline{\varphi}_g, \\ \underline{w}_c^{\eta} (\eta + 1) \left[1 - \left(\frac{\eta}{\eta+1}\right)^{\alpha} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \right] & \text{if } \overline{\varphi}_g > \varphi_{g0} \geq \underline{\varphi}_g, \\ \underline{w}_c^{\eta} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \alpha \left[\frac{\eta+1}{\alpha} \left(1 - \left(\frac{\eta}{\eta+1}\right)^{\alpha}\right) - 1 \right] & \text{if } \overline{\varphi}_g > \underline{\varphi}_g > \varphi_{g0}. \end{cases}$$

Finally, aggregate revenue can be obtained from $R_g = M_g \int_{z^{\min}}^{\infty} z\ell(z) f_g(z) dz$. The solution is given by

$$R_g = M_g W_r^{-\eta} A_{k1}^{\eta} \begin{cases} \varphi_{g0}^{\eta+1} \left(\frac{\eta}{\eta+1}\right)^{\eta} \left(\frac{\alpha}{\alpha-\eta-1}\right) & \text{if } \varphi_{g0} \geq \overline{\varphi}_g, \\ \underline{w}_c^{\eta} \varphi_{g0} \left(\frac{\alpha}{\alpha-1}\right) \left\{ 1 + \left(\frac{\eta}{\alpha-\eta-1}\right) \left(\frac{\eta}{\eta+1}\right)^{\alpha-1} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha-1} \right\} & \text{if } \overline{\varphi}_g > \varphi_{g0} \geq \underline{\varphi}_g, \\ \underline{w}_c^{\eta+1} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \left(\frac{\alpha}{\alpha-1}\right) \left\{ 1 + \left(\frac{\eta}{\alpha-\eta-1}\right) \left(\frac{\eta}{\eta+1}\right)^{\alpha-1} \right\} & \text{if } \overline{\varphi}_g > \underline{\varphi}_g > \varphi_{g0}. \end{cases} \tag{3.21}$$

Once again, the partial derivative with respect to the wage floor is

$$\frac{\partial R_g}{\partial \underline{w}_c} = M_g W_r^{-\eta} A_{k1}^{\eta} \begin{cases} 0 & \text{if } \varphi_{g0} \geq \overline{\varphi}_g, \\ \underline{w}_c^{\eta-1} \varphi_{g0} \eta \left(\frac{\alpha}{\alpha-1}\right) \left[1 - \left(\frac{\eta}{\eta+1}\right)^{\alpha-1} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha-1}\right] & \text{if } \overline{\varphi}_g > \varphi_{g0} \geq \underline{\varphi}_g, \\ \underline{w}_c^{\eta} \left(\frac{\varphi_{g0}}{\underline{w}_c}\right)^{\alpha} \alpha \left[\frac{\eta}{\alpha-1}\left(1 - \left(\frac{\eta}{\eta+1}\right)^{\alpha-1}\right) - 1\right] & \text{if } \overline{\varphi}_g > \underline{\varphi}_g > \varphi_{g0}. \end{cases} \tag{3.22}$$

To solve the Nash bargaining problem, it is useful to compute the total derivative with respect to the wage floor. Let $X \in \{L, WB, R\}$ be any of the aggregate quantities defined above. Then, the total derivative is given by

$$\frac{dX_g}{d\underline{w}_c} = \frac{\partial X_g}{\partial \underline{w}_c} - \eta \frac{X_g}{W_r} \frac{dW_r}{d\underline{w}_c},$$

where $dW_r/d\underline{w}_c$ is the derivative of the wage index with respect to the wage floor.

3.F.2 Equilibrium of structural model

3.F.2.1 Definition of equilibrium

Given a CB network $\mathcal{C}$, values for $\{\{N_r\}_{r\in\mathcal{R}}, \{M_g\}_{g\in\mathcal{G}}\}$, parameters for the worker problem $\{\zeta, \{b_r\}_{r\in\mathcal{R}}, \eta, \{A_{k1}\}_{k1\in\mathcal{K}1}\}$, bargaining power parameters $\{\beta_c\}_{c\in\mathcal{C}}$, and productivity processes parameters $\{\alpha, \{\varphi_{g0}\}_{g\in\mathcal{G}}\}$, an equilibrium is defined as a vector of employment shares $\{\mu_r\}_{r\in\mathcal{R}}$, a set of wage floors $\{w_c^*\}_{c\in\mathcal{C}}$, and a set of wage indices $\{W_r^*\}_{r\in\mathcal{R}}$ such that:

1. Employment shares satisfy equation (3.8), with expected utility given by (3.7).
2. Firms at every φ choose wages given labor supply according to (3.9).
3. Wage floors simultaneously solve the Nash-in-Nash problem given by (3.13) for all $c \in \mathcal{C}$.
4. Wage indices clear the labor market in each region:

$$\sum_{g:r(g)=r} M_g \int_{\varphi_{g0}}^{\infty} \ell(\varphi) f_g(\varphi) d\varphi = \mu_r N_r. \tag{3.23}$$

Existence and uniqueness of equilibrium. Let $\hat{\Gamma} = \Gamma(\frac{\eta-1}{\eta})^{\zeta}$ and write $\hat{L}_r = \sum_{g:r(g)=r} \hat{L}_g$. Then, the market clearing condition can be written as

$$W_r^{-\eta} \hat{L}_r = \frac{\hat{\Gamma} W_r^{\zeta}}{\hat{\Gamma} W_r^{\zeta} + b_r} N_r. \tag{3.24}$$

This is a continuous function of W_r, and applying the intermediate value theorem we can conclude that a unique solution exists for any vector of wage floors.[48]

Then, it remains to discuss the equilibrium of the Nash-in-Nash problem. If all objective functions were concave, then the problem would be a concave maximization problem over a convex set, and thus a unique solution would exist. However, the objective functions may actually be decreasing at first, increasing later on, and then decreasing again. These "convex regions" would occur if, starting from a non-binding wage floor, an increase in the floor decreases the wage bill and thus the problem's objective function. This could be due to either general equilibrium effects or because some low productivity local labor markets are fully constrained by the wage floor and experience negative wage floor effects. These convex regions are problematic if they result in multiple maxima, which occurs in a knife-edge case where the level of utility with a non-binding wage floor is equal to the level of utility of a binding wage floor that is also a local maximum.

In summary, the Nash-in-Nash problem has a unique solution if all objective functions in the Nash-in-Nash problem are concave. In the case some objective function has a convex region, the problem has multiple equilibria only in a knife-edge case that is unlikely to occur in practice.

Effect of wage floor on regional wage index. A closed form solution to (3.24) does not exist. However, we can obtain an expression for the derivative of the wage index with respect to the wage floor using the implicit function theorem:

$$\frac{dW_r}{d\underline{w}_c} = \frac{\left(\hat{\Gamma} W_r^{\zeta} + b_r\right)}{\hat{\Gamma} W_r^{\zeta-1}\left[(\zeta+\eta) W_r^{\eta} N_r - \zeta \hat{L}_r\right]} \sum_{g:c(g)=c} \frac{\partial \hat{L}_g}{\partial \underline{w}_c}. \tag{3.25}$$

This expression will be positive as long as the summation is positive. I.e., a wage floor increase that increases employment will have a positive effect on the wage index.

3.F.2.2 An algorithm to compute the equilibrium

Given a set of parameters, I solve for equilibrium wage indices $\{W_r\}_{r\in\mathcal{R}}$ and wage floors $\{w_c\}_{c\in\mathcal{C}}$ using an algorithm that mixes fixed-point iteration and Gauss-Seidel's coordinate update algorithm (**Galichon2022**). The algorithm proceeds as follows:

1. Take an initial guess $\{\{W_r^0\}, \{\underline{w}_c^0\}\}$
2. Use fixed-point iteration to find equilibrium wage indices $\{W_r^1\}$ using the previous wage floors vector.

[48] Define $h(W_r) = W_r^{-\eta} \hat{L}_r - \frac{\hat{\Gamma} W_r^{\zeta}}{\hat{\Gamma} W_r^{\zeta} + b_r} N_r$. Then, $W_r \to 0$ implies that $h(0) \to \infty$ and $W_r \to \infty$ implies that $h(\infty) \to -N_r$. Thus, by the intermediate value theorem, there exists a W_r^* such that $h(W_r^*) = 0$.

3. Iteratively find equilibrium wage floors $\{\underline{w}_c^1\}$ numerically solving the Nash bargaining split for each c, using the equilibrium wage indices $\{W_r^1\}$ and the previously found wage floors.
4. Take $\{\{W_r^1\}, \{\underline{w}_c^1\}\}$ as new starting point and repeat.
5. Iterate until convergence.

I set as starting point $W_r = .5$ for every r, and $\underline{w}_c = 1.05 \times \min_{c:g(c)=c}\{z_{g0}\}$ for every c. I set as criteria that the mean absolute difference between consecutive iterations is less than 10^{-8}, both for the fixed-point iteration and for the entire loop.

Finding wage indices. Using the four labor market-clearing equations it is straightforward to write $W_r = T(W_r)$, where $T(\cdot)$ is a contraction mapping. In particular, using (3.24), we can write

$$W_r = \left(\frac{\hat{\Gamma} W_r^{\zeta} + b_r}{\hat{\Gamma} W_r^{\zeta}} \frac{\hat{L}_r}{N_r}\right)^{1/\eta}.$$

Thus, to find wage indices I use a fixed-point iteration using the most up-to-date wage floors to compute aggregate labor demand.

Finding wage floors. In this step I use the Nash bargaining FOCs, defined in equation (3.2). However, as the FOC may not always hold in a maximum, I select several candidate points for maximizers and then evaluate the objective function to select the global maximum.

The first issue with the FOCs is that the derivatives of the wage bill and revenue are discontinuous when $\varphi_{g0} = \underline{w}_c$. Fortunately, this happens only in a small share sectors, usually belonging to the largest CB units. As a result, derivatives are generally well-defined in the feasible range where the wage floor will be set. However, for different counterfactuals this might not be the case. As a result, I add all points where $\varphi_{g0} = \underline{w}_c$ for some g as candidate points for maximizers.

Second, when there are multiple local labor markets in a CB unit, the objective functions are sums of terms, some of which do not depend on the wage floor (the unconstrained ones). Because derivatives are discontinuous, when a local labor market enters this sum the FOC may change discontinuously, generating the possibility of local optima. To deal with this, I search for all wage floors at which equation (3.2) holds and add them to the set of candidate maximizers.

