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The Mixed Effects of Minimum Wage in Imperfect Labor Markets

Author
Munguia Corella, Luis Felipe

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The Mixed Effects of Minimum Wage in Imperfect Labor Markets

DISSERTATION

submitted in entire satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Luis Felipe Munguia Corella

Dissertation Committee:
Professor David Neumark Irvine, Chair
Associate Professor Damon Clark
Associate Professor Yingying Dong

2020

DEDICATION

To

my wife Laura Elisa, and to my parents June and Luis

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I would like to thank the financial support given to me by UCMEXUS and CONACYT. I had the fantastic opportunity to study my PhD without worrying about working and other sources of incomes, which allowed me to concentrate in learning more and focus on research thru the years that I was in UCI.

Finally, I thank to the Ministry of Labor of Mexico, that allowed me to apply all my knowledge and my research findings in this dissertation into policymaking in Mexico.

CURRICULUM VITAE

Luis Felipe Munguía Corella

EDUCATION

University of California, Irvine
PhD in Economics, 2020

University of California, Irvine
MSc in Public-Choice Economics, 2017

El Colegio de México. Mexico City, Mexico
MSc, Economics, June 2008

Universidad de Sonora. Hermosillo, Sonora, Mexico
BS, Economics, December 2004

Research Interests: Labor economics, econometrics, and inequality.

PROFESSIONAL EXPERIENCE AND RESEARCH

President of the Mexican Minimum Wages National Commission, *Mexico, Mexico City, 10/20 – present*
In charge of the minimum wages policy in Mexico.

Technical Director of the Mexican Minimum Wages National Commission, *Mexico, Mexico City, 12/18 – 9/20*
In charge of the research and evaluation of the minimum wages policies in Mexico.

Research Assistant Professor David Neumark, *University of California, Irvine, 4/15 – Present*
Topics: Minimum Wages, Discrimination, Econometrics, Labor Policies Evaluation and Applied Machine Learning.

External Consultant, *The World Bank, Washington, DC, 2/15 – 9/15.*

- Analyze and make a summary of the main literature of the effect of the minimum wage on the productivity. The literature review was used for a study of the effect of the minimum wage in Mexico.
- Analyze and evaluate the impact of taxation and social spending on inequality and poverty in Africa, Latin America, and Eastern Europe.

External Consultant, *Inter-American Development Bank, Washington, DC, 7/15 – 10/15.*
Analyze and evaluate the impact of taxation and social spending on inequality and poverty in Latin America, with emphasis in the effects on gender and race.

Research Assistant Professor Nora Lustig, *University of Tulane, New Orleans, 2/15 – 9/15*
Analyze the impact of taxation and social spending on inequality and poverty in individual countries, and provide a roadmap for governments, multilateral institutions, and nongovernmental organizations in their efforts to build more equitable societies.

Economist of the Office of Foreign Trade, *Central Bank of Mexico, Mexico City, 10/10 – 8/14*
Developed econometric models for seasonal adjustment that correct for shocks related with the Mexican economy. Designed a methodology to calculate an export and import unit value index, this methodology is used internally to analyze the trend of the export and import prices. Wrote internal papers about the external and domestic factors that affect Mexican export industry productivity and employment. Wrote reports about the government expenditure and its effects on industry job and wages.

Deputy Director of Foreign Trade Statistics, *Secretariat of Economy, Mexico City, 1/10 – 10/10*
Wrote a paper about the effect of the trade tariffs reduction on the employment, which was published as a chapter of a book. Worked with the Global Competitiveness Index of the World Economic Forum and make projections in order to calculate the effect of the changes in the trade policy of Mexico in its position in this index. This work was used by the Secretariat to forecast the effects of the implementation of a Single Window for imports.

Consultant of Economic Studies, *Federal Competition Commission, Mexico City, 1/09 – 12/09*
Worked with a database aimed at identifying the effects of the concentration of the Mexican supermarkets in their prices, resulting in an internal publication with Dr. Alejandro Castañeda, this information was used in the case of the Commission against Walmart in Mexico.

Head of the Department of Impact Evaluation, *National Council for the Evaluation of Social Development Policy, Mexico City, 9/08 – 12/08*
Oversaw verifying the soundness of the methodology for the evaluation of diverse social policies in agencies of Mexico. Conducted several meetings with the heads of department in charge of evaluation in their agencies in order to unify criteria and disseminate how an impact evaluation should be conducted, facilitating a better understanding between agencies, and coherent criteria for evaluation. Evaluation of the Mexican Government policies in poverty and inequality.

Teaching Assistant, *Course: Microeconomics, Professor Dr. Gerardo Esquivel, Center of International Studies, El Colegio de México, Mexico City, 8/08 – 12/08*
Taught Microeconomics lab sessions for undergraduate students of public administration. Used examples that helped the students to grasp the essence of the topics covered in the syllabus.

Research Assistant, *Advisor: Dr. Alain Ize, Center of Economics Studies, El Colegio de México/World Bank, Mexico City, 8/08 – 9/08*
Collected data and information from different countries. Made statistical analysis of the data. Made presentations, graphics, and tables with the information.

Research Assistant, *Advisor: Dr. Isidro Soloaga, Center of Economics Studies, El Colegio de México, Mexico City, 5/08 – 8/08*
Reviewed literature on different methods to calculate the poverty rate. Proposed a methodology that uses propensity score matching in order to identify poverty and vulnerability and how government policies affect it.

Research Assistant, Advisor*: Dr. Gerardo Esquivel, Center of Economic Studies, El Colegio de México, Mexico City, 7/07 – 6/08*

Worked with different databases and wrote a paper on the effect of the grown in the inequality between different regions in Mexico. Programmed in GAUSS complex matrix calculations. Programmed in Stata using different types of regressions. Worked in my own dissertation, resulting in online articles and my MSc dissertation (published by El Colegio de México).

Research Assistant, *Coordinator: Dr. Oscar Contreras, Industrial Relations Program, El Colegio de Sonora, Hermosillo, Mexico, 5/05 – 5/06*
Conducted and designed interviews with engineers and blue collar workers of the Ford assembly plant. Compiled and created a dataset using econometrics methods to calculate the impact of Ford in the local economy. Resulting in a report to the Secretariat of Economy, the publication of two articles in peer-review journals and two presentations in a local seminar.

Research Assistant, *Advisor: Dr. Marco A. Cordova, Internship in the Central Bank of Mexico, Hermosillo, Mexico, 10/04 – 4/05*
Compiled national data on main sectors of the economy, with especial emphasis in the manufacturing industry. Created an index with econometrics models that estimates key indicators for the local economy.

PUBLICATIONS AND PAPERS

Munguía Corella, Luis Felipe (2020). "Monopsony Power and Minimum Wages." *IZA Journal of Labor Economics*.

Munguía Corella, Luis Felipe and David Neumark (2020). "Meta-Analysis of Minimum Wages Effects on Employment in Developing Countries". *Elsvier World Development.*

Burn, Ian, Patrick Button, Luis Felipe Munguía Corella, and David Neumark (2020). "Older Workers Need Not Apply? Ageist Language in Job Ads and Age Discrimination in Hiring." *IZA DP* 13506.

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Munguía Corella, Luis Felipe, (2012). 'Impacto de la reducción arancelaria sobre el empleo del sector industrial' [The Impact of Tariff Reduction on Industrial Employment] in Martínez-Trigueros, Lorenza and César Hernández: *La Política del Comercio Exterior. Regulación e Impacto 2006-2012* [The Foreign Trade Policy. Regulation and Impact 2006-2012]. *Instituto Tecnológico Autónomo de México* (ITAM) and the Secretariat of Economy.

Munguía Corella, Luis Felipe (2009). *Crecimiento económico y desigualdad de Ingresos en México* [Economic Growth and Income Inequality in Mexico], publication of dissertation, *El Colegio de México*. Supervisor: Dr. Gerardo Esquivel.

Contreras, Oscar and Luis Felipe Munguía (2007). 'Evolución de las maquiladoras en México. Política industrial y aprendizaje tecnológico' [Evolution of the Maquiladoras in Mexico. Industrial Policy and Technological Learning] in *Region y Sociedad* [Society and Region] special edition (CONACYT Research Journal) *El Colegio de Sonora*.

Contreras, Oscar, Munguía, **Luis Felipe and Vincente Solís** (2006). *Redes globales de producción y aprendizaje local. El caso de los ingenieros de Ford Hermosillo* [Global Networks of Production and Local Learning. The case of Ford Engineers in Hermosillo]. *El Colegio de Sonora.*

Contreras, Oscar, Munguía, **Luis Felipe and Álvaro Bracamonte** (2006). *Impacto de la ampliación de la Ford Motor Co. en Hermosillo, Sonora* [Impact of the extension of Ford Motor Co. in Hermosillo, Sonora]. *FUMEC.*

ABSTRACT OF THE DISSERTATION

Minimum Wages Mixed Effects in Imperfect Labor Markets

By

Luis Felipe Munguia Corella

Doctor of Philosophy in Economics

University of California, Irvine, 2020

Professor David Neumark, Chair

My dissertation's primary contribution is to explain and reconcile the heterogeneous effects of minimum wage on employment documented in the large strand of the literature on minimum wage. In the first chapter, I analyze the effects of minimum wages in developing countries using the enforcement of the law as a plausible explanation of the heterogeneous effects on employment across these countries. I construct a reliable indicator for the degree of enforcement by reading and organizing 82 developing countries' labor codes and quantifying penalties and enforcement degrees. I interact minimum wage changes with the degree of enforcement (grouped in none, weak, and strong enforcement) and estimate minimum wage effects in different enforcement settings. My main results are that the minimum wage has adverse effects on total employment in countries with strong enforcement.

The second chapter explores the causes that explain the different effects of the minimum wage on employment using a meta-analysis of the minimum wage literature in developing countries. The main findings indicate that the heterogeneity is systematic, with estimated effects more consistently adverse in studies with relatively more features for which institutional factors and the competitive

model more strongly predict negative effects. This chapter resulted in a publication by my chair David Neumark and me in the Journal World Development.

Finally, the last chapter analyzes the effect of minimum wages under monopsonistic labor markets. In this chapter, I estimate the effects of the minimum wage for the U.S. under concentrated labor markets and low-mobility jobs (two variables that measure monopsony), identify heterogeneous effects among different scenarios derived from the monopsony model, and provide a plausible explanation of the mixed results about the minimum wage effects in the literature. My main findings indicate that minimum wages have an elasticity to teenage employment of -0.418 under perfect competition, which is, as expected, much higher than the expected results in the literature. The minimum wage has an insignificant positive effect between 0.04 and 0.29 under full monopsonistic labor markets. The results are consistent among different specifications and control for possible external shocks and omitted variables.