Finally, I evaluate candidate points and pick the one that yields the global maximum. When evaluating the objective function I adjust wage indexes to account for general equilibrium effects.

3.F.3 Defining local labor markets and aggregating data

I construct the aggregate data from the universe of firms observed in 2011–2012. I construct local labor markets, and then compute aggregate data used for model estimation.

Defining local labor markets and CB units

The goal is to avoid small local labor markets as much as possible, since these add computation time to the model without significantly affecting the conclusions of the analysis. I start by dropping broad sectors that negotiate under a different bargaining regime, such as agriculture.[49] Then, I define exporter firms as those with any exports in 2011–2012, and I group provinces in 4 regions: Centro (center), Cuyo (west), Norte (north), and Patagonia (south).[50]

The next step is to create a coarse 4-digit sector variable. First, if an exporter, region, 4-digit sector and CB unit cell has less than 10 firms, then I assign the most common 4-digit sector in the CB unit to the cell.[51] I do so to keep firms in the correct CB unit, while simplifying the large heterogeneity observed within CB units. Second, I group some 4-digit sectors to broader categories to reduce the number of sectors. I try to keep the 4-digit resolution in Manufacturing, since this is where most of the export shocks take place.[52] After these changes I still observe 10.6% of firms in coarse 4-digit sector by region cells with less than 5 firms. For these cells, I change the 4-digit sector code so that they are assigned to a related group.[53] This process results in a coarse 4-digit sector that has at least 5 firms in every category by region cell.

The final step before grouping the data is to define local labor markets and CB units. The baseline cell is the coarse 4-digit sector by region grouping created above. I further split each cell using provinces if the

[49]Specifically, I drop the broad categories, or 1-digit sectors, A (Agriculture, forestry and fishing), B (Mining and quarrying), P (Education), R (Arts, entertainment and recreation), T (Activities of households as employers), and U (Activities of extraterritorial organizations and bodies). I also drop the 4-digit sectors that correspond to waste management and employment agencies.

[50]The regions are (1) "Centro," which includes Ciudad de Buenos Aires, Buenos Aires, Córdoba, Entre Ríos, La Pampa, Mendoza, and Santa Fe; (2) "Cuyo," which includes La Rioja, Mendoza, San Juan, and San Luis; (3) "Norte," including Catamarca, Chaco, Corrientes, Formosa, Jujuy, Misiones, Salta, and Tucumán; and (4) "Patagonia," grouping Chubut, La Pampa, Neuquén, Río Negro, Santa Cruz, and Tierra del Fuego.

[51]Additionally, I define as non-exporter all firms in cells that have less than 2 employees, regardless of their actual exporting status.

[52]To be precise, I take the following steps. First, if a firm has a 4-digit sector that corresponds to any broad category *excluding* "Manufacturing" (broad category C), "Wholesale and retail trade" (G), "Transportation and Storage" (H), and "Accommodation and food service activities" (I), I group the 4-digit sector code to the broad category level. Second, for any 4-digit sector *excluding* those in "Manufacturing" I group the 4-digit sector code to the 3-digit level. Third, I group to the 2-digit level all 4-digit sectors included in 2-digit sectors that have less than 50 firms and broad categories that have more than 99 firms. Similarly, I group to the 3-digit level all 4-digit sectors included in 3-digit categories with less than 50 firms, 2-digit categories with more than 49 firms, and broad categories with more than 99 firms.

[53]More specifically, I change the 4-digit code for cells that were not grouped so far and have less than 5 firms to the most common 4-digit sector in the corresponding 3-digit grouping. There still remain 5.9% of firms in cells with less than 5 firms. I group all these 4-digit sector cells to the broad category level.

coarse 4-digit sector by region by province cell has more than 75 firms.[54] I also use a modified version of the observed CB code to split the cells. The reason to modify the CB code is, once again, to reduce the number of local labor markets and CB units in the economy. The key modification is to define "local" CB units if a given CB unit has low employment or a small share of firms observed with a wage floor.[55] Then, my local labor markets are defined by region, province, exporter status, coarse 4-digit sector, and CB unit. The CB network is simply defined by the modified CB unit codes.

Aggregating data

I group the firm-level data to the local labor market level and compute aggregate statistics. I compute the mean **wage** and **wage floor** in each local labor market by taking the mean of the firm-level average wage in each CB unit. I adjust the firm-level average wages by part-time work. To do so, I define a worker to be part-time if she earns less than 90% of the wage floor, and double their wage when computing the local labor market average. To obtain the **share of firms bunching**, I compute the average of a "firm buncher" indicator. There are some concerns of measurement error in wage floors, which complicate the definition of this indicator. First, wage floors are estimated from data, which means they are noisy.[56] Second, workers earning less than the wage floor are common. I define a worker to be a "buncher" (that is, a worker with a deviation of 0 from the wage floor) if she is full-time and her wage is between 90 and 105% of the floor, or if she is part-time and her wage is between 40 and 60% of the floor. Then, I define a "firm buncher" as a firm with all workers being bunchers.[57] To compute the **number of firms** in each local labor market, I simply count the number of distinct firm IDs. To compute **employment** I sum all workers in each firm.

The mean wage floors computed above are noisy and vary across local labor markets within a CB unit. I re-define wage floors at the CB unit level, which is the level at which they are determined. I start by computing the average of the local market mean wage floors in each CB unit by region cell, weighting for the share of workers with a valid wage floor in the local labor market. If all regional wage floors are within 5% of each other, I take the average and use it to define the CB unit's wage floor. I allow regional wage floors

[54] Only 1% of these cells have more than 75 firms.

[55] More precisely, I take the following steps to simplify the CB unit code. First, if a CB unit has less than 200 workers I assign it to the *local* category. The second step concerns CB units with more than 200 workers. If a local labor market is non-exporter, has less than 25 firms and 1000 employees, the share of employment with an assigned wage floor is less than 10%, and it corresponds to less than 10% of the employment in the CB unit, then I assign it to the *modal* CB unit in the region by coarse 4-digit sector cell. This introduces a small error in the CB network, but makes it more likely that these small local markets will get a valid wage floor estimate. Finally, if a CB unit has less than 2000 workers and the share of workers with a valid wage floor is less than 1%, then I assign the corresponding local markets to the "local" CB unit.

[56] Small CBAs and categories within CBAs with a few workers are both lost in the estimation step.

[57] There are some workers earning less than 40% of the estimated wage floor. The average firm-level share of workers that earn less than 40% of the wage floor is approximately 0.13. I drop firms with more than 50% of workers earning less than 40% of the wage floor from the computation of the aggregate share of firms bunching.

within a CB unit only if the region's average wage floor is sufficiently different from the rest.[58] I consider regional differences in wage floors as constant when estimating the effects of shocks, and allow a single wage floor per CB unit to be determined in the negotiations. This aligns with usual practice in Argentina, where regional differences within CBAs are usually constant.

The estimated shares of firms bunching are also noisy, the reason being that many workers do not have a valid wage floor assigned.[59] As a result, I adjust the estimated share of firms bunching in several ways. First, I notice that exporting local labor markets systematically report a lower share of firms bunching, even after controlling for the mean wage floor and the mean wage. This is problematic because it would lead to an over-estimate of the productivity of these local labor markets, and since these local markets are the ones hit by shocks these could have consequences for the model simulations. To deal with this, I simply add a random number between 0 and 0.03 to the share. Second, I compute a GLM regression of the share bunching on local market covariates and use it to predict the share of firms bunching for "problematic" local labor markets.[60] It is important to note that the model used to assign shares for a given region is estimated excluding that region, so all assigned shares are out-of-sample predictions. The model is used to impute 15.6% of local labor markets that have less than 10% of workers with a valid wage floor or an implausible share given the ratio between the mean wage and mean wage floor. Finally, I shrink the estimated share of firms bunching using the James-Stein estimator towards the region by coarse 4-digit sector average.

3.F.4 Estimating model parameters

Preference heterogeneity parameter η. To estimate η, I exploit the relationship between firm size ℓ and wages w implied by (3.6). To do so, I regress log wages on log employment at the firm level controlling for region and 1-digit sector fixed effects. However, the structure of labor supply implies an exact fit of this regression.

To allow for a non-perfect fit the model can be extended to incorporate hours. In particular, assume that production takes place before the firm draws a random value h such that realized employment is $\ell^* = h\ell$. In that case, the labor supply to the firm equation can be written as $\ell^*(j) = hW_r^{-\eta} A_{k1}^{\eta} w(j)^{\eta}$, and so by taking logs and rearranging terms I can write observed log wages as a function of observed log employment and $\ln h$

[58] Specifically, if the difference between regions is greater than 5% and the maximum is in Patagonia, I assign the maximum to Patagonia and average the rest. I then implement a handful of manual adjustments.

[59] They may not get a valid wage floor if they are in small CB units or if their occupation code within the CB unit is not observed.

[60] The covariates of the model are the mean wage floor, the mean wage, a dummy for exporter status, the share of workers with a valid wage floor, the share of part time workers, and coarse 4-digit sector fixed effects. The model is estimated only on local labor markets with more than 10% of workers with a valid wage floor and with a share of firms bunching between .5% and 99.5%.

plays the role of the error term.

Specifically, I use the firm-level data and estimate the regression

$$\ln w_{jt} = \bar{\eta} \ln \ell_{jt} + b_{k1(j)} + \delta_{r(j)t} + \nu_{jt}.$$

where $k1(j)$ is the 1-digit sector of firm j and $\delta_{r(j)t}$ corresponds to year by region fixed effects. Under the assumption that unobservables (hours) are uncorrelated with firm size within regions and time periods, the coefficient $\bar{\eta}$ identifies the inverse of η.

Appendix Table 3.J shows the estimates, which are stable when varying the set of fixed effects included in the regression. My preferred specification is column (3), which yields $\eta = 4.0995 \approx 1/0.2439$. The literature provides other values for this parameter. Monte, Redding, and Rossi-Hansberg (2018) in the US use county-level data and estimate a value of 3.3. Ahlfeldt, Roth, and Seidel (2022b) for Germany use a firm-level dataset and find a value of 5.2. Parente (2022) uses a calibration approach in Brazil and finds values of 4.52 and 4.22 for 1996 and 2012, respectively. Datta (2023) estimates a labor supply elasticity of 4.6 in the UK using HR data from a multi-establishment firm.

I also rely on column (3) of Appendix Table 3.J to obtain my estimates of the amenity values $\{A_{k1}\}_{k1\in\mathcal{K}1}$. The omitted category is chosen so that all b_{k1} are positive. Then, using the model structure I compute $A_{k1} = \exp(-b_{k1})$, which results in values ranging from 1 to 1.81. The amenity values allow firms to have different sizes conditional on the wage across 1-digit sectors.

Preference for formal employment ζ. From equation (3.8) the extensive-margin labor supply elasticity can be computed to be

$$\frac{d\mu_r}{dV_r}\frac{V_r}{\mu_r} = \zeta(1 - \mu_r).$$

The elasticity is estimated to be around 0.2 in the literature (Chetty et al., 2011). Using the average share of formal employment from Appendix Table 3.K, I set $\zeta = 0.2813$. Differences in the shares of formal employment across regions will load on outside options.

Shape of productivity distributions α. I calibrate the shape of the productivity distributions α to 5.50. As discussed in Section 3.6.2.2, the maximum share of firms constrained by the wage floor that is consistent with the model is given by

$$S_g^{\max} = 1 - \left(\frac{\eta}{\eta + 1}\right)^{\alpha}.$$

Inverting this expression for α and plugging-in the 98.8th percentile of the share of firms constrained by the wage floor for $S_g^{\max}$ and the value of η estimated above results in $\alpha \approx 5.50$.[61] For comparison, Parente (2022) uses a Pareto-LogNormal distribution where the shape of the Pareto is calibrated to 6.02 and 6.33 in two different periods.

Minimum productivities. I invert the share of firms constrained by the wage floor to obtain the minimum productivities $\{\varphi_{g0}\}_{g\in\mathcal{G}}$. First, if the share of firms constrained by the wage floor is zero, then I set $\varphi_{g0} = (\eta/(\eta+1))\underline{w}_c$. This assumes that the productivity is the minimum value that allows the local labor market to be unconstrained. Second, if the share of firms constrained is equal to the maximum possible share, then I set $\varphi_{g0} = \underline{w}_c$. This assumes that the productivity is the minimum value that makes the local labor market to be fully constrained. These two cases are rare, and happen only in a few small local labor markets. Finally, for all other observed values $S_g = s$, I invert equation (3.20) to get

$$\varphi_{g0} = \underline{w}_c\,(1-s)^{1/\alpha}\left(\frac{\eta}{\eta+1}\right).$$

For a given $\underline{w}_c$, a larger observed share s implies that the minimum productivity must be smaller.

Outside options b_r. To obtain outside options I use equation (3.8), which requires computing V_r. Furthermore, V_r requires knowledge of regional wage indexes W_r. So, I proceed as follows. First, noting that $L_g = W_r^\eta \hat{L}_g$, I compute wage indexes using the labor market clearing condition:

$$W_r = \left(\frac{\sum_g \hat{L}_g}{\mu_r N_r}\right)^{1/\eta},$$

where μ_r and N_r are obtained from household survey data. In particular, N_r is the number of private sector workers (formal or informal), and μ_r the share of formal private sector workers.[62] Then, equation (3.7) is used to obtain V_r. Finally, I compute b_r inverting equation (3.8). Appendix Table 3.K shows the results.

Bargaining power parameters. I invert the closed form expression for the Nash bargaining solution of each CB unit to obtain the bargaining power parameters, which is given by (3.2). This condition will hold in any equilibrium with $\underline{w}_c < \min_{g:c(g)=c}\{\varphi_g\}$, i.e., when no market is maximally constrained by the wage floor. Importantly, the inversion relies on computing the equilibrium value of the derivative of the objective functions of the union and the employer association with respect to the wage floor, both of which enter the bargaining weight. This condition may not hold if a CB unit's optimum is not found at an interior solution.

[61] Matching higher percentiles requires increasingly larger values of α, with $\alpha \to \infty$ if we want to match $S_g^{\max} = 1$.
[62] Formal workers are those that declare to contribute to the social pension system.

Let $\gamma_c = WB_c/R_c$ be the share of the wage bill of the union in local labor market, computed using equilibrium quantities. Then, the bargaining power parameters are computed as

$$\beta_c = \frac{\gamma_c}{\gamma_c + (1-\gamma_c)\left(-\frac{dU}{d\underline{w}_c} / \frac{d\Pi}{d\underline{w}_c}\right)}.$$

Note that these derivatives are evaluated at the equilibrium wage floors and wage indexes, and include the general equilibrium term $dW_r/d\underline{w}_c$ from equation (3.25). Since γ_c is bound by construction in the model, the key variation used to obtain the bargaining parameters comes from the ratio of the derivatives of U and Π with respect to the wage floor. The distribution of the ratio of derivatives is shown in Panel (a) of Appendix Figure 3.R. Panel (b) of Appendix Figure 3.R shows the estimated bargaining power parameters.

I check whether the inverted bargaining power parameters actually result in maximizers of the Nash bargaining objective functions. To do so I test whether the wage floor can be recovered from the model equilibrium using the bargaining power parameters. While I do recover the correct wage floor in most cases, for one important CB unit I do not: the retail CB unit 0130/75. For this CB unit the condition that delivers the bargaining power parameter is a local maximum, but for this bargaining power parameter the global maximum is a non-binding wage floor. To deal with this issue I keep fixed the wage floor of this CB unit in counterfactual exercises.

3.G Additional Tables and Figures

Table 3.B: Description of coverage of selected collective bargaining agreements in the textile industry

CBA	Spanish	English
0500/07	Obreros de la industria textil, son los ocupados en establecimientos cuya actividad principal comprenda procesos destinados a la confección de colchones, bolsas, tejer, lavar, clasificar, peinar, cardar, hilar, urdir, tramar, retrocer, estrusar, devanar, desfibrar, teñir, aprestar, texturizar, bordar, cortar, coser, atar, anudar, bobinar, planchar, estampar, terminar, o similares y que se lleve a cabo sobre cualquier tipo de fibras, sean naturales o manufacturadas, ya sea manualmente o mediante la utilización de maquinarias subordinadas al proceso industrial textil.	Workers in the textile industry are those engaged in establishments whose main activity involves processes intended for making mattresses, bags, knitting, washing, sorting, combing, carding, spinning, warping, weaving, twisting, extruding, winding, defibring, dyeing, finishing, texturing, embroidering, cutting, sewing, tying, knotting, spooling, ironing, stamping, completing, or similar activities, carried out on any type of fibers, whether natural or manufactured, either manually or through the use of machinery subordinate to the textile industrial process.
0501/07	Trabajadores, empleados, supervisores, encargados, mecánicos, personal auxiliar de ambos sexos, de administración, de comercialización y de fábrica únicamente de las empresas industriales de indumentaria y afines.	Workers, employees, supervisors, managers, mechanics, auxiliary staff of both genders, from administration, sales, and exclusively from clothing industrial companies and related fields.
0746/17	Todos los trabajadores de la industria de la confección de indumentaria y afines según se especifican en los respectivos capítulos de la misma, comprende también a las empresas que fabrican toldos en general y sus respectivos accesorios, en artículos con tela de lona, plástica, sintéticas y/o similares, empresas que confeccionan y arman colchones en general y con sus respectivos accesorios, con todo tipo de materiales. Están comprendidos también los lavaderos industriales de los procesos de producción, tanto internos como externos. También incluye las empresas que producen avíos y accesorios internos para todo tipo de prendas de vestir en general, cualquiera fuere el material empleado en su producción y/o elaboración.	All workers in the apparel manufacturing industry and related fields as specified in the respective [CBA] chapters. This also includes companies that manufacture awnings in general and their respective accessories, in articles made of canvas fabric, plastic, synthetic and/or similar materials. It also covers companies that make and assemble mattresses in general and their accessories, with all types of materials. Also included are industrial laundries involved in the production process, both internal and external. It also includes companies that produce fittings and internal accessories for all types of clothing in general, regardless of the material used in their production and/or manufacturing.

Notes: The figure shows the description of the areas of representation for three selected collective bargaining agreements (CBAs) in the textile industry. The three CBAs specify the entire country as their regional scope, and were signed by different unions. CBA 0500/07 was signed by *Asociación Obrera Textil*, CBA 0501/07 was signed by *Sindicato de Empleados Textiles de la Industria y Afines*, and CBA 0746/17 was signed by *Federación Obrera Nacional de la Industria del Vestido y Afines*. The description in Spanish is verbatim from the CBA, and the description in English is a translation obtained using the large language model GPT-4.

Table 3.C: Summary statistics of main estimating sample, firm cross-section

	N	Mean	Std. Dev	Min	Max
Unique 4d sector	222				
Unique 6d sector	467				
Firm shock (2013-12 vs. 2010-09)	7,972	0.4325	0.3661	-3.6234	3.2670
Pre firm shock (2010-09 vs. 2008-07)	7,972	0.1363	0.2951	-0.8630	1.3006
Average employment 2007-09	7,972	46.04	55.66	1.00	396.33
Indicator employment 2007-09 $\leq$10	7,972	0.2346	0.4238	0.0000	1.0000
Average monthly wage 2007-09 (2010 ARS)	7,972	3,593.46	2,503.61	428.75	57,295.78
Log mean value exported 2011-2012	7,972	11.2357	2.1860	6.0638	16.9391
Observed in survey data	7,972	0.2258	0.4181	0.0000	1.0000

Notes: Data are from the baseline sample of firms that exported in 2011–2012. The tables show summary statistics of the data used in the main difference-in-differences estimates.

Table 3.D: Summary statistics of main estimating sample, CB units cross-section

	N	Mean	Std. Dev	Min	Max
Shock 2013-2012 minus 2010-09	174	0.5305	0.7440	-1.5745	4.2882
Shock 2010-2009 minus 2007-08	174	0.2912	1.0960	-8.1718	4.2070
Num. firms 2011-12	174	2,975.14	20,682.06	6	263,629
Num. firms in baseline sample 2011-12	174	45.82	246.99	1	2,310
Share employment exporting firms 2011-12	174	0.4065	0.2834	0.0162	0.9837

Notes: Data are from a panel of firms that exported in 2011–2012. The tables show summary statistics of the data used in the main difference-in-differences estimates.

Table 3.E: Summary statistics of main estimating sample, firm panel

	N	Mean	Std. Dev	Min	Max
Year	86,238	2,011.94	3.15	2,007	2,017
CB shock 2013-2012 minus 2010-09	86,238	0.4739	0.3780	-1.5745	4.2882
Firm shock 2013-2012 minus 2010-09	86,238	0.4328	0.3660	-3.6234	3.2670
Log average monthly wage	50,703	7.8383	0.1942	6.5794	9.6323
Log average monthly wage floor	85,777	3.2267	1.1976	0.0000	8.5142
Log employment	85,564	3.1432	1.1880	0.0000	8.4933
Share main hiring modality	86,238	0.6871	0.2839	0.0000	1.0000
Indicator active firm	87,692	0.9834	0.1277	0.0000	1.0000

Notes: Data are from a panel of firms that exported in 2011–2012. The tables show summary statistics of the data used in the main difference-in-differences estimates.