1. Minimum Wages and Enforcement Effects on Employment in Developing Countries

1.1 Introduction

Minimum wages are a controversial policy. Following the perfect competition model, increases in minimum wages should have a negative impact on employment. However, while most of the empirical evidence leans toward minimum wages increase having a negative effect on employment, some papers conclude that minimum wages have zero or positive effects on employment. Results are more drastic in developing countries where more studies fail to find any effect on employment. Countries' cultures, institutions, and labor markets are very different across the developing countries; hence, it is unsurprising that evidence about the effects of minimum wages is more mixed.

Are differences in institutions and labor law enforcement among countries sufficient to explain the different effects of minimum wage on employment? Focusing on critical differences across countries may be crucial to get more consistent and interpretable evidence to understand the effects of minimum wage change in developing countries. This paper addresses the question by analyzing the enforcement laws of minimum wages, which vary widely across developing countries. Using a panel with data about minimum wages, enforcement, employment, and other economic variables, I estimate the heterogeneous effects of minimum wage and enforcement on employment.

Most of the literature in developing countries has focused on one or two countries. The results depend on the country, the variable of analysis (total employment or affected employment), whether the law covers the sector of the economy, and whether the minimum wage is binding. However, the results are all over the map. Some studies find negative effects on employment (Alaniz et al. (2011), Bhorat et al (2014), Fang and Lin (2015), Arango and Pachón (2004)) and others report no effects at all (Dinkelman and Ranchhod (2012), Mayneris (2014), Ni et al (2011), Hohberg and Lay (2015), Pelek (2011)).

Studies of minimum wages across countries can be divided into two categories: panel data studies and meta-analyses. Neumark and Wascher (2004) was the only paper that I found that used panel data of different countries. However, they use data from industrialized countries only (members of the Organisation for Economic Co-operation and Development (OECD)). More papers conduct meta-analyses and surveys of the vast literature, but most of them are concentrated in developed countries. The few papers that focus on developing countries are Neumark and Wascher (2007), which is a survey focused on different groups of countries, including developing economies; Cunningham

(2009), which analyze Latin America; Broecke et al. (2017), which studies what they call "emerging economies"[1]; and Munguia and Neumark (2020), that review the literature in developing countries and conduct a meta-analysis to determine what are the factors that determine the mix results of minimum wages in developing countries.

This paper contributes to the literature by analyzing the effect of the minimum wage in a large panel of developing countries, providing a measurement of enforcement of the minimum wage by country as one plausible explanation of the mixed results of the effects of minimum wage on employment. I construct a reliable indicator for the degree of enforcement by reading and organizing 82 developing countries' labor codes and quantifying penalties and the degree of enforcement of the minimum wage policy (grouped in none, weak, and strong enforcement). I interact the effect of the enforcement with the minimum wage. My main results are that the minimum wage negatively affects total employment in countries with stronger enforcement and weak negative to no effects on countries with weak enforcement. The minimum wage's adverse effect is stronger for female and young adult employment because, as shown in the literature, minimum wages tend to be more binding on these groups. These effects are sharper when I interact the enforcement and minimum wages with a measurement of the institutions' quality.

The rest of the paper is organized as follows: In Section 1.2, I review the most relevant literature on minimum wage and enforcement to contextualize the importance of the contribution presented in this paper. Section 1.3 presents the econometric model that is used to identify the effects of minimum wage and enforcement on employment. Section 1.4 describes the data sources and how the panel was constructed. Section 1.5 explains the procedure used to quantify the enforcement variable across all the countries. Section 1.6 presents the main results. In Section 1.7, I run tests to analyze if the results are robust and reliable. Section 1.8 further analyzes the role of enforcement and quality of institutions by interacting my enforcement variable with quality enforcement measures. Finally, in Section 1.9, I conclude.

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[1] Argentina, Brazil, Chile, China, Colombia, India, Indonesia, Mexico, Poland, the Philippines, the Russian Federation, South Africa, Thailand, and Turkey

1.2 Discussion of Relevant Literature

1.2.1 Minimum Wage in Developing Countries

There is considerable heterogeneity in minimum wage policies in the developing world. Like Indonesia, some countries implemented minimum wage as early as 1956, but it was symbolic until 1989 when the law changed to stipulate that minimum wage must at least cover basic needs. Latin American countries like Mexico enacted minimum wage laws in 1935; during the mid-70s, minimum wages were critical in Mexico because they increase significantly average wages. However, in contrast with Indonesia, the minimum wage in Mexico was merely symbolic until 2019. In 2017, the minimum wage amount was so low that only 1% of the population earned it. Besides, some countries have one consolidated national minimum wage (like Argentina and Chile), whereas others have minimum wages that vary by regions and professions[2] (like Mexico and South Africa). Finally, the laws of minimum wage differ significantly across countries. Countries like Mexico and Ethiopia do not have any penalties in the law for companies that fail to abide by minimum wage regulations. However, Guatemala has prison penalties, and in Ecuador, the government can shut down the entire company if an inspector determines that a worker is earning below the minimum wage.

All these differences are key to understanding the diverse effects of minimum wage policies across countries. However, most previous research on the minimum wage has concerned individual countries. A simple comparison of results among different studies in developing countries yields mixed effects of the minimum wage on employment, even when the papers analyze the same region or if the authors are using similar methodologies. The results are mixed. Some papers find negative effects on employment (e.g., Gindling and Terrell (2007), for Costa Rica; Gindling and Terrell (2009), for Honduras; Alaniz et al. (2012), for Nicaragua; Neumark et al. (2006), for Brazil; Maloney and Nuñez Mendez (2004), for Colombia; Suryahadi et al. (2003) for Indonesia). Some studies find positive or no effects (e.g., Lemos (2009), for Brazil; Hohberg and Lay (2015), for Indonesia; Wang and Gunderson (2011), for China; Dinkelman and Ranchhod (2012), for South Africa).

As mentioned earlier, very few papers have analyzed the effects of minimum wage using consolidated data across countries. Comparing between countries can help identify differences that may affect the impacts of the minimum wage on the employment level. One paper that compares the effects of minimum wages is Neumark and Wascher (2004). They analyze the effect of the minimum

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[2] I follow the ILO rules for countries with more than one minimum wage: "In some cases, an average of multiple regional minimum wages is used. In countries where the minimum wage is set at the sectoral level or occupational level, the minimum wage for manufacturing or unskilled workers is generally applied."

wage across 17 OECD member countries. They propose a standard panel with two-way fixed effects and country-specific trends, finding negative effects on teenagers' employment (elasticities of -0.18 to -0.24) and young adults (-0.13 to -0.16). They also interact with measures of the rigidity of the labor market. They conclude that the minimum wage has more adverse effects when labor standards are more restrictive, and when unions have more coverage. However, other labor laws, like employment protection offset these effects.

In addition, there exist some papers that conduct meta-analyses and surveys of the vast literature on minimum wage. Neumark and Wascher (2007) review and conduct a narrative review of literature in different countries, including developing economies. They conclude that papers with the most credible evidence mostly point to negative effects on employment. Cunningham (2009), in a report for the World Bank, describes the literature on minimum wage in Latin America and its effects on employment and wages. She concludes that the evidence is mixed and that the minimum wage effects depend on the institutions present in each country. Broecke et al. (2017) study literature in what they call "emerging economies"; they run meta-regressions and present funnel plots of several papers' estimates in these countries. The authors conclude that the minimum wage has minimal impact on employment and that there is evidence of reporting bias toward statistically negative effects on employment. Finally, Munguia and Neumark (2020) review and systematically classify the literature on minimum wages in developing countries. They identify differences among papers that can explain the minimum wage's heterogeneous effects on employment in the developing world.

Bell (1999) separately explores Mexico and Colombia's cases using panel data with two-way fixed effects, finding that the minimum wage has very different effects in these two countries. In Mexico, the minimum wage had zero effects on employment, whereas the effect was significant and negative in Colombia. There may be several reasons explaining why minimum wage has different effects on employment in two different countries. For instance, one plausible explanation (which can be inferred from Bell's results) refers to the different legal frameworks in each country. For instance, in Mexico, the law does not penalize employers that pay below the minimum wage, whereas, in Colombia, employers that are caught paying less than the minimum wage must pay between 1 and 100 times the hourly minimum wage as a fine (see Table A1).

In summary, the collection of papers that studies minimum wages across countries suggests that differences in institutions and rules can explain, in part, mixed results. Following this rationale, it is striking that to the best of my knowledge, no studies are investigating the role of the enforcement on the minimum wage effects. In my view, it is imperative to consider the enforcement of minimum wage laws because the grade of enforcement determines whether or not the minimum wage level is relevant. Theoretically, if the government does not enforce the rules, economic agents will maximize their profit without any constraints. Agents will decide not to pay minimum wage because no consequences are resulting from their actions.

Some papers focus on analyzing the formal and informal sectors. Their results indicate that the enforcement of minimum wage laws is important. For instance, Gindling and Terrell (2009), as well as Alaniz, Gindling, and Terrell (2011) show that uncovered sectors of the economy in Costa Rica and Honduras, which are never audited by the government, do not comply with minimum wage policy rules. Therefore, they find no significant effects on the employment of the informal sector. In the same vein, Dung (2017) considers the effects of the minimum wage in Vietnam. The author finds negative effects on employment in large companies (a proxy for the formal sector) and no small companies' effects (a proxy for the informal sector). Finally, Gindling and Terrell (2012), using data from Nicaragua, calculated a probit model and found that a 1% increase in the minimum wage reduces the probability of being employed in the formal sector by 0.52% but has no effects on the informal sector.

This paper's unique contribution is that I work with a panel that includes most of the countries in the developing world, and I provide an explanation of the mixed results using a measurement of enforcement. Finally, as in Neumark and Wascher (2004), I interact minimum wage with the enforcement indicator, but the difference is that my indicator is directly related to the minimum wage because it quantifies the enforcement specifically for minimum wage policy.

1.2.2 The Role of Enforcement

It is plausible that differential enforcement of the law explains minimum wage policies' heterogeneous effects in different countries. The political economy literature has analyzed the effect of the enforcement on the effectiveness of different policies. For instance, Scribner and Cohen (2001) review law enforcement on merchant compliance with the minimum legal drinking age; they find that the compliance among stores increased 51% when they received citations for non-compliance. Dasgupa et al. (2000) analyze the enforcement compliance of the environmental laws in Mexico. The authors find that firms maximize profit while accounting for the probability of being cited for non-compliance. Because regulation is Mexico is very weak, the probability of getting caught is small; hence, there is poor compliance with the law. Finally, in the minimum wage case, Gindling and Terrell (2009 and

2011) show that when a sector of the economy is not covered or when the authorities do not enforce the law, the minimum wage is not binding for general wages.