Table 3.F: Static difference-in-differences estimates, worker-level estimates

	Log mean wage				
	(1)	(2)	(3)	(4)	(5)
CB shock × Post	0.0165	0.0108	0.0189		0.0192
	(0.0058)	(0.0067)	(0.0101)		(0.0066)
CB shock × Post × Main CB				0.0209	
				(0.0108)	
CB shock × Post × Secondary CB				0.0060	
				(0.0132)	
Firm shock	Y	Y	Y	Y	N
Worker-firm FE	Y	Y	Y	Y	N
Firm-year FE	N	N	N	N	Y
Worker FE	N	N	N	N	Y
2d sector-province-year FE	Y	N	N	N	N
4d sector-province-year FE	N	Y	N	N	N
6d sector-province-year FE	N	N	Y	Y	N
2d sector-secondary CB-year FE	Y	Y	Y	Y	N
Hiring modality-year FE	Y	Y	Y	Y	N
Num. fixed effects	124,661	128,979	132,666	132,666	186,814
Num. observations	793,751	793,751	793,751	793,751	850,164
Adjusted R^2	0.8727	0.8795	0.8846	0.8846	0.9102

Notes: Data are a panel of workers that worked in exporting firms in 2011–2012 in 2008, 2011, and 2014 The table shows estimates of the effect of CB shocks on the log mean real monthly wage. Columns (1) to (3) show estimates a difference-in-differences model using the main CB shock variable in the firm as treatment. Column (4) replicates column (3), but interacts the primary CB shock with an indicator for whether the worker's CB unit is the primary CB unit in the firm. Column (5) shows estimates of a difference-in-differences model using the CB unit of the worker to define the treatment. The "2d sector-secondary CB-year" fixed effects include interactions between 2-digit sector, an indicator for whether the worker's CB is the primary CB in the firm, and year indicators. The "Hiring modality-year" fixed effects include interactions between all possible hiring modality indicators as of 2008 with year indicators. Standard errors are clustered at the CB unit level.

Table 3.G: Static difference-in-differences estimates, robustness to local labor market controls

	Log mean wage			Log mean wage floor			Log employment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CB shock × Post	0.0458	0.0338	0.0466	0.0541	0.0560	0.0526	-0.0233	-0.0174	0.0022
	(0.0202)	(0.0236)	(0.0172)	(0.0213)	(0.0239)	(0.0199)	(0.0351)	(0.0372)	(0.0305)
Firm shock × Post	0.0134	0.0106	0.0119	-0.0036	-0.0035	-0.0031	0.0297	0.0211	0.0257
	(0.0070)	(0.0063)	(0.0064)	(0.0021)	(0.0025)	(0.0018)	(0.0210)	(0.0175)	(0.0194)
6d sector shock × Post			0.0144			-0.0013			-0.0229
			(0.0064)			(0.0033)			(0.0138)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
2d sector by province by year FE	N	N	Y	N	N	Y	N	N	Y
4d sector by province by year FE	Y	N	N	Y	N	N	Y	N	N
6d sector by province by year FE	N	Y	N	N	Y	N	N	Y	N
Num. firms	7,972	7,972	7,953	7,654	7,654	7,636	7,972	7,972	7,953
Num. fixed effects	27,976	33,671	21,289	19,860	23,252	15,808	27,976	33,671	21,289
Num. observations	85,777	85,777	85,634	50,703	50,703	50,644	85,777	85,777	85,634
Adjusted R^2	0.8480	0.8499	0.8540	0.9253	0.9267	0.9282	0.8965	0.8992	0.9002

Notes: Data are from the baseline sample of exporting firms. The figure show regression coefficients on the firm, CB, and 6-digit sector shocks variables interacted with an indicator for year great
are analogous to the ones in Table 3.1, but changing the approach to control for local labor market effects. The economic sector categories are constructed from a granular 6-digit economic secto
of Argentina and is based on ISIC, rev 4. Standard errors are clustered at the CB unit level.

Table 3.H: Static difference-in-differences estimates, robustness to model specification

	Log mean wage				Log employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CB shock × Post	0.0458	0.0366	0.0458	0.0484	-0.0233	-0.0119	-0.0243	-0.0232
	(0.0202)	(0.0164)	(0.0202)	(0.0221)	(0.0351)	(0.0272)	(0.0354)	(0.0351)
Firm shock × Post	0.0134	0.0141	0.0134	0.0128	0.0297	0.0309	0.0298	0.0297
	(0.0070)	(0.0084)	(0.0070)	(0.0070)	(0.0210)	(0.0165)	(0.0210)	(0.0208)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	N	Y	Y	Y	N	Y	Y
Pre-period firm shock	N	N	Y	N	N	N	Y	N
Pre-period CB shock	Y	Y	Y	N	Y	Y	Y	N
Local labor market by year FE	Y	Y	Y	Y	Y	Y	Y	Y
Num. firms	7,972	7,972	7,972	7,972	7,972	7,972	7,972	7,972
Num. fixed effects	27,976	18,530	27,976	27,965	27,976	18,530	27,976	27,965
Num. observations	85,777	85,777	85,777	85,777	85,777	85,777	85,777	85,777
Adjusted R^2	0.8480	0.8491	0.8479	0.8478	0.8965	0.8984	0.8965	0.8965

Notes: Data are from the baseline sample of exporting firms. The table show regression coefficients on the firm and CB shocks variables interacted with an indicator for year greater than or e
controls. All outcomes are computed using the full set of workers in the firm-year. The regressions are analogous to the ones in Table 3.1, but changing the set of included controls. Standard err
variable, and at the firm level for the firm shock variable.

Table 3.I: Static difference-in-differences estimates, robustness to inclusion of CB units

	Log mean wage				Log employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CB shock × Post	0.0458	0.0463	0.0687	0.0265	-0.0233	-0.0215	-0.0490	-0.0316
	(0.0202)	(0.0202)	(0.0226)	(0.0174)	(0.0351)	(0.0349)	(0.0414)	(0.0428)
Firm shock × Post	0.0134	0.0129	0.0131	0.0170	0.0297	0.0279	0.0283	0.0098
	(0.0070)	(0.0069)	(0.0070)	(0.0092)	(0.0210)	(0.0214)	(0.0214)	(0.0268)
Excluded CB units (n. exp. firms)	< 5	None	< 30	< 5	< 5	None	< 30	< 5
Include 0130/75 (retail CB unit)	Y	Y	Y	N	Y	Y	Y	N
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y	Y	Y
Pre-period CB shock	Y	Y	Y	Y	Y	Y	Y	Y
Local labor market by year FE	Y	Y	Y	Y	Y	Y	Y	Y
Num. firms	7,972	8,019	7,754	5,733	7,972	8,019	7,754	5,733
Num. fixed effects	27,976	28,083	27,412	22,571	27,976	28,083	27,412	22,571
Num. observations	85,777	86,273	83,441	61,741	85,777	86,273	83,441	61,741
Adjusted R^2	0.8480	0.8485	0.8488	0.8504	0.8965	0.8966	0.8962	0.8969

Notes: Data are from the baseline sample of exporting firms. The table show regression coefficients on the firm and CB shocks variables interacted with an indicator for year greater than or equ
to the ones in Table 3.1, but changing the sample of CB units included in the regression. "n. exp. firms" refers to the number of exporting firms in CB unit. Standard errors are clustered at the C
level for the firm shock variable.

Table 3.J: Estimates of preference heterogeneity

	Log average monthly pay				
	(1)	(2)	(3)	(4)	(5)
Log employment	0.2372	0.2368	0.2439	0.2307	0.2351
	(0.0108)	(0.0109)	(0.0065)	(0.0061)	(0.0065)
Year	Y	N	N	N	N
Year-Region FE	N	Y	Y	N	N
1d sector FE	N	N	Y	N	N
Year-Region-CBA FE	N	N	N	Y	N
Year-Region-CBA-6d sector FE	N	N	N	N	Y
Observations	1,243,640	1,243,640	1,240,627	1,240,627	1,240,627

Notes: Data are from labor market administrative records from Argentina. The sample is a panel of firm-years between 2012–2017 with 99% or more of their workers with a declared CBA code, and no workers with a declared wage below 40% of the wage floor. The table shows estimates of preference heterogeneity $1/\eta$ in the theoretical model. The dependent variable in all models is the log average monthly pay. The independent variable is the log of employment, computed in "full-time equivalent" workers by weighting part-time workers by 1/2. Columns show estimates changing the value of the fixed effects. Standard errors are clustered at the province by CBA level.

Table 3.K: Region-level data and 2011–2012 model estimates

Region	Centro (center)	Cuyo (west)	Norte (north)	Patagonia (south)
Private-sector workforce	5,099,128	399,917	837,599	204,902
Formal workforce	2,588,641	177,622	280,195	123,478
Share formal	0.5077	0.4441	0.3345	0.6026
W_r	0.8690	0.8530	0.8548	1.2763
V_r	1.0580	1.0384	1.0406	1.5538
b_r	0.9853	1.2648	2.0118	0.7465

Notes: Data are from the national household survey from INDEC and estimates from the structural model.

Table 3.L: Summary statistics of local labor markets

Variable	N	Mean	Std. Dev.	Min	Max
Unique regions	4				
Unique sectors	681				
Unique non-local CB units	438				
Unique local CB units	456				
Indicator region "Centro"	3,701	0.6347	0.4816	0.0000	1.0000
Indicator local CB unit	3,701	0.1359	0.3427	0.0000	1.0000
Indicator retail CB 0130/75	3,701	0.1489	0.3560	0.0000	1.0000
M_g	3,701	1.0000	4.7276	0.0070	171.2625
Mean wage (2010 ARS, 000s)	3,701	2.8210	1.6467	0.1842	28.2819
Mean wage adj. part-time (2010 ARS, 000s)	3,593	3.4498	1.5810	1.3225	28.8316
Mean wage floor (2010 ARS, 000s)	3,701	2.6216	0.8856	1.1674	13.3039
Share of firms bunching	3,701	0.2258	0.1696	0.0007	0.9962
Estimated minimum productivity	3,701	3.1008	1.0562	1.4388	15.0234

Notes: Data are from local labor market aggregates constructed using the administrative labor market data of Argentina. The table shows summary statistics of local labor markets in 2011–2012.

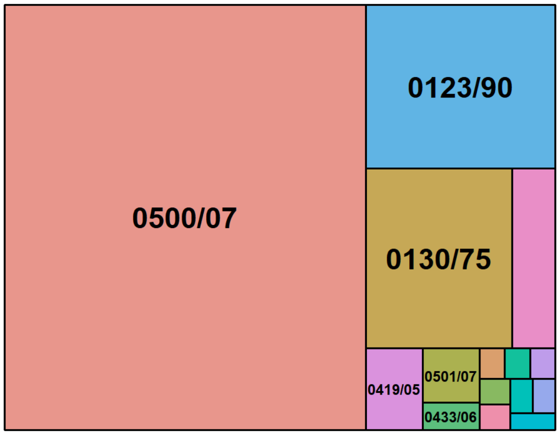
Table 3.M: Effects of export shocks on aggregate revenue, survey sample

	Change aggregate revenue			
	(1)	(2)	(3)	(4)
Sectoral shock by Exporter	0.2238	0.1436	0.2475	0.2223
	(0.1192)	(0.0697)	(0.1381)	(0.1188)
Sectoral shock by Non-exporter	0.0849	0.0527	0.0689	0.0713
	(0.0564)	(0.0508)	(0.0577)	(0.0533)
Excluded CBA	None	Metal	Retail	Plastic
Exporter-specific intercept	Yes	Yes	Yes	Yes
R2	0.0510	0.0391	0.0524	0.0547
Observations	154	110	147	149
Number of firms	4,840	3,268	4,405	4,513

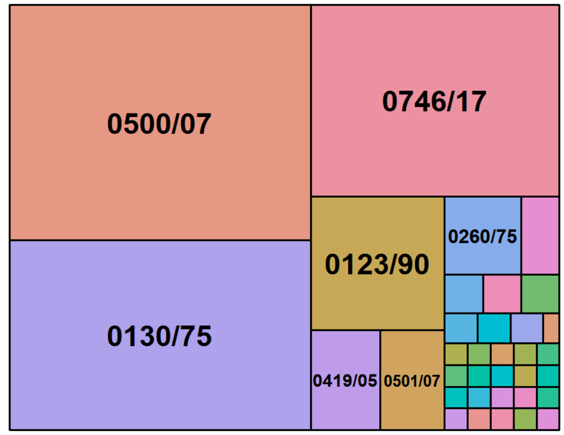
Notes: Data are from a sample of firms surveyed in the first wave of the *Encuesta Nacional de Dinámica del Empleo y la Innovación* (ENDEI). The table show estimates of export shocks on aggregate revenue at the 4-digit economic sector level on exporters and non-exporters. The baseline sample includes economic sectors with at least 4 exporting firms and 6 total firms. The sectoral shock is computed as follows. First, I compute the change in average log world import demand at the firm level between 2010 and 2012. Second, I define the sectoral shock as the average firm-level shock, weighting by employment in 2010, for all exporting firms. The change in aggregate revenue is computed as the log difference in the sum of revenue declared by surveyed firms in each exporter status by 4-digit economic sector cell. The Metal CBA has code 0260/75, the Retail CBA has code 0130/75, and the Plastic CBA has code 0419/05. The row "Number of firms" reports the number of firms used to compute aggregate revenue. Standard errors are clustered at the 4-digit sector level.

Figure 3.E: Illustration of heterogeneity in coverage of CB units, textile industries

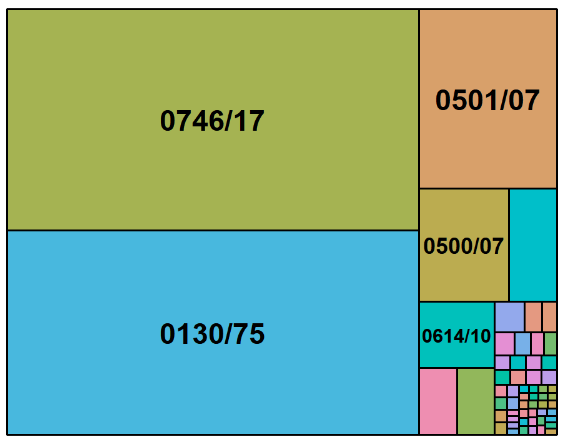
(a) Weaving of textiles



(b) Manuf. of textiles (ex. apparel)

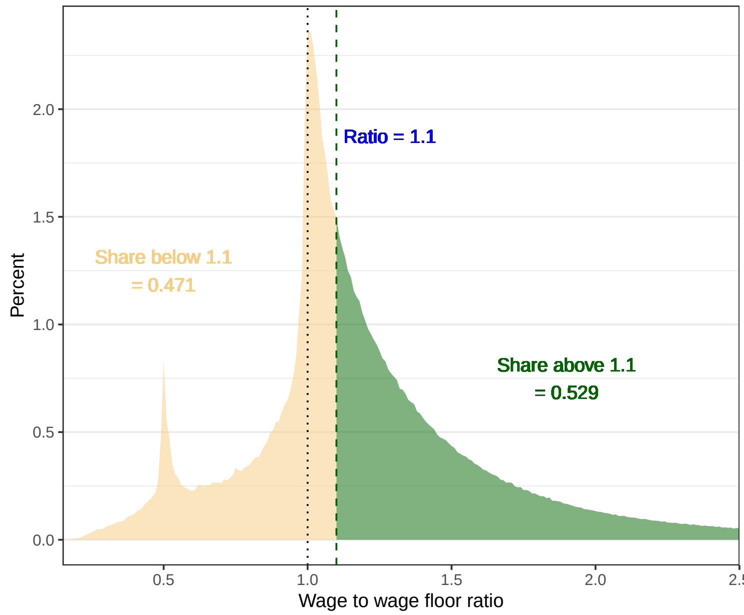


(c) Manuf. of wearing apparel (ex. fur)



Notes: Data include all firms that had positive employment in 2012. Each box represents the sum of firms in the given economic sector, and the rectangles within each box represent the number of firms in each CB unit. The figure is based on the CB unit code of each firm obtained by the procedure described in Appendix 3.B.2.3.

Figure 3.F: Distribution of wage to wage floor ratio, worker-level data in 2012

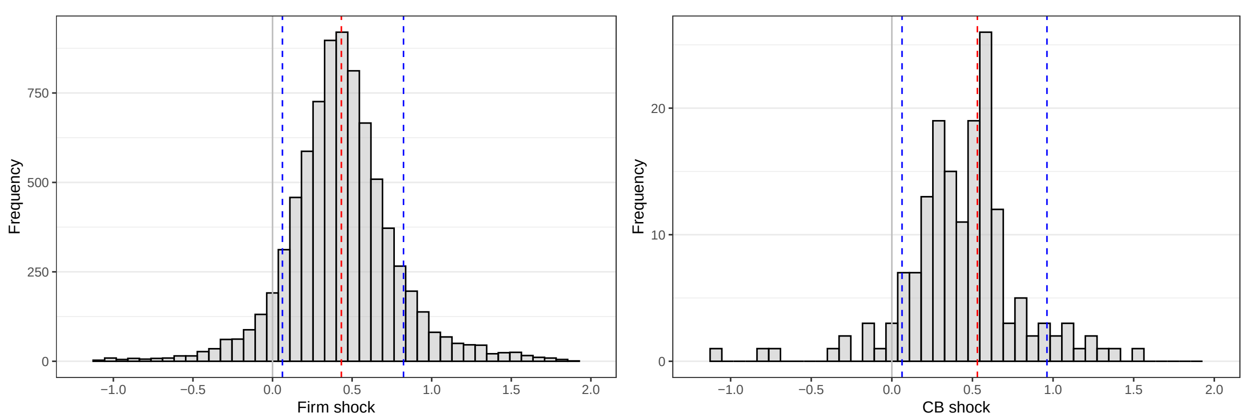


Notes: Data include all workers with a valid wage floor in 2012 declared in the main hiring modality. The figure shows a histogram of the ratio of the worker's wage to the wage floor that applies to the respective worker (excluding tenure-based compensation). The color fill varies for workers earning more or less than 1.1 times the wage floor. The dotted black line marks the ratio of 1. The green dashed line marks the 1.1 threshold that determines the color of the distribution. The blue dashed line marks the average wage cushion.

Figure 3.G: Distribution of export shocks

(a) Shocks to firms

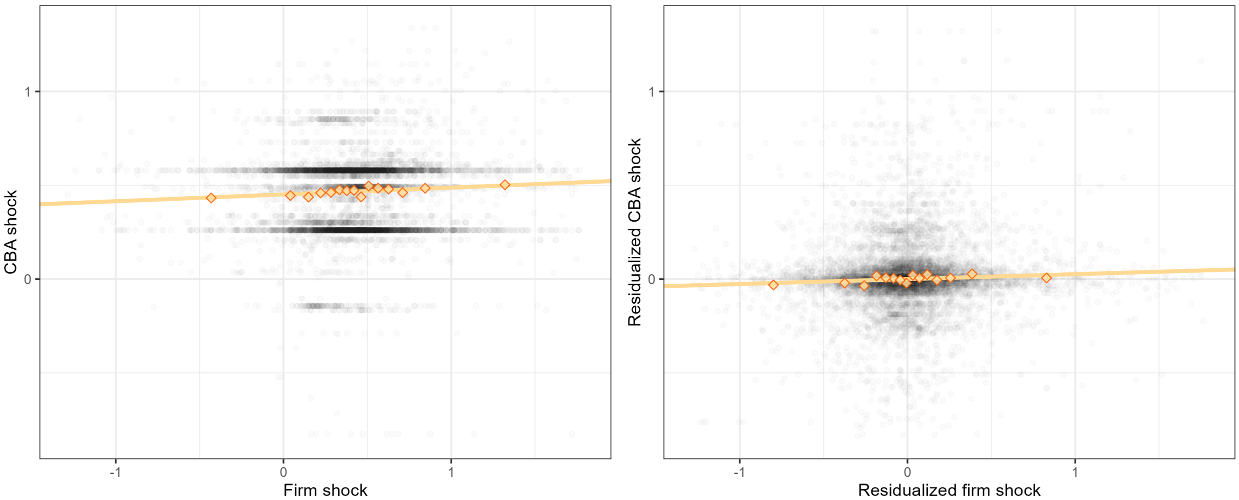
(b) Shocks to CB units



Notes: Data are constructed from a panel of firms that exported in 2011–2012. The figure illustrates the histogram of the firm and CB unit shocks. The figure excludes values lower than -1 and larger than 2 to increase visual clarity. The red dotted line in the middle shows the average shock, whereas the blue dotted lines on the sides show the 10th and 90th percentiles, respectively.