Other papers have focused on analyzing enforcement and laws' effects on economic variables. A very comprehensive analysis of labor laws and codes was conducted by Botero et al. (2004). They found that more rigid regulation has adverse effects on labor force participation. The authors analyze the effect of mandatory benefits, labor laws protecting workers, less flexibility to hire and fire workers, among others, but they do not include minimum wage laws or their effects on employment.

Several papers in political economy analyze the effects of regulation using penalties as a proxy for enforcement, and they conclude that the formal rules matter because they affect the behavior of economic agents—see Botero et al. (2004), La Porta et al. (1997, 1999, 2003, 2004), and Djankov et al. (2002, 2003). Hence, the use of penalties as a proxy for enforcement of the minimum wage is precedented.

1.3 Econometric model

The objective is to measure the minimum wage effects with different degrees of enforcement of the law mandating a minimum wage. Therefore, I use two specifications. One is the canonical two-way fixed effects panel, and I estimate minimum wage effects on the employment rate separately for the sample of countries with no enforcement, weak enforcement, and strong enforcement. The other specification pools the data and includes interactions of the minimum wage and the legal degree of enforcement. The former estimates minimum wage effects by the degree of the enforcement, and the later estimate the differences between the degrees of enforcement. To control by the level of bindingness that the minimum wage has in each country, I use a Kaitz Index (the minimum wage divided by the average wage in each country) instead of the minimum wage in levels. In both specifications, I include covariates to control for differences among countries. The base econometric identification model is:

$$\ln\left(E_{it}\right) = \alpha + \beta\left(\frac{MW_{it}}{Avg\ Wage_{it}}\right) + \boldsymbol{\Gamma X_{it}} + \vartheta_i + \tau_t + t * \vartheta_i + \epsilon_{it} \tag{1.3.1}$$

E_{it} refers to the dependent variable (affected employment rate) in country *i* in time *t*. The variable of interest is the Kaitz Index $\left(\frac{MW_{it}}{Avg\ Wage_{it}}\right)$ where MW_{it} is the minimum wage for a given country *i* and time *t*. Minimum wages year of implementation varies by country. For instance, it was implemented in Mexico in 1974, in contrast, Bangladesh implemented it until 2006. Therefore, the panel is unbalanced, and I am evaluating minimum wages increase and implementations.

Additionally, I add a vector of covariates $\boldsymbol{X_{it}}$ that includes the population's natural logs, gross domestic product (GDP), and the ratio of young adults and women to the total population (these last two variables are used for regression of young adults and female employment rates, respectively)[3]. Finally, as in Neumark and Wascher's (2004) paper on cross-country analysis, I include country fixed effects ϑ_i to control for specific characteristics that are not captured in the covariates, time fixed effect $\tau_t$to control for time shocks, and country-specific trends $t * \vartheta_i$ in order to control for incremental changes in employment associated with longer term developments in labor force participation or labor demand that are unrelated to changes in a country minimum wage. In addition, Allegretto et. al. (2011) state that country-specific trends are needed to assess spatial heterogeneity among observations. That is, there are specific characteristics that are not captured in the two-way fixed effect model, because it assumes that the employment trends (or average wage) are all the same among all regions.

Equation (2) includes the interactions of the minimum wage and the variable of enforcement:

$$\ln\left(E_{it}\right) = \alpha + \beta\left(\frac{MW_{it}}{Avg\ Wage_{it}}\right) + \gamma_1 enf\_weak_i + \gamma_2 enf\_strong_i + \gamma_3\left(enf\_weak_i * \left(\frac{MW_{it}}{Avg\ Wage_{it}}\right)\right) + \gamma_4\left(enf\_strong_i * \left(\frac{MW_{it}}{Avg\ Wage_{it}}\right)\right) + \boldsymbol{\Gamma X_{it}} + \vartheta_i + \tau_t + t * \vartheta_i + \epsilon_{it} \quad (1.3.2)$$

The added variables are as follows: enf_weak_i, which is a dummy variable equal to 1 if there is a weak penalty for not complying with the law (e.g., small fines) and 0 otherwise; and enf_strong_i, a dummy variable equal to 1 if there is a strong penalty (e.g., costly fines or time in prison) of the law and 0 otherwise. The omitted dummy is "No Enforcement," which means that all the enforcement dummies are relative to countries with no enforcement. Note that the enforcement dummies do not change over time; this is explained in more detail in section 1.5.

The dummy variables are interacted with $\left(\frac{MW_{it}}{Avg\ Wage_{it}}\right)$. Hence, β is the effect of *MW* in the absence of any punishment (omitted variable), and γ_3 and γ_4 are the difference in the minimum wage between a country with enforcement (weak and strong, respectively) and countries with no

[3] Population is used to control for the size of each country labor force, ratios of young adults and women to population is used for control by the cohort and group size, and the GDP controls for the business cycle

enforcement. Specifically, the effect of weak enforcement is $\beta + \gamma_3$, and the effect of strong enforcement is $\beta + \gamma_4$.

One possible threat to my identification is that the minimum wage might be endogenous. Therefore, I test for the possibility in section 7. Ideally, this issue can be solved using an instrument variable, but I do not have one. Instead, I test if the policies were determined endogenously using a specification with one-year lag and one-year lead for both equations. The validation test indicates that it is unlikely that endogeneity or pre-trends bias the results when I control for country-specific trends (i.e., leads are not significant). Therefore, my country-specific trend is preferred.

1.4 Data: ILOSTAT and Harmonized Developing Countries Data

Collecting data for all developing countries would be a challenging task. Some countries have poor statistics websites, and the English translation is not always accurate. However, the International Labour Organization (ILO) publishes a rich database of labor indicators. The statistics department ILOSTAT[4] reports statistics of all member countries.[5] The statistics include employment by sex, age, education, level of skill, and economic activity (including informal economic activity for 24 countries); earnings by sex and economic activity; the number of hours worked by sex and age; and minimum wages. Besides, I collected average wages per country, available in the ILO document Global Wage Report.[6]

This database's information covers most of the variables of interest: total employment, employment by age and sex, earnings, wages, and minimum wages. The ILO also harmonizes most of the different countries' time series with a standard methodology that facilitates cross-comparison within and between countries. However, in the case of earnings, the ILO does not have harmonized data; they report the countries' administrative data, but breaks in the series make these data very limited. I rely more on average wages[7] from the Global Wage Report because even though it is not harmonized, there are no breaks in the series, and it seems comparable within and between countries. Average wages are used to construct a Kaitz index.

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[4] https://www.ilo.org/ilostat

[5] As of April 2016, the ILO has 187 state members. 186 of the 193 member states of the United Nations plus the Cook Islands are members of the ILO.

[6] http://www.ilo.org/global/research/global-reports/global-wage-report/2016/lang--en/index.htm

[7] Note that the difference between earnings and wages is that earnings includes all employment of benefits and wages is only the salary per month.

According to their income, modern classification of countries divides them into low-income economies, lower-middle-income economies, upper-middle-income economies, and high-income-economies. I am analyzing 82 countries using the same classification as in Fields (2010). He defines "developing [countries]" as all the countries with low and middle income: in other words, only excluding the high-income economy category.

The variables used in the paper are as follows. Table 1.1 reports a summary of the mean of these variables.

1) Minimum Wage: The minimum wage paid in each country in local currency and adjusted by Purchasing Power Parity (PPP).
2) Average Wage: Average wage paid in each country in local currency and adjusted by PPP[8].
3) Total Employment[9]: Total employment by country.
4) Employment of Young Adult Workers: Total employment of young adults (defined as workers 15–24 years old).
5) Employment of Female Workers: Total employment of female workers.
6) Employment of Unskilled Workers: Total employment of unskilled workers, defined as workers with low and medium occupational skill levels. Low-level occupations are elementary occupations based on the International Standard Classification of Occupation (ISCO): examples are carpenters, painters, etc. Medium skill workers are blue-collar industrial workers.[10]
7) Employment of Unskilled Female Workers: Total employment of unskilled female workers.
8) Quality of Institution Index: Calculated by the World Bank, this index includes government efficiency measures, control of corruption, regulatory quality, and the rule of law (see Appendix C for more information). I only use regulatory quality.

Table 1.1 Average of the main variables by country, period 1996–2015

Country	Kaitz Index	Total Employment Rate	Teenage Employment Rate	Unskilled Workers Employment Rate	Female Employment Rate	Regulatory Quality Index
Albania	0.544	0.363	0.312	0.310	0.304	0.807
Algeria	0.470	0.239	0.207	0.191	0.061	-0.657
Argentina	0.659	0.386	0.353	0.286	0.291	-0.184

[8] I only collect data of average wages of total workers, it was not possible to get data by groups of workers.
[9] All the workers are in the formal sector of the economy. There was not possible to analyze the effect of minimum wages on the informal economy because the information is not available for most of the countries and periods.
[10] The reason that I am including medium level of skill is because this category includes blue collar workers for basic industries. For more information on the classification, see: http://www.ilo.org/public/english/bureau/stat/isco/isco68/major.htm