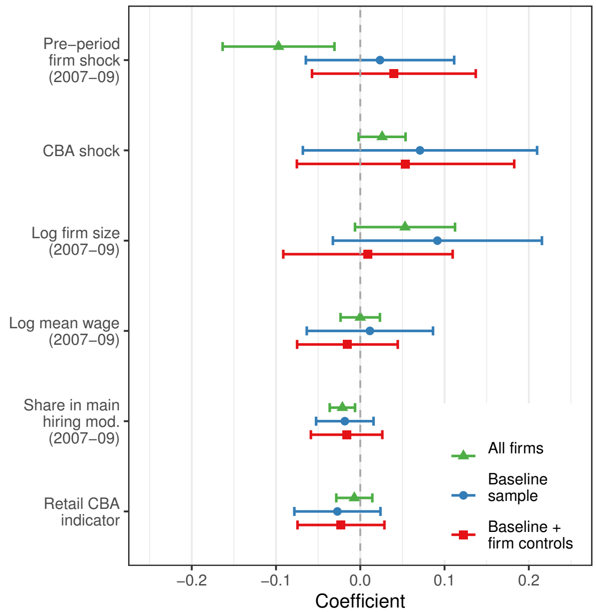
Figure 3.H: Correlation of export shocks to firms with export shocks to CB units

(a) Raw data (b) Residualized on local labor market effects



Notes: Data are constructed from a panel of firms that exported in 2011–2012. Panel (a) shows the correlation of firm shocks with CB shocks. Panel (b) shows the same correlation after residualizing for 4-digit sector by province fixed effects. To increase visual clarity Panel (a) excludes values lower than -1 and larger than 2 of both shocks, and Panel (b) excludes values lower than -1.5 and larger than 1.5 of both shocks. The orange line in both plots shows a non-parametric best fit.

Figure 3.I: Conditional correlation of firm shocks with baseline outcomes, firm-level cross-section

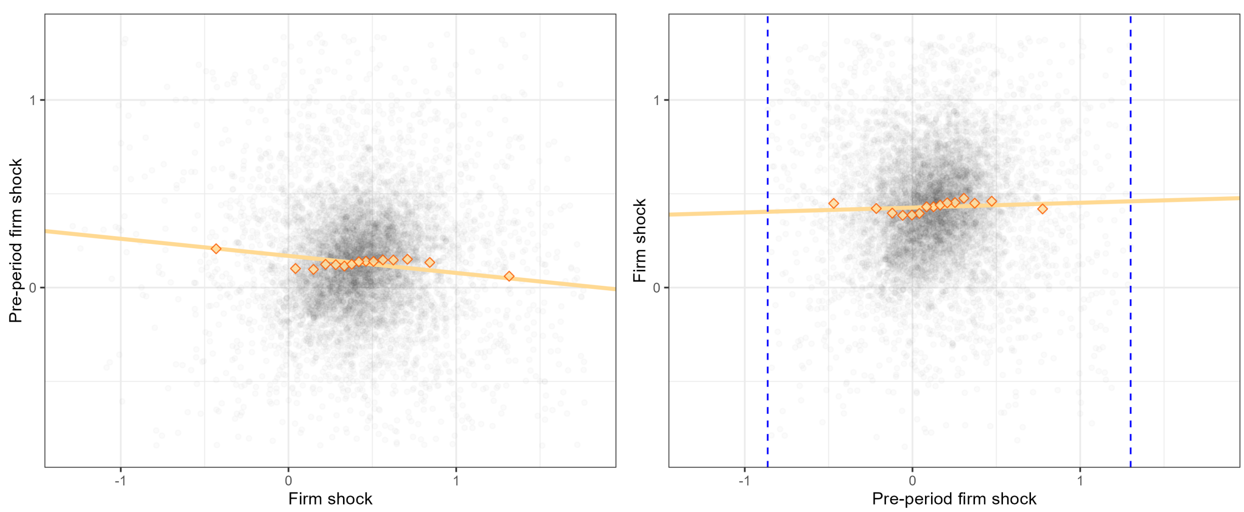


Notes: Data are constructed from a cross-section of firms that exported in 2011–2012. The figure shows the estimated coefficient on the firm shock on a regression of the outcome variable on the firm shock, controlling for 4-digit sector by province fixed effects. Standard errors are clustered at the CB unit level.

Figure 3.J: Auto correlation of export shocks to firms

(a) All firms

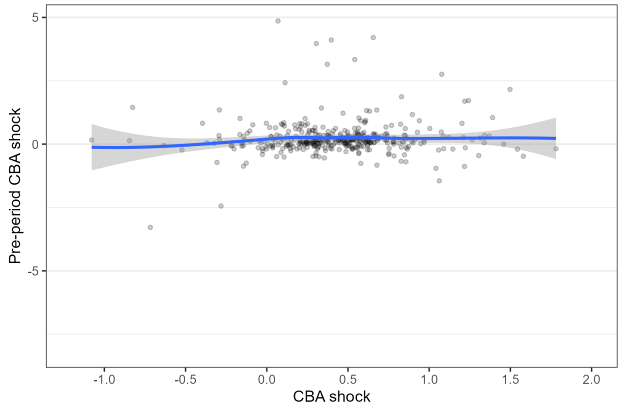
(b) Excluding top and bottom 1%



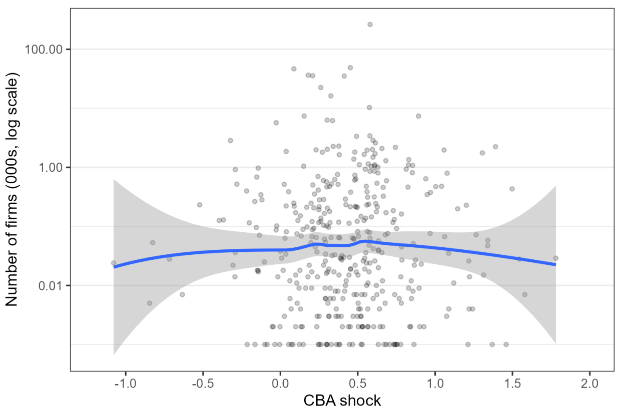
Notes: Data are constructed from a panel of firms that exported in 2011–2012. The figure shows the correlation of firm shocks, defined as the change between the 2012–13 and the 2009–10 average in the value-weighted average world import demand for the firm, with an analogous firm shock computed as the change between 2009–10 and 2007–08. Panel (a) uses all exporting firms, while Panel (b) excludes firms that are in the top or bottom 1% of the pre-period distribution of firm shocks. The orange diamonds show the average firm shock within 15 bins of the pre-period firm shock. The orange line shows a linear fit to the data. The blue dashed lines indicate the 1st and 99th percentiles of the pre-period firm shock.

Figure 3.K: Correlates of export shocks to CB units with observables

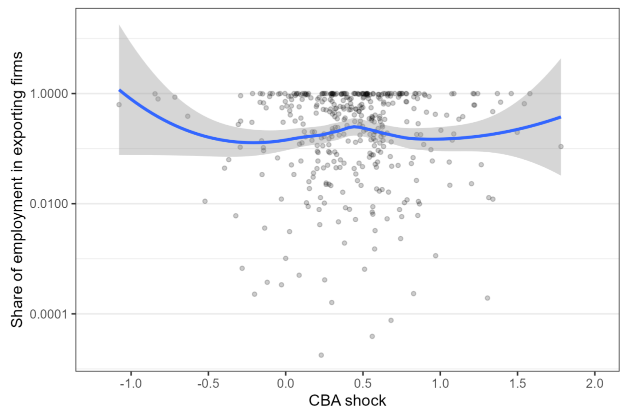
(a) Pre-period CBA shock



(b) Log number of firms



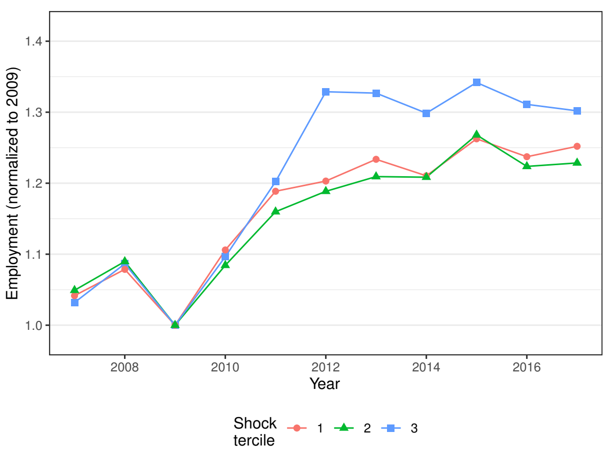
(c) Share employment in exporting firms



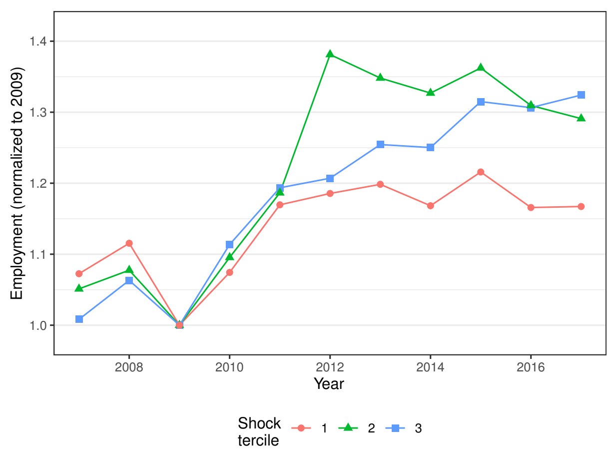
Notes: Data are constructed from a panel of firms that exported in 2011–2012. The figure shows a scatter plot of the CB unit shock with CB unit-level observables. Panel (a) uses the pre-period CB shock, constructed by differencing the proxy z_{jt} between 2008–2009 and 2008–2007. Panel (b) uses the number of firms. Panel (c) uses the share of employment in exporting firms. The line represents a locally estimated scatterplot smoothing curve fitted to the data.

Figure 3.L: Evolution of employment by level of CB unit and firm shock, baseline sample

(a) Shocks to firms



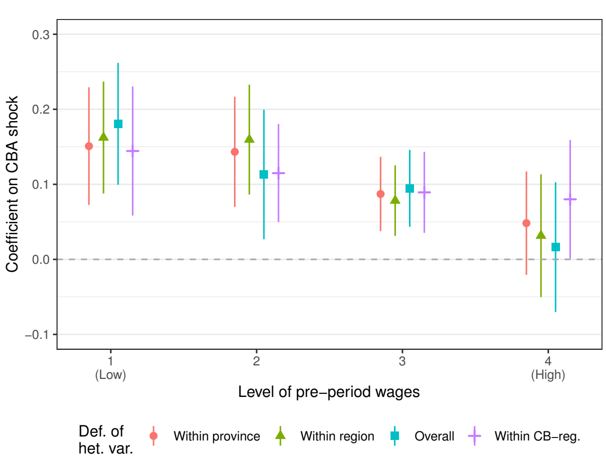
(b) Shocks to CB units



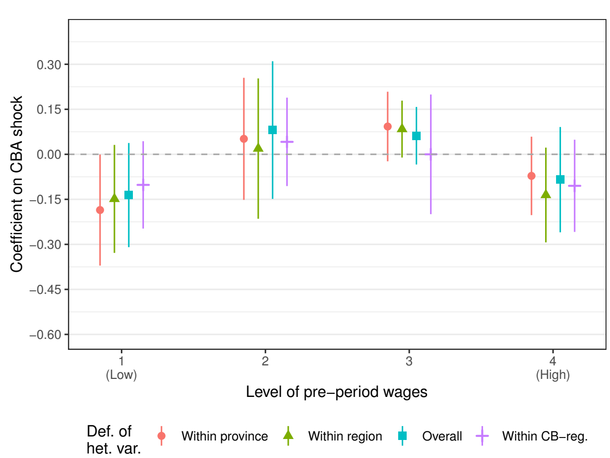
Notes: Data are from the baseline sample of exporting firms. The figure shows the average evolution of employment for firms in different terciles of the distribution of the firm shock (Panel a) and the CB shock (Panel b), relative to 2009. The firm and CB shocks are defined as the average changes in world import demand between 2009–2010 and 2012–2013, weighting by appropriate exposure shares.

Figure 3.M: Effect of export shocks to CB units, heterogeneity analysis

(a) Wage outcomes



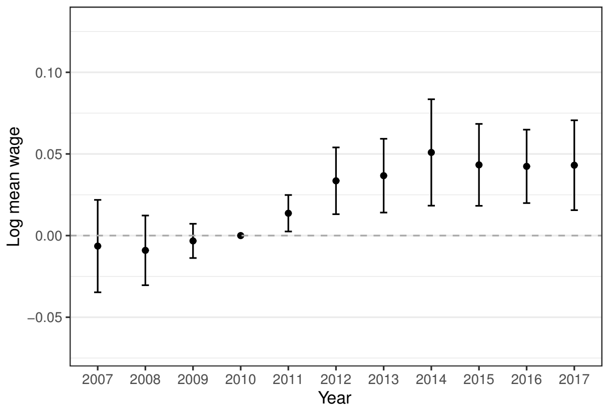
(b) Employment outcomes



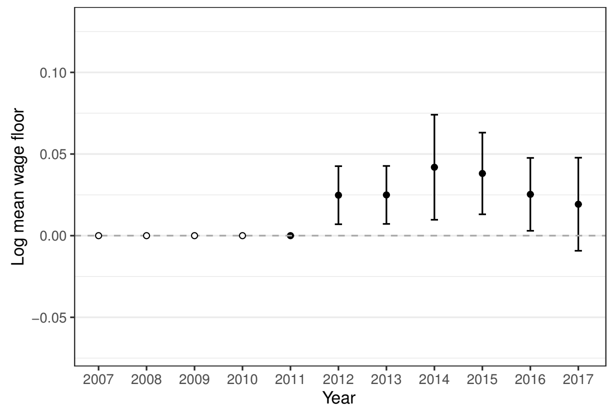
Notes: Data are from the baseline sample of exporting firms, excluding CB unit by province cells with less than 6 firms. The figure shows estimates of the effects of CBA shocks on a given outcome interacted with dummies for different quartiles of the pre-period level of wages (2007–2009), defined in within province, within region, within CB unit by region, and overall. The top figure shows the effect of the place CBA shock on log mean wage and log mean wage floor, and the bottom figure shows the effect on log employment and the share of workers hired in the main hiring modality in the firm. The regression model follows Table 3.3. Standard errors are clustered at the CB unit level.

Figure 3.N: Effect of export shocks to CB units on non-exporting firms

(a) Log mean wage



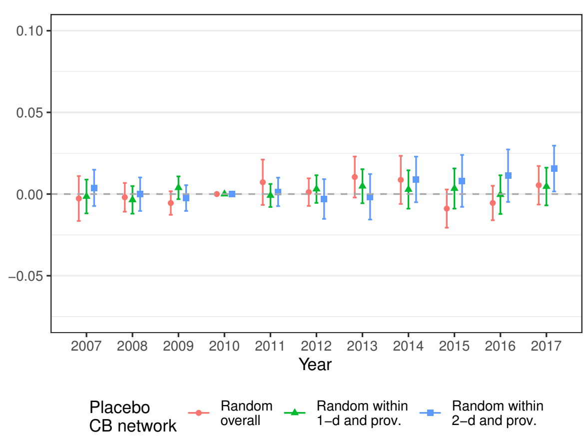
(b) Log mean wage floor



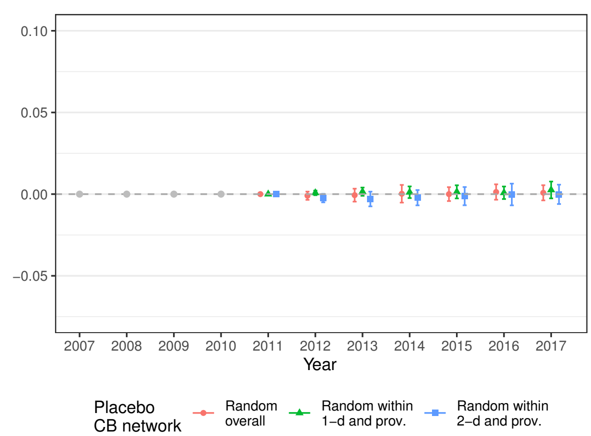
Notes: Data are from a panel of non-exporting firms that are covered by an exporting CB unit in 2011–2012. The figure shows the dynamic the effects of CB shocks on mean wages and mean wage floors. The regression omits the year 2010 for the wage variable, and the year 2011 for the wage floor variable. The sample includes firms smaller than 500 employees at baseline, firms in CB units with more than 30 firms in 2011, and firms that were active in 2007 and 2009. The regression includes firm fixed effects, 4-digit economic sector by exporter status by province by year fixed effects, time-varying firm controls, and a similar CB shock for the pre-period interacted with year dummies. Firm controls consist of a firm size indicator (categories 1-9, 10-24, 25-99, and 100-500) interacted with 2-digit sector and year and the pre-period share of workers in the main hiring modality interacted with year. The firm and CB shocks are defined as the average changes in world import demand between 2009–2010 and 2012–2013, weighting by appropriate exposure shares. Standard errors are clustered at the CB unit level.

Figure 3.O: Effect of export shocks to CB units, placebo network exercise

(a) Log mean wage



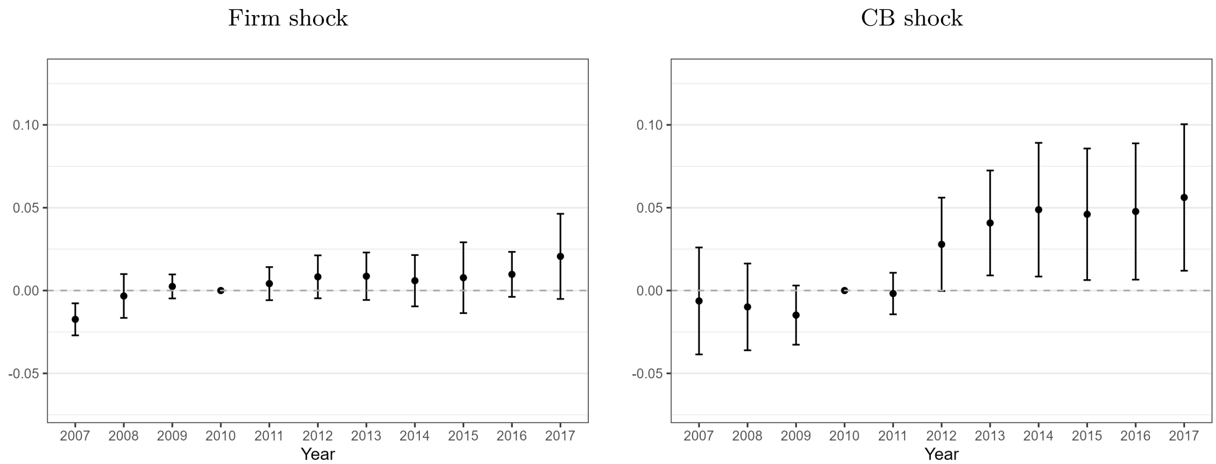
(b) Log mean wage floor



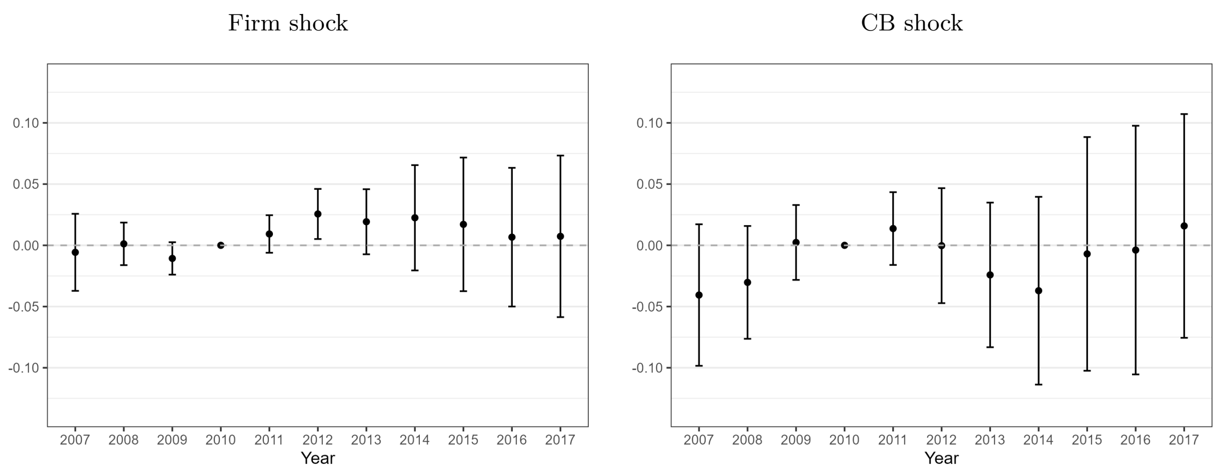
Notes: Data are from the baseline sample of exporting firms. The figure shows estimates of placebo shocks to CB units. The placebo network was constructed by randomly shifting the CB agreement code across all firms, across firms within 1-digit sector and province, and within 2-digit sector and province. The top figure shows the effect of the place CB shock on log mean wages, and the bottom figure shows the effect on the log mean wage floor. The regression includes the firm shock, firm fixed effects, 4-digit economic sector by province by year fixed effects, time-varying firm controls, and a similar CB shock for the pre-period interacted with year dummies. Firm controls consist of a firm size indicator (categories 1-9, 10-24, 25-99, and 100-500) interacted with 2-digit sector and year and the pre-period share of workers in the main hiring modality interacted with year. Standard errors are clustered at the CB unit level.