Country	Kaitz Index	Total Employment Rate	Teenage Employment Rate	Unskilled Workers Employment Rate	Female Employment Rate	Regulatory Quality Index
Armenia	0.225	0.375	0.232	0.270	0.320	1.067
Azerbaijan	0.158	0.433	0.348	0.338	0.407	-0.083
Bangladesh	0.486	0.419	0.517	0.372	0.306	-0.744
Benin	0.825	0.397	0.526	0.375	0.366	0.150
Bolivia	0.442	0.430	0.491	0.381	0.362	-0.041
Botswana	0.173	0.391	0.407	0.325	0.356	1.749
Brazil	0.319	0.439	0.492	0.357	0.351	1.104
Bulgaria	0.395	0.394	0.245	0.274	0.362	1.622
Burkina Faso	0.726	0.433	0.748	0.426	0.411	0.520
Burundi	0.050	0.445	0.676	0.427	0.453	-0.986
Cambodia	0.000	0.496	0.725	0.469	0.484	0.174
Chad	0.624	0.349	0.516	0.333	0.310	-0.927
China	0.447	0.555	0.576	0.505	0.513	0.504
Colombia	0.538	0.398	0.363	0.331	0.295	1.184
Costa Rica	0.295	0.405	0.422	0.310	0.272	1.644
Croatia	0.342	0.390	0.259	0.275	0.333	1.432
Cuba	0.529	0.421	0.361	0.271	0.302	-1.348
Dominican Republic	0.525	0.359	0.364	0.305	0.242	0.591
Ecuador	0.532	0.411	0.433	0.344	0.303	-0.627
Egypt	0.092	0.275	0.233	0.191	0.107	0.174
El Salvador	0.561	0.382	0.420	0.336	0.298	1.189
Ethiopia	0.393	0.427	0.723	0.417	0.380	-0.908
Fiji	0.902	0.352	0.357	0.292	0.227	0.067
Gambia	0.250	0.292	0.368	0.272	0.237	0.142
Georgia	0.105	0.455	0.261	0.344	0.408	0.889
Ghana	0.301	0.407	0.433	0.382	0.400	0.736
Guatemala	0.678	0.356	0.528	0.324	0.232	0.648
Honduras	0.531	0.369	0.492	0.329	0.240	0.336
India	0.376	0.372	0.391	0.335	0.208	0.262
Indonesia	0.627	0.434	0.413	0.402	0.325	0.270
Iran, Islamic Republic of	0.848	0.271	0.247	0.227	0.084	-1.351
Jamaica	0.225	0.409	0.326	0.334	0.342	1.215
Jordan	0.357	0.218	0.185	0.153	0.060	1.214
Kazakhstan	0.204	0.471	0.442	0.348	0.437	0.235
Kenya	0.309	0.340	0.351	0.322	0.310	0.502
Kyrgyzstan	0.083	0.394	0.405	0.323	0.331	0.298
Lao People's Democratic Republic	0.355	0.464	0.651	0.441	0.471	-0.958
Lebanon	0.725	0.304	0.236	0.211	0.135	0.590
Lesotho	0.714	0.304	0.349	0.276	0.264	-0.114
Macedonia	0.417	0.296	0.160	0.228	0.231	1.025
Madagascar	0.446	0.469	0.719	0.456	0.455	0.075
Malawi	0.155	0.400	0.537	0.380	0.397	-0.059
Malaysia	0.382	0.415	0.404	0.312	0.300	1.702
Maldives	0.381	0.370	0.435	0.266	0.277	0.932
Mali	0.524	0.277	0.425	0.264	0.198	0.106
Mauritius	0.212	0.417	0.373	0.346	0.281	1.810
Mexico	0.262	0.395	0.468	0.325	0.266	1.420
Moldova, Republic of	0.339	0.384	0.263	0.320	0.368	0.549
Mongolia	0.294	0.387	0.353	0.305	0.356	0.494
Montenegro	0.397	0.327	0.204	0.198	0.279	0.879
Nepal	0.899	0.503	0.751	0.484	0.491	-0.268
Nicaragua	0.291	0.353	0.416	0.298	0.247	0.357
Niger	0.088	0.311	0.522	0.297	0.187	-0.214
Nigeria	0.042	0.294	0.326	0.276	0.238	-0.642
Pakistan	0.726	0.299	0.388	0.243	0.103	-0.311
Panama	0.465	0.399	0.388	0.323	0.280	1.508
Paraguay	0.852	0.425	0.555	0.367	0.322	-0.075
Peru	0.335	0.447	0.491	0.367	0.378	1.482
Romania	0.307	0.448	0.307	0.360	0.399	1.452

Country	Kaitz Index	Total Employment Rate	Teenage Employment Rate	Unskilled Workers Employment Rate	Female Employment Rate	Regulatory Quality Index
Russian Federation	0.061	0.470	0.353	0.285	0.428	0.273
Rwanda	0.010	0.474	0.729	0.462	0.488	-0.016
Senegal	0.207	0.293	0.407	0.277	0.214	0.558
Serbia	0.417	0.359	0.213	0.259	0.292	0.275
Solomon Islands	0.275	0.268	0.266	0.225	0.240	-1.119
South Africa	0.182	0.275	0.157	0.216	0.230	1.544
Sri Lanka	0.470	0.373	0.311	0.309	0.228	0.805
Syrian Arab Republic	0.569	0.255	0.298	0.217	0.077	-1.082
Tajikistan	0.133	0.360	0.402	0.304	0.321	-0.968
Tanzania, United Republic of	0.256	0.458	0.722	0.441	0.446	0.149
Thailand	0.625	0.561	0.511	0.487	0.505	1.250
Timor-Leste	0.162	0.272	0.303	0.234	0.176	-1.062
Togo	0.248	0.414	0.579	0.391	0.407	-0.568
Tunisia	0.372	0.291	0.240	0.236	0.136	0.754
Turkey	0.418	0.319	0.363	0.258	0.183	1.338
Uganda	0.027	0.410	0.648	0.388	0.408	0.773
Ukraine	0.363	0.452	0.333	0.299	0.415	-0.113
Uzbekistan	0.143	0.360	0.362	0.298	0.289	-1.454
Venezuela	0.527	0.393	0.372	0.321	0.293	-0.970
Viet Nam	0.391	0.539	0.624	0.497	0.515	-0.267

Notes: Kaitz Index = (MW/Avg Wage). The Regulatory Quality Index is part of the World Bank Worldwide Governance Indicators. The index is normalized, negative means that the regulatory quality is below the average in all the countries, positive indicates that the quality is above the mean. See Appendix for more details.

1.5 Measurement of Law Enforcement

In my base model, I examine the minimum wage effect on employment using a panel of developing countries from 1994 to 2016. A compilation of minimum wage laws has been done by Cunningham (2009); however, the author only gathered the information for some Latin American countries and did not study the effects of enforcement. To test the effect of the enforcement, I use minimum wage laws by country. To construct a reliable indicator for the degree of enforcement, I read and organized the labor codes of 82 developing countries. My primary source is the ILO's "Database of National Labour, Social Security and Related Human Rights Legislation" (NALEX),[11] which compiles records of labor laws of 196 countries and 160 territories. Most of the laws were in English, but there were also French or Spanish, which I translated into English. However, sometimes the law is only available in its original language (Russian); I used Google Translator in those cases.

In addition, I complemented the NALEX information using the "Country Reports on Human Rights Practices[12] from the U.S. Department of State's Bureau of Democracy, Human Rights and Labor. This report contains an assessment of labor law enforcement's effectiveness, the level of the minimum wage (if any), and sometimes penalties for not complying with the law. The report and

[11] http://www.ilo.org/dyn/natlex/natlex4.home?p_lang=en
[12] https://www.state.gov/j/drl/rls/hrrpt/humanrightsreport/index.htm

NALEX information agree on the enforcement's effectiveness, and I mainly used the report to confirm that the classification was robust. I estimate the primary results when the two sources have contradictions, but the results do no change substantially (See Appendix B).

The ILO (2014) document about minimum wage systems has a classification of mechanisms to enforce minimum wage compliance. They are as follows: (1) financial penalties; (2) financial penalties or infractions per worker; (3) financial penalties and increase in case of repeat offense; (4) financial penalties per worker and imprisonment in case of repeat offense; (5) financial penalties per worker, shut down, freeze of subsidies in case of repeat offense; (6) no punishment and presence of collective bargaining;[13] and (7) no enforcement. The presence of the last option (7) implies that the law is "incomplete," meaning that the law states that a minimum wage must be paid, but it does not say anything about penalties when a firm does not comply. It is expected that the issue will be resolved in court and that a judge will decide the penalties[14].

Laws and labor codes differ by country substantially, but to simplify the enforcement's effect, I divided the law into weak and strong depending on the punishment for not fulfilling the law. The ILO (2014) document includes examples of each group, which I used to classify the countries' labor codes by these seven groups. Countries with labor codes that pertain to groups 1, 2, and 3 are classified as having weak enforcement; labor codes that belong to groups 4 and 5 are classified as strong; finally, countries that belong to groups 6 and 7 are classified as having no enforcement.

I present an example of each classification in Table 1.2. In column (6) I provide the classification according to ILO classifiers, column (7) is a summary of the comments of the Country Reports on Human Rights Practices about the labor codes, and, in column (8), I report the relevant text of the labor laws or codes of each country. Ghana does not have any penalty specified in its Labor Act of 2003. It only established a Tripartite Committee that oversees the minimum wage rate, but the law does not specify what happens when an establishment does not abide by the law. Moreover, as indicated in column (7), minimum wages are below the poverty line.[15] Hence, Ghana is classified under "No Enforcement." In the case of Burundi, the law specifies fines that can be increased in recidivism case. However, the fines are small (up to 5.60 USD). Burundi is classified as a "Weak Enforcement"

[13] The ILO argues that it is expected that unions might help enforcing the law. However, it has shown that in developing countries, collective bargaining does not always represent the interests of the workers and they do not enforce the minimum wage law.

[14] Usually, since minimum wages are not penalized, fines defined in court are very low. For instance, in Mexico, where the is not penalties in the minimum wage law, on average, workers recover less than 30% of their claim. In addition, workers receive higher percentages of their claims in settlements than in trial judgements (Kaplan et al. 2007).

[15] The minimum wage is 1.61 USD and the poverty line is 1.90 USD (according to the World Bank).

country. Finally, Bolivia has strong penalties and a reliable mechanism to inspect the companies, the fines are costly (up to 1,447 USD per violation), and they might shut down the establishment in recidivism case; hence Bolivia is classified as a "Strong Enforcement" country.

Table 1.2 Examples of the Classification of the Enforcement Variable using Labor Law and Codes, and Reports on Human Rights Practices

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices	Labor Law or Code
Ghana	1	0	0	None	(7) No enforcement	MW is below extreme poverty necessary income. There was widespread violation of the minimum wage law.	**Labor Act, 2003** Part XIII. National Tripartite Committee Art 113. (1) The National Tripartite Committee shall (a) determine the national daily minimum wage; (…) (2) The Minister shall publish in the Gazette and in such public media as the Minister may determine, a notice of the national daily minimum wage determined under subsection (1). (3).The Ministry shall provide the National Tripartite Committee with such secretarial services as the Committee may require for the effective performance of its functions. (...) No penalties specified.
Burundi	0	1	0	Financial penalties	(3) FP increase if repeat	Law enforcement with low fines (3 to 12 US dollars).	**Labor Code** Art 292. The authors of infringements of the provisions of articles 2 (…) as well as their measures of executions are punished a fine from 2500 to 5000 Burundi francs (1.40 to 2.80 USD) , and in recidivism case, from 5000 to 10000 Burundi francs (2.80 to 5.60 USD).

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices	Labor Law or Code
Bolivia	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	Random inspections are common. Increased penalty and establishment may be closed down. Aggresive increases of MW.	**Ministerial Resolution 855/14:** Art 1. Employers (…) have the obligation to present quarterly information on salaries and work accidents. All the information is jury. Art 5 I. (...) Delays in presenting the information (up to 180 days) will received a fine from 1000 bolivianos to 10,000 bolivianos (145 to 1,447 USD). II. More than 180 of delay, the fine will be equivalent to 40% of revenues plus another fine from 1000 to 10,000 bolivianos depending on the number of workers. Art 11. The Minister of Labor, Employment and Social Security, may conduct necessary inspections the verified the information provided by employers. If there is evidence of false information, corresponding fines will applied (The fines are defined in the General Labor Law). Art 12. Fines will apply from 1,000 to 10,000 bolivianos by (1) Not avoiding with the Social Law (minimum wages) (...). **General Labor Law** Title XI. Art. 165: Fines of the General Labor Law : Fines from 50 to 150,000 bolivianos (from 7 to 21,725 USD) can be applied on case basis. In recidivism case, fines can be double, and it might result in a fiscal intervention and shut

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices	Labor Law or Code
							down of the establishment.

Source: NATLEX

Some countries with no enforcement of the minimum wage law are Afghanistan, Angola, Congo, Kenya, and Mexico. Some examples of countries with weak enforcement are Albania, Brazil, Dominican Republic, and Ukraine. Some examples of countries with strong enforcement are Bangladesh, Bolivia, Ecuador, Guatemala, and Vietnam. A summary of all classification of the laws and labor codes by countries is provided in Appendix B Table A1.

It is important to mention that the dummies of enforcement do not change over time. I am aware that it is more desirable to capture changes in the labor codes that have a fixed indicator; however, labor codes barely change once a minimum wage policy is enacted in the law. For instance, minimum wage law has not changed in Mexico since 1974, and in Bangladesh, it has not changed since 2006, the year when the minimum wage was established for the first time.

Before I proceed, it is essential to discuss a concern with this approach of measuring enforcement with the penalties in the law. The enforcement of rules varies among developing countries, and therefore formal rules and penalties might provide little information on the degree of enforcement of the law. Indeed, I cannot measure the enforcement directly. However, I roughly control the heterogeneity of the enforcement in these countries by using countries' fixed effects, country-specific trends, and interactions of institution quality indicators with minimum wages (Section 8). Also, despite the criticism that the formal rules do not matter, it has been shown in Botero et al. (2004), La Porta et al. (1997, 1998, 2003, 2004), and Djankov et al. (2002, 2003) that rules matter a lot in compliance with economic policies.

1.6 Results: Effect of Minimum Wage on Employment

In this section, I present the results. I estimate the effect of minimum wages on employment, including the effect of enforcement of the law. The estimates include different groups of workers (young adults, unskilled and female workers).

In Table 1.3, I estimate the minimum wage effect (measure as a Kaitz Index) on the log of the employment rate without any interaction with the degree of enforcement. However, each column includes different order trends. The objective of using trends of different orders is twofold: firstly, to check if the results are robust to different trends, and secondly, to reduce possible problems of heterogeneity across countries that can bias the results.

Allegretto et al. (2011) raised the issue that spatial heterogeneity can bias minimum wage estimates. Hence, it is important to assess spatial heterogeneity among observations (as in their case states of the U.S.). There are specific characteristics not captured in the two-way fixed effect model because it assumes that the employment trend is the same among all countries. These trends are unique of each country and can confound the minimum wage effect; therefore, most of the specifications have different country-specific trends. However, as Neumark et al. (2014) suggested, specific linear trends can be misleading. They suggest that the results must be robust to different polynomial order trends. Hence, I test polynomial trends of the 1st, 2nd, 3rd, and 4th order, and I also calculate a trend using an HP filter.

Table 1.3 Effect of Minimum Wages on Total Employment Rate

Dependent Variable: Ln (Emp Rate)	(1) Without Country Trends	(2) 1st Order Trend	(3) 2nd Order Trend	(4) 3rd Order Trend	(5) 4th Order Trend	(6) HP Filter Trend
Kaitz Index						
Coefficient	0.0374*	0.000602	0.0319	0.0133	-0.0228	-0.00180
	(0.0216)	(0.0250)	(0.0209)	(0.0190)	(0.0138)	(0.00576)
Elasticity	0.014*	0.000	0.012	0.005	-0.009*	-0.001
	(0.008)	(0.009)	(0.008)	(0.007)	(0.005)	(0.002)
R-squared	0.881	0.957	0.972	0.979	0.984	0.986
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Specific Trend	No	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment rate is in logs. Kaitz Index = (MW/Avg Wage). The panel is unbalanced, and it includes 82 countries, period 1994–2016, and 10.7 years on average. The incremental R^2 for country-specific trends is explaining between 0.073 to 0.078. Controls included: Log of the total population, log of the GDP and log of one lag of the GDP.

In column (1), I am controlling only for two-way fixed effects (country and time); column (2) has controls for two-way fixed effects and country-specific time linear trend; column (3) has controls for two-way fixed effects and country-specific 2nd order trend, (4) for 3rd order trend, and (5) for 4th order trend. In column (6), I use a trend calculated with the HP filter. This last tool has been widely used to identify the trend and the cycles of a time series.[16] Finally, it is important to mention that the

[16] Hamilton (2017) argues that "…the filter produces spurious dynamic relations and values at the end of the sample are very different from those in the middle." However, for the purpose of this paper, it is better to use HP filter, because Hamilton filter is designed for long series of high frequency data, and it needs to drop 4 periods before and after, losing 8 years in total.

incremental R^2 is around 0.07 for Table 1.3, but for other tables, it is around 0.08, which is the proportion of the R^2 explained by merely adding country-specific trends. Typically, the R^2 is high (above 0.9) when country-specific trends are added. In column (1), there is a significant positive effect of the minimum wage on employment; however, when I control for country-specific trends in the rest of the specifications, I cannot reject the null hypothesis that minimum wage has no significant effects. These results are very similar for the rest of the estimations. However, I report lineal trends in all the next tables (my prefer specification), all specifications with two-way fixed effects and different country-specific trends are available upon request. In addition, I report the coefficients and the elasticities (because the Kaitz Index is not in logs) for all the results.

In Table 1.4 I present my main results. I look at three groups of countries and four types of employment. In columns (1) to (3), I report the effect on total employment grouping countries according to their enforcement degree. Only the countries with strong enforcement have negative and significant effects on employment. The elasticity of the minimum wage effect on total employment in countries with strong enforcement is -0.030. This elasticity is similar to results for the total employment found by Lemos (2004d) and Neumark et al. (2006) in Brazil; Rama (2001) in Indonesia; and Fang and Lin (2015) in China.

The elasticity on total employment is small because the estimation corresponds to the pool of all workers among all the wage distribution, i.e., many workers that earn above the minimum wage are not affected. Theory predicts that the most affected workers are those with lower wages, i.e., young adults, low skill, and female workers. The literature on developed countries shows that minimum wage typically has negative effects on younger adults and unskilled workers (Neumark and Wascher, 1992, 1994a, 1996 and 2001; Zavodny, 2000; Sabia, 2006; Neumark and Wascher, 2004; Yuen, 2003). For developing countries, results are more mixed. Therefore, I explore the effect of minimum wages on these groups. Column (4) to (6) refers to unskilled workers, column (7) to (9) to young adults, and (10) to (11) to female workers.

The effect of the minimum wage is adverse across all groups when the enforcement is strong. For unskilled workers, the minimum wage in weak enforcement countries is significant and positive (elasticity of 0.034) and negative (elasticity of -0.024) when the enforcement is strong. For young workers, the only significant effect is negative (-0.058) when the country's enforcement is strong. Finally, the minimum wage harms female workers in countries with strong enforcement too. The elasticity for female workers is -0.055. All the elasticities in these vulnerable groups are larger in magnitude with respect to the total workers (except unskilled workers). These results are robust to different specifications and different polynomial trends.

Next, I further analyze the minimum wage policy's effect by estimating if the difference between minimum wage effects in countries with strong and weak enforcement is significantly different from the baseline (countries with no enforcement). Table 1.5 has four specifications, as before; in column (1), I report total employment, column (2) unskilled workers, (3) young adults, and (4) female workers. This table's objective is to test if the effects of minimum wage on wages are significantly different between countries with no enforcement compared to countries with weak and strong enforcement.

The baseline estimation is not significantly different from zero, and neither is the interaction for weak enforcement (except for unskilled, where weak enforcement is significant and positive). This finding is consistent with Table 1.4; minimum wage policy does not significantly affect employment if there are no penalties in the law for not complying. By contrast, the interaction with strong enforcement is negative and significant for total and female workers (difference of elasticity of -0.008 and -0.021, respectively), which means that the countries with strong enforcement have a negative differentiated effect with respect to the baseline for these two types of workers. For instance, using total employment estimates, the minimum wage elasticity in countries with strong enforcement is 0.005 – 0.008 = - 0.003.

The interactions are not significantly different from zero for unskilled and young workers. This finding does not mean that the minimum wage has no effects on unskilled and young employment, but rather that the elasticity in countries with strong and weak enforcement is not significantly different from countries with no enforcement[17].

These results are not surprising; studies show that minimum wage has stronger negative effects on female workers in developing countries (Feliciano, 1998; Suryahadi et al., 2003; Arango and Pachón, 2004; Chun and Khor, 2010). To further explore which group of female workers is most affected by the policy, I broke down the effect of the minimum wage on female workers by skill level. In Table 1.6, I present the effects of minimum wage and enforcement by the skill required for the female workers' occupation. Specifications (1), (2), and (3) estimate the effects of the minimum wage in countries with no, weak, and strong enforcement, respectively, on low-skilled female workers. In

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[17] The results change when I interact with the quality of institutions (Table 1.8), where the minimum wage interaction has negative effects across all the group of workers.

(4), (5), and (6), I estimate for high-skilled female workers. The minimum wage is significant and negative for low-skilled female workers in countries with strong enforcement (column 3). The elasticity (-0.052) is very similar to the elasticity of total female workers (-0.055). Coefficients for highly skilled females in countries with weak and strong enforcement are negative; however, the estimates are not significantly different from zero. Moreover, the effect on countries with no enforcement is positive and significant.