Figure 3.P: Effect of export shocks on wages, not excluding firms with extreme values of the pre-period shock

(a) Log mean wages

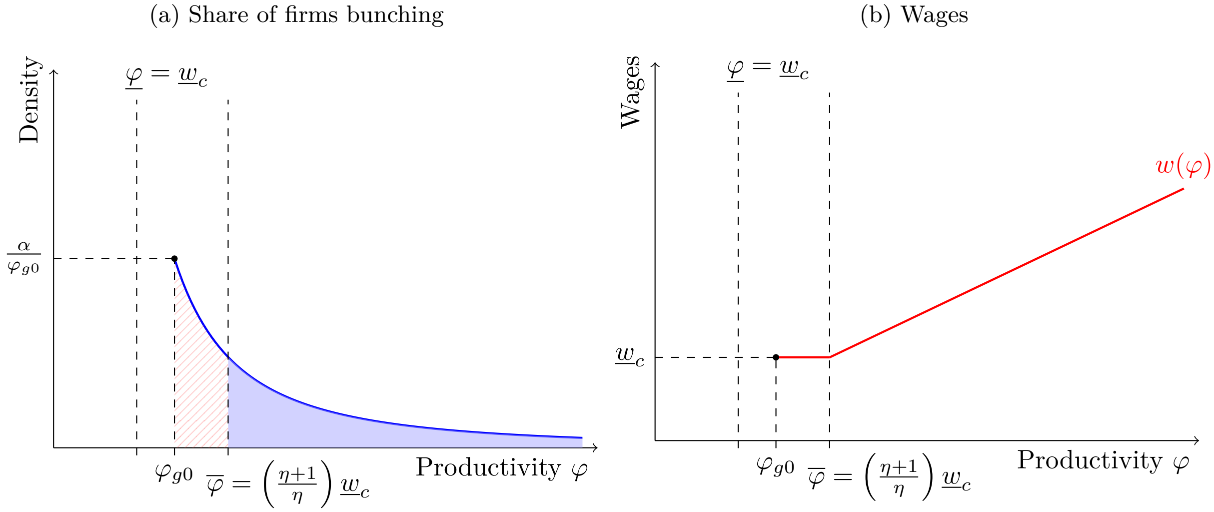


(b) Log employment



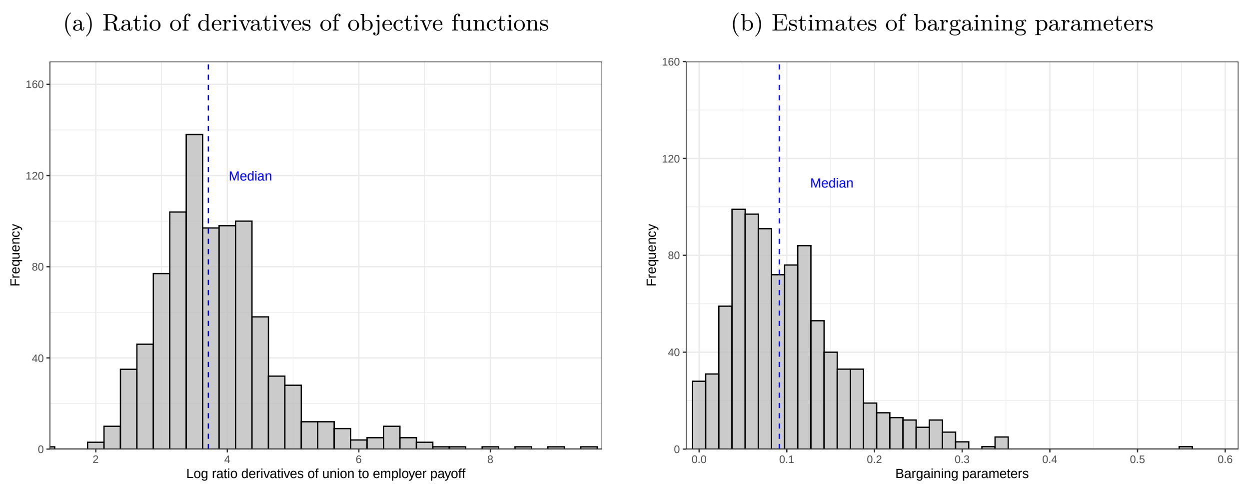
Notes: Data are from a panel of firms that are covered by an exporting CB in 2011–2012. The figure shows estimates of the effects of firm and CB shocks on log mean wages in Panel (a) and log employment in Panel (b). Estimation is done using a difference-in-differences strategy as in Figure 3.5 but keeping firms with extreme values of the pre-period shock. Standard errors are clustered at the CB unit level.

Figure 3.Q: Illustration of firms productivity distribution



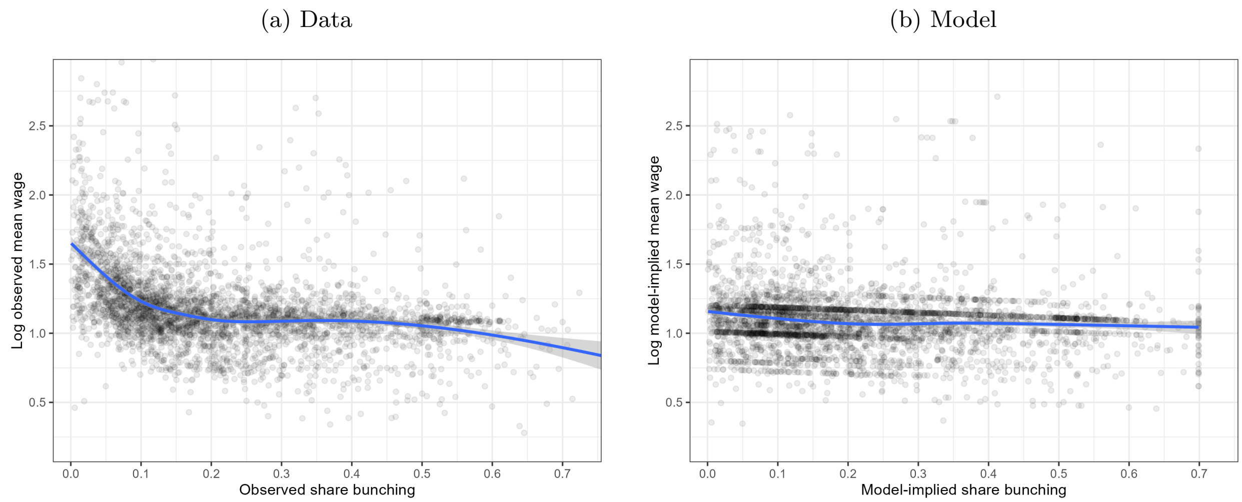
Notes: The figure illustrates the productivity distribution of firms in local labor market g with binding wage floor $\underline{w}_c$, shape productivity parameter α, and minimum value φ_{g0}.

Figure 3.R: Estimates of bargaining power parameters



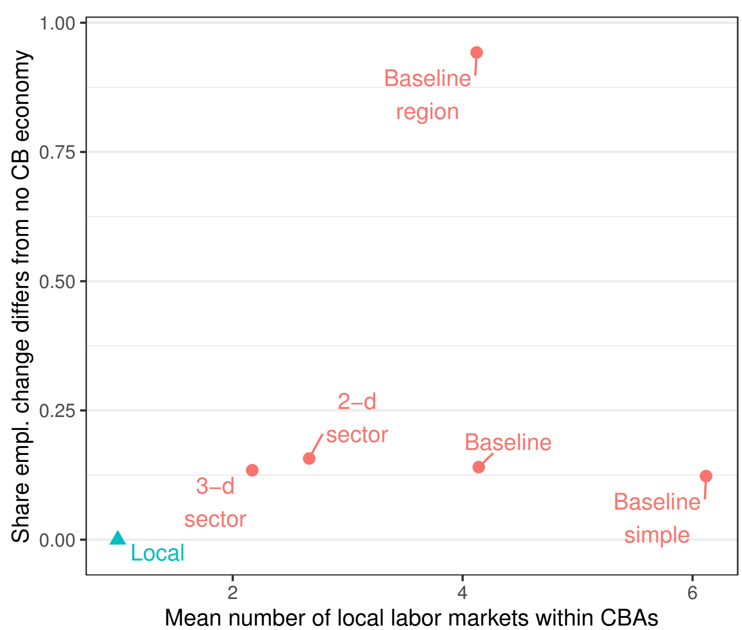
Notes: The figure shows model estimates. Panel (a) shows the estimated ratio of derivatives of the union objective function and the employer association objective function, both with respect to the wage floor. Panel (b) shows the estimated bargaining parameters. I exclude the estimates for the retail CBA 0130/75.

Figure 3.S: Share of firms bunching and mean wage in the data and in the model, for 2011–2012



Notes: The figure shows a correlation between the share of firms bunching and the mean wage in each local labor market. Panel (a) shows the data, and panel (b) shows the model-based data. The line represents a locally estimated scatterplot smoothing curve fitted to the data.

Figure 3.T: Centralization of bargaining and employment responses to shocks across CB networks



Notes: Data are from model simulations pre- and post-export shocks under different CB networks. The figure shows the share of local labor markets with an employment response to shocks different from a non-bargaining economy against the level of bargaining centralization. Specifically, the y-axis measures the share of local labor markets for which the absolute employment change following the shocks is more than 0.25% different from the counterfactual employment change in an economy without CB. This computation excludes local labor markets that correspond to the retail CBA (0130/75) at baseline and to CBAs with less than 5% of employment in exporting firms. Bargaining centralization is measured as the average number of local labor markets per CBA.

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