Table 1.4 Effect of Minimum Wages on Log of Total, Unskilled, Young Adult, and Female Employment Rates–Countries Grouped by Different Degree of Enforcement

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Ln (Emp Rate)	Total			Unskilled		
	No enforcement	Weak Enforcement	Strong Enforcement	No enforcement	Weak Enforcement	Strong Enforcement
Kaitz Index	-0.0200	0.0332	-0.0667*	-0.0622	0.0807**	-0.0533
	(0.0382)	(0.0369)	(0.0362)	(0.0450)	(0.0359)	(0.0355)
Elasticities	-0.00573	0.0138	-0.0302*	-0.0178	0.0335**	-0.0241
	(0.0109)	(0.0153)	(0.0164)	(0.0129)	(0.0149)	(0.016)
Observations	277	414	157	277	414	157
R-squared	0.984	0.984	0.990	0.984	0.980	0.989
Number of Countries	30	35	17	30	35	17
	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable: Ln (Emp Rate)	Young Adults			Female		
	No enforcement	Weak Enforcement	Strong Enforcement	No enforcement	Weak Enforcement	Strong Enforcement
Kaitz Index	-0.0681	0.0335	-0.128**	0.0300	0.0485	-0.121*
	(0.114)	(0.0714)	(0.0546)	(0.0641)	(0.0648)	(0.0660)
Elasticities	-0.0195	0.0139	-0.0581**	0.00859	0.0202	-0.0549*
	(0.0327)	(0.0297)	(0.0247)	(0.0183)	(0.0269)	(0.0298)
Observations	277	414	157	277	414	157
R-squared	0.980	0.978	0.982	0.993	0.992	0.992
Number of Countries	30	35	17	30	35	17

Robust standard errors in parentheses clustered by countries

*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment rate is in logs. Kaitz Index = (MW/Avg Wage). The panel is unbalanced, and it includes 82 countries (grouped in different samples), period 1994–2016, and 10.7 years on average. The incremental R^2 for country-specific trends is explaining around 0.08 for total employment, 0.10 for unskilled employment, between 0.15 to 0.5 for teenagers, and 0.29 for female workers. Two-way fixed effects and linear country-specific trends are used (specifications with other polynomial trends are available upon request). Unskilled employment is calculating using low and medium skill classifications (which include painters, carpenter, blue-collar workers among others), young adults are workers between 15 and 24 years old. Controls for total and unskilled workers includes log of population, log of the relative size of youth to the population for young adult workers, and log of the relative size of the female to the population for female workers. Log of the GDP and log of one lag of the GDP are included for all specifications

Table 1.5 Effect of the Minimum Wages and Interactions with Enforcement on Log of Total, Unskilled, Young Adults and Female Employment Rates

Dependent Variable: Emp Rate	(1) Total Employment	(2) Unskilled Employment	(3) Young Adults Employment	(4) Female Employment
Coefficients				
Kaitz Index	0.0135	-0.0142	0.00659	0.0708
	(0.0354)	(0.0279)	(0.109)	(0.0542)
Kaitz Index x Weak Enforcement	0.0108	0.0863*	0.0309	-0.0276
	(0.0517)	(0.0484)	(0.128)	(0.0873)
Kaitz Index x Strong Enforcement	-0.0949*	-0.0614	-0.129	-0.250**
	(0.0506)	(0.0489)	(0.127)	(0.0989)
Elasticities				
Kaitz Index	0.00512	-0.00538	0.0025	0.0269
	(0.0135)	(0.0106)	(0.0415)	(0.0206)
Kaitz Index x Weak Enforcement	0.00219	0.0175*	0.00625	-0.00558
	(0.00219)	(0.0175)	(0.00625)	(-0.00558)
Kaitz Index x Strong Enforcement	-0.00795*	-0.00514	-0.0108	-0.021**
	(0.834)	(0.0746)	(0.809)	(0.752)
Observations	850	850	850	850
R-squared	0.985	0.983	0.977	0.992
Time FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Country-Specific Trend	Yes	Yes	Yes	Yes

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment rate is in logs. Kaitz Index = (MW/Avg Wage). The panel is unbalanced, and it includes 82 countries, period 1994–2016, and 10.7 years on average. The incremental R^2 for country-specific trends is explaining around 0.07. Weak and strong enforcement is measured with a dummy variable equal to 1 if the country has weak or strong enforcement (high penalties). The enforcement dummies are fixed in time. Dummies not interacted are dropped because they are colinear to country fixed effects. The omitted dummy is "no enforcement." Lineal country-specific trends are used (specifications with other polynomial trends are available upon request). Unskilled employment is calculating using low and medium skill classifications (which include painters, carpenter, blue-collar workers among others), young adults are workers between 15 and 24 years old. Controls for total and unskilled workers includes log of population, and log of the relative size of youth to the population for young adult workers, and log of the relative size of the female to the population for female workers. Log of the GDP and log of one lag of the GDP are included for all specifications.

Table 1.6 Effect of the Minimum Wages on Log Female Employment Rate by Level of Skill—Countries Grouped by Different Degree of Enforcement

	(1)	(2)	(3)	(4)	(5)	(6)
	Low Skill Female Workers			High Skill Female Workers		
Dependent Variable: Emp Rate	No enforcement	Weak enforcement	Strong enforcement	No enforcement	Weak enforcement	Strong enforcement
Kaitz Index	-0.0155	0.0955	-0.115*	0.302**	-0.158	-0.128
	(0.0736)	(0.0724)	(0.0622)	(0.123)	(0.115)	(0.110)
Elasticities	-0.00442	0.0397	-0.0521*	0.0864**	-0.0656	-0.0578
	(0.021)	(0.0301)	(0.0281)	(0.0353)	(0.0478)	(0.0499)
Observations	298	414	162	298	414	162
R-squared	0.992	0.992	0.991	0.996	0.986	0.990
Number of Countries	30	35	17	30	35	17
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-Specific Trend	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment rate is in logs. The panel is unbalanced, and it includes 82 countries (grouped in different samples), period 1994–2016, and 10.7 years on average. The incremental R^2 for country-specific trends is explaining between 0.20 and 0.40 of the variation. Linear country-specific trends are used (specifications with other polynomial trends are available upon request). Controls for female workers include log of the relative size of the female to population and log of the GDP and log of one lag of the GDP. Unskilled female workers are calculated as unskilled employment in previous tables.

1.7 Possible Threats to the Identification

In this section, I test the possibility that the minimum wage might be endogenous. It is possible to think that policymakers decided to increase the minimum wage when the economy was growing and employment levels were rising. Ideally, this issue can be solved using an instrument variable, but I do not have one. Instead, I can test if the policies were determined endogenously using a specification with one-year lag and one-year lead. I do not expect any significant effect on the leads unless there are preexisting trends that can indicate that the policymaker increased the minimum wage because of employment changes. As far as lags go, in some cases, it would be expected for there to be a persistent effect (Neumark and Wascher (1992 and 1994a)); thus, lags can be significant without threatening the validity of the results.

I present in Table 1.7 the validity test using equation (1), i.e., estimate separately by groups of enforcement. Table 1.7 is divided into two parts: one has only two-way fixed effects as controls, and the other has country-specific trends. For both parts, in columns (1) to (4), I use only the sample of countries with no enforcement, columns (5) to (8) weak enforcement, and (9) to (12) strong enforcement. In the first part, without specific trends, the estimator for minimum wage is significant for the lags and leads for the group of unskilled workers, and it is negative significant for the leads for the total workers in a sample of countries with no enforcement. No significant leads suggest that the minimum wage policy is not endogenous, and no significant lags indicate that the policy's effect is not persistent on employment over time. This implies that the minimum wage could be endogenous for unskilled workers in countries with no enforcement; in other words, minimum wage increases are approved when the employment is growing. In other words, there is an existing pre-trend that confounds the estimations for countries with strong enforcement. As in Allegretto et al. (2011), a solution is to control for country-specific trends, as I do in the next part of the table. In the countries with weak enforcement, there are no evidence of endogeneity, but the minimum wage has a positive and significant effect on the lags of total, unskilled, and young adult workers. For countries with strong enforcement, the minimum wage has no effects on any group of workers in levels, lags or leads.

Results change when I control for country-specific trends in the second part. The specific trends correct the issue for the total workers but not for the unskilled ones, which means that the results on unskilled workers are unreliable. In countries with strong enforcement, minimum wage policy has contemporary negative effects on female workers and a significant negative lag, which implies that the effect after one year of implementation is higher than the immediate effect. None of the rest of lags and leads are significant (except for unskilled workers), which is evidence that using country-specific, at least, trends reduce the endogeneity problem.

In conclusion, the validation test points out that it is unlikely that endogeneity or pre-trends bias the results when I control for country-specific trends (i.e., leads are not significant). Finally, the minimum wage effects are stronger and significant for female workers.

Table 1.7 Identification Test: Effect of the Kaitz Index (lags and leads) on Log of Total, Unskilled, Young Adult, and Female Employment Rate—Countries Grouped by Different Degree of Enforcement

Two-way fixed effects, no trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	No Enforcement				Weak Enforcement				Strong Enforcement			
Dependent Variable: Emp Rate	Total	Unskilled	Young Adults	Female	Total	Unskilled	Young Adults	Female	Total	Unskilled	Young Adults	Female
Kaitz Index	-0.190**	-0.249**	-0.233	-0.154*	0.0244	0.0505	0.0340	0.0545	-0.0708	-0.0466	0.0180	0.0696
	(0.0870)	(0.0960)	(0.179)	(0.0791)	(0.0212)	(0.0390)	(0.0551)	(0.0406)	(0.0484)	(0.0579)	(0.150)	(0.136)
Kaitz Index (t-1)	0.0276	-0.0656	-0.258	-0.0212	0.0558**	0.0758***	0.204***	0.0238	0.0468	0.0138	0.0663	0.0727
	(0.0877)	(0.102)	(0.184)	(0.121)	(0.0233)	(0.0267)	(0.0643)	(0.0433)	(0.0448)	(0.0372)	(0.142)	(0.126)
Kaitz Index (t+1)	0.133*	0.281**	0.0382	0.112	-0.00526	-0.00171	-0.0322	-0.0377	0.0220	-0.0226	0.122	0.205
	(0.0762)	(0.125)	(0.181)	(0.142)	(0.0200)	(0.0197)	(0.0528)	(0.0500)	(0.0673)	(0.0798)	(0.120)	(0.122)
Observations	206	206	206	206	338	338	338	338	123	123	123	123
R-squared	0.991	0.992	0.988	0.996	0.993	0.992	0.989	0.997	0.998	0.998	0.996	0.998
Number of Countries	21	21	21	21	29	29	29	29	12	12	12	12
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Specific Trend	No	No	No	No	No	No	No	No	No	No	No	No

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7 (continuation) Identification Test: Effect of the Kaitz Index (lags and leads) on Log of Total, Unskilled, Young Adult, and Female Employment Rate —Countries Grouped by Different Degree of Enforcement

Two-way fixed effects, country-specific trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	No Enforcement				Weak Enforcement				Strong Enforcement			
Dependent Variable: Emp Rate	Total	Unskilled	Young Adults	Female	Total	Unskilled	Young Adults	Female	Total	Unskilled	Young Adults	Female
Ln MW	0.0504	-0.0258	0.189	0.137	0.0242	0.0493	0.00889	0.0420	-0.0797	-0.100	-0.0793	-0.199***
	(0.0728)	(0.0886)	(0.173)	(0.117)	(0.0225)	(0.0298)	(0.0551)	(0.0362)	(0.0495)	(0.0597)	(0.0860)	(0.0618)
Ln MW(t-1)	-0.0826	-0.209**	-0.187	-0.0904	0.0375	0.0587*	0.0827	0.0666	-0.0356	-0.0282	-0.123**	-0.109*
	(0.0536)	(0.0933)	(0.169)	(0.0777)	(0.0270)	(0.0316)	(0.0606)	(0.0466)	(0.0356)	(0.0485)	(0.0511)	(0.0527)
Ln MW(t+1)	0.0344	0.121**	-0.0264	0.148	0.0210	0.0517***	0.0254	0.0171	-0.0139	-0.00249	-0.000903	-0.0252
	(0.0557)	(0.0487)	(0.0945)	(0.0954)	(0.0252)	(0.0181)	(0.0618)	(0.0408)	(0.0484)	(0.0465)	(0.0776)	(0.0631)
Observations	206	206	206	206	338	338	338	338	123	123	123	123
R-squared	0.984	0.985	0.978	0.993	0.986	0.981	0.977	0.993	0.995	0.994	0.990	0.996
Number of Countries	21	21	21	21	29	29	29	29	12	12	12	12
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment rate is in logs. The panel is unbalanced, and it includes 62 countries (grouped in different samples), table with a smaller number of countries because some countries are only present in two years that are lost with the lags and leads. Period 1996–2014, and 11.3 years on average. The incremental R^2 for country-specific trends is explaining around 0.08 for total employment, 0.10 for unskilled employment, between 0.12 to 0.44 for teenagers, and 0.22 for female workers. No trends and lineal country-specific trends are used. Unskilled employment is calculating using low and medium skill classifications (which include painters, carpenters, blue-collar workers, among others), and young adults are workers between 15 and 24 years old. Controls for total and unskilled workers include log of population, log of the relative size of youth to the population for young adult workers, and log of the relative size of the female to the population for female workers. Log of the GDP and log of one lag of the GDP are included for all specifications.

1.8 Robustness Test: Quality of Enforcement

In this section, I analyze the quality of enforcement. As mentioned before, one concern is that the presence of a penalty in the law does not necessarily mean that the country is enforcing the law. One possible way to alleviate this concern is to use variables that are proxies to the quality of the enforcement in each country.

The World Bank Governance Office constructs indicators that measure the quality of institutions in time for several countries. The indicators are measurements of the rule of law's quality, government efficiency, regulatory quality, and control of corruption. A more detailed explanation of the governance indicators is given in Appendix II. I decide to use regulatory quality over the rest of the indicators because it makes more sense. This indicator is closely related to the quality of minimum wage laws and the associated penalties. The rest of the indicators measure the quality of institutions, but they are unrelated to implementing sound policies and regulations.

In Table 1.8, I present triple interactions of the regulatory quality, minimum wages, and the degree of enforcement. The objective of these specifications is to analyze if the regulatory quality matters. In columns (1) to (4), I analyze the interaction for total, unskilled, young adults and female employment. To make the analysis more straightforward to understand, I omitted the weak enforcement dummy, which means that the strong enforcement dummy is compared with countries with no enforcement and weak enforcement. Results do not change if I include the weak enforcement dummy. Also, the regulatory quality variable is a deviation from the mean, so I can directly calculate the minimum wage elasticity as the effect on employment.

The triple interactions measure if the difference of the countries with strong enforcement and better quality of regulations have significant effects with respect to countries with strong enforcement but lower quality regulation, countries with better quality regulations but a lack of strong enforcement, and countries with neither strong enforcement nor better quality regulations.

The baseline (elasticity of the Kaitz Index) and the interaction of strong enforcement and minimum wage are no significant; however, the triple interaction is negative and significantly different from zero for all employment types (elasticities between -0.003 to -0.006 for the more affected group of workers). The implication is that the effects of minimum wages on employment are significantly negative in countries with strong enforcement and better regulatory quality. The results are very sharp: the effect of an increase of the minimum wage on employment in countries with strong enforcement and good institutions is negative and significant; moreover, the effect is stronger on vulnerable workers.

Table 1.8 Kaitz Index-Interactions with Enforcement and Regulatory Quality. Effects on Log of Total, Unskilled, Young Adult, and Female Employment Rate

VARIABLES	(1) Total Employment	(2) Unskilled Employment	(3) Young Adults Employment	(4) Female Employment
Coefficients				
Kaitz Index	-0.0177	0.0113	-0.0485	0.00861
	(0.0347)	(0.0431)	(0.0563)	(0.0671)
Kaitz Index x Strong Enforcement	-0.0444	-0.0604	-0.0263	-0.146*
	(0.0442)	(0.0480)	(0.0747)	(0.0839)
Kaitz Index x Regulation Quality	0.0726**	0.0595	0.123***	0.103
	(0.0295)	(0.0456)	(0.0402)	(0.0641)
Strong Enforcement x RQ	0.0365	0.0112	0.0236	0.0456
	(0.0278)	(0.0460)	(0.0571)	(0.0461)
Kaitz Index x Strong Enforcement x RQ	-0.147***	-0.144**	-0.203**	-0.275***
	(0.0496)	(0.0711)	(0.0959)	(0.0993)
Regulation Quality	-0.0104	0.00480	0.00417	0.000394
	(0.0136)	(0.0289)	(0.0203)	(0.0283)
Elasticities				
Kaitz Index	-0.00686	0.00438	-0.0189	0.00335
	(0.0135)	(0.0167)	(0.0219)	(0.0261)
Kaitz Index x Strong Enforcement	-0.00378	-0.00515	-0.00224	-0.0124*
	(0.00377)	(0.00409)	(0.00637)	(0.00715)
Kaitz Index x Regulation Quality	0.0123	0.0101	0.0208	0.0174
	(0.005)	(0.00774)	(0.00683)	(0.0109)
Strong Enforcement x RQ	0.00175***	0.000536**	0.00113**	0.00218***
	(0.00133)	(0.0022)	(0.00273)	(0.0022)
Kaitz Index x Strong Enforcement x RQ	-0.00296***	-0.00292**	-0.00411**	-0.00556***
	(0.001)	(0.00144)	(0.00194)	(0.00201)
Observations	747	747	747	747
R-squared	0.955	0.920	0.813	0.924
Number of Countries	80	80	80	80
Time FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Country-Specific Trend	Yes	Yes	Yes	Yes

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment rate is in logs. The panel is unbalanced, and it includes 80 countries, period 1996–2015, and 9.3 years on average. There is less information for institutional indicators. The incremental R^2 for country-specific trends is explaining around 0.07 for total employment, 0.10 for unskilled employment, 0.46 for teenagers, and 0.31 for female workers. Weak and strong enforcement is measured with a dummy variable equal to 1 if the country has weak or strong enforcement (high penalties). The enforcement dummies are fixed in time. Dummies not interacted are dropped because they are colinear to country fixed effects. The omitted dummy is "no enforcement." No trends and cubic country-specific trends. Unskilled employment is calculating using low and medium skill classifications (which include painters, carpenter, blue-collar workers among others), young adults are workers between 15 and 24 years old. Controls for total and unskilled workers includes log of population, and log of the relative size of youth to the population for young adult workers, and log of the relative size of the female to the population for female workers. See Online Appendix to see more information the regulatory quality indicator.

1.9 Conclusions

This paper contributes to the literature by explaining the mixed results found in the literature on minimum wage in developing countries. I have shown that the minimum wage only has negative effects on employment in countries with strong enforcement, and the difference with respect to the baseline countries is significant and negative. The effects are stronger for vulnerable groups (i.e., female workers and young adult workers), and they are more robust for female workers. In particular, unskilled female workers are more negatively affected by increases in the minimum wage in countries with strong enforcement. The results are robust to including controls for quality of enforcement and pre-trends, the validation test points out that it is unlikely that endogeneity or pre-trends bias the results when I control for country-specific trends implies that my results using country-specific trends are robust and reliable. Finally, the effects are sharper if I interact the enforcement with the quality of the institutions, which means that the enforcement's quality is also critical.

Some limitations of this paper are that identified effects are broad averages of very heterogeneous countries: while I control for country-specific variation, there might still be some unknown variation among countries that can affect the results. In addition, minimum wages effects are also affected by many other factors. For instance, the labor market concentration and, as it is shown in Munguia (Forthcoming), the informal sector (i.e., jobs that are not reported in many countries), and the degree of binding of the minimum wage are important factors that explain differences in minimum wage effects among countries. Future research can identify the effects of the minimum wage on labor markets with different degrees of competition.

Finally, this paper has policy implications. The minimum wage has positive effects on average wages in general, which implies that policymakers can use minimum wage if they want to increase average workers' income. However, minimum wages also have adverse effects depending on the penalties in each country and its institutions. Increasing the minimum wage negatively impacts employment, and the effect is stronger on female workers; therefore, other policies must accompany minimum wage policies to reduce negative effects that can lead to more gender inequality in the labor markets.

2. Do Minimum Wages Reduce Employment in Developing Countries? A Survey and Exploration of Conflicting Evidence

2.1 Introduction

Minimum wages have been a controversial subject among policymakers and economists in the United States and many other countries.[18] The evidence on employment effects in developing countries is quite mixed. In the studies we survey in this paper, simple averaging of all of the reported estimates yields a fairly modest negative employment elasticity of −0.061 and averaging the authors' preferred estimates from each study yields an elasticity of −0.102. However, looking across all the studies reveals considerable heterogeneity, with many negative estimates (some substantially larger in absolute value than these averages) but also many (although fewer) positive estimates.

The goal of our analysis is to examine the evidence from the large set of studies we survey, to try to understand this heterogeneous evidence and what we can learn from it. Is there simply no consistent evidence of negative employment effects of minimum wages in developing countries? That is, do we get heterogeneous effects – with both positive and negative estimates – even when studies are similar in looking at workers likely to be affected by minimum wages both because of their skills and because of the nature of a country's minimum wage law? Or, instead, is the heterogeneity in estimated minimum wage effects more systematic, with negative effects where we would expect them – e.g., for vulnerable low-skill workers where minimum wage laws are strong and binding – but not for higher-skill workers or where minimum wages laws are weaker and/or less binding?

We pursue these questions by conducting a version of a meta-analysis of a large set of studies of minimum wage effects in developing countries. Our focus is on understanding the differences in estimated employment effects across studies, which contrasts with the more common foci of meta-analyses on arriving at a single estimate from a body of studies and on publication bias (e.g., Belman and Wolfson, 2019; Broecke et al., 2016). Our analysis also contrasts with general surveys of the evidence for developing countries (Belman and Wolfson, 2016; Bhorat et al., 2017). Still, there are clearly complementarities between our evidence and the evidence in these other surveys or meta-analyses.

It is important to consider how to interpret our evidence. There are three important points. First, stronger and more consistent evidence of adverse employment effects under conditions where

[18] For a recent review of the U.S. evidence, including discussion of the conflicting evidence and which methods point to disemployment effects, see Neumark (2019).

we would expect adverse effects – e.g., for less-skilled workers, when minimum wages are binding or strongly enforced, or in the formal sector – would not negate the fact that estimated employment effects in developing countries vary. But such evidence would be informative about the institutional settings and contexts in which minimum wages reduce employment – such as when they are imposed in the formal sector and are strongly enforced.

Second, such evidence could indicate that minimum wages have more adverse consequences when they have the greatest potential benefits – i.e., for low-skilled workers for whom they are effective at raising wages. Of course, evidence that minimum wages reduce employment of lower-skilled workers does not imply that minimum wages are the wrong policy choice. However, such evidence would imply that minimum wages in developing countries reflect more of a tradeoff between higher wages and lower employment than what one might conclude from a simple overview of the heterogeneous evidence. Ultimately, we think the wisdom of higher wages in developing countries should hinge more on whether they help raise incomes of low-income families.[19]

And third, this kind of evidence may speak to the right model of the labor market to use in thinking about labor market policy and other questions in developing countries. If evidence on employment effects of minimum wages is inconsistent for studies of less-skilled workers, in the formal sector, when minimum wages are binding and enforced, then it is possible that the monopsony model may better explain the evidence than the competitive model.[20] In contrast, consistent evidence of disemployment effects in studies meeting these criteria, despite less consistent evidence in studies where negative employment effects are less likely to arise, would bolster the competitive characterization of labor markets (although it could still be possible to reconcile such evidence with monopsony).

We conclude that one can draw firmer conclusions about the employment effects of minimum wages in developing countries than first meets the eye when simply looking at all the estimates. We find that the estimated employment effects of minimum wages in developing countries are more likely to be negative, and larger negative, when estimates focus on data and sectors for which the competitive model predicts disemployment effects, and in institutional settings in which we would expect the minimum wage to have more adverse impact. Specifically, there is more consistent evidence of negative employment effects when the minimum wage is binding, where minimum wage enforcement

[19] For evidence on this question from over a decade ago, see Neumark et al. (2006) and Cunningham (2007).

[20] Still, the monopsony model makes more direct predictions than simply that employment effects are heterogeneous, and it would be important to test these predictions. For related work for the United States, see Azar et al. (2009) and Munguía Corella (forthcoming).

is stronger, for estimates of effects in the formal sector, and when the data focus on more vulnerable (lower-wage) workers.

One dimension we do not explore is whether monopsony power is sometimes relevant. We do find that positive estimates are more prevalent in studies with only one feature or no features for which the competitive model and institutional factors predict negative effects. Monopsony is a potential explanation, but not the only one; for example, the standard two-sector competitive model predicts positive employment effects in the informal sector.

2.2 Meta-Analysis in the Context of Minimum Wages Studies

Meta-analysis developed as a method of combining results from existing studies, to derive conclusions from a body of research on a particular question or effect. In medicine, for example, early meta-analyses studied the evidence from randomized, controlled clinical trials, addressing the problem that individual medical studies sometimes lack enough observations to reach reliable statistical conclusions about the effect of the treatment studied. A meta-analysis pooling the evidence across studies can yield a more precise estimate of the impact of treatment, or other outcomes, than individual studies contributing to the pooled analysis. The technique was also applied, in the middle of the last century, to research in agricultural science, psychology, education, and sociology, although the term "meta-analysis" was apparently coined by Gene Glass in 1976, who described it as "analysis of analyses." (See the history of meta-analysis described Hunt, 1997).

Meta-analyses in economics also pool results, often in meta-regressions used to estimate an average effect (or treatment) size, and sometimes to estimate the impact of study features on the estimated effect size. Economists have been quite concerned with "publication bias," which considers the possibility that some results are not published because of editors' and reviewers' (and perhaps authors') prior views, or because of diminished interest in statistically insignificant results, either of which can lead to bias in average estimates based on published work. Economists using meta-analysis also consider some of the more conventional problems that can arise in regression models – such as heteroskedasticity resulting from variation in precision of estimates across different studies owing to sample sizes or empirical strategies.

These same questions carry over to meta-analysis in the minimum wage literature. These analyses have pooled results to obtain estimates of the effect size – typically the magnitude of the elasticity of employment to the minimum wage. They have also been used to test for publication bias, and to try to interpret and systematize results that vary across studies done using different techniques, different data, or estimating effects for different groups. Our work in this paper is most closely related to meta-analyses that try to identify what features of studies explain heterogeneity of the estimated effects.[21] For instance, Card and Krueger (1995) conduct a meta-analysis of time-series studies of the effect of the minimum wage on teen employment in the United States, and conclude that it is very likely that the results are affected by publication bias, induced by editors' and authors' tendencies to look for negative and significant estimates of the employment effects of the minimum wage, a conclusion shared by Doucouliagos and Stanley (2009). who also conclude that there is little or no evidence of a negative effect of the minimum wage once one corrects for publication bias. In contrast, Neumark and Wascher (2007) find that the results of published time-series studies of minimum wage effects are consistent with structural change and that the evidence does not reject the null hypothesis of no publication bias. The most recent meta-analysis of the minimum wage literature (Belman and Wolfson, 2019), based on newer studies that tend to use panel data with sub-national minimum wage variation, finds little effect of publication bias and more evidence of minimum wage-employment elasticities for teens and other low-skill groups of around −0.1. And a smaller meta-analysis of studies of emerging economies (Broecke et al., 2017) finds more evidence of publication bias.

Our analysis is, in a sense, a meta-analysis, in that it is, to quote Glass, and "analysis of analyses." In particular, we conduct a version of a meta-analysis of a large set of studies of minimum wage effects in developing countries. However, in contrast to the main focus of meta-analyses of the minimum wage on questions like publication bias, or arriving at a single estimates from a body of studies (e.g., Belman and Wolfson, 2019; and Broecke et al., 2016), and also in contrast to general surveys of the evidence (e.g., Belman and Wolfson, 2016; Bhorat et al., 2017), our focus is explicitly on understanding the differences in estimated employment effects across studies.[22] Some meta-analyses do study sources of variation in estimated effects. In particular, Belman and Wolfson (2016) is a broad survey of the effects of minimum wages on many different outcomes, and does not – in contrast to the present paper – focus on reconciling conflicting evidence, but more on issues of empirical methods. Broecke et al. (2017) use a meta-analysis to analyze 14 emerging economies, and

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[21] See Wolfson and Belman (2019) for more discussion about different types and uses of meta-analysis in general, and in the minimum wage literature.

[22] See Neumark (2016) for discussion of some of these meta-analyses of estimated minimum wage effects in the United States, especially with reference to testing for publication bias. In a nutshell, it is hard to distinguish between publication bias and other sources of patterns in the published evidence consistent with publication bias. For example, meta-analyses like Doucouliagos and Stanley (2009) argue that if published negative estimates of minimum wage effects have larger standard errors, this is evidence of publication bias. However, the same phenomenon can arise if studies using better research designs lead to "truer" (i.e., less biased) estimates, which happen to be negative, and which have larger standard errors because they demand more of the data.

present some evidence on differences for vulnerable workers and the formal sector. But both studies, as well as Doucouliagos and Stanley (2009), focus in large part on estimating the overall effect of the minimum wage on employment, and on publication bias.

We have some criticisms of using meta-analysis to study these questions with regard to the minimum wage literature (see Neumark, 2016). First, it is very hard to distinguish between publication bias and other sources of patterns in the published evidence consistent with publication bias. For example, meta-analyses like Doucouliagos and Stanley's argue that if published negative estimates of minimum wage effects have larger standard errors, this is evidence of publication bias. However, the same phenomenon can arise if studies using better research designs lead to "truer" estimates, which happen to be negative, but also have larger standard errors because the research designs demand more of the data. Second, averaging across estimates from studies of minimum wage effects, as meta-analyses do, is problematic. The populations studied vary, and this and other factors can influence how binding the minimum wage is, generating variation in estimated effects that there is no reason to simply average. For example, Neumark and Wascher (2007) show that studies more sharply focused on workers most likely to be affected by minimum wage increases reveal clearer evidence of disemployment effects. Among other factors potentially influencing the magnitude of the effect is of course how binding the minimum wage is, which may not be captured well in a standard regression framework (Neumark and Wascher, 2002). In short, the meta-analysis "paradigm" for combining estimates from many similar studies – say, randomized trials of a drug (Hunt, 1997) – carries over poorly to the minimum wage literature (and likely many other literatures in economics), although it can still be useful in identifying features of studies that lead to differences in estimates.

Because of these issues, our analysis does not follow the usual meta-regression approach of estimating average effects and testing for publication bias, although we do estimate some meta-regressions to help interpret our data by estimating relevant conditional differences across studies. Rather, we summarize, in a variety of ways, how estimated minimum wage effects vary based on features of the studies we examine. In particular, we summarize how the results vary with the inclusion of different study features among those that more strongly predict negative employment effects, and different combinations of them. We do not embed this analysis in a single meta-regression capturing all combinations of study features, because we are simultaneously considering study features that can exist in very many different combinations. But by considering the evidence on different combinations of study features, we go beyond the usual meta-regressions used in the minimum wage literature, which do not study combinations of study features at all (Belman and Wolfson, 2019; Broeke et al., 2017). Moreover, in terms of the substantive question we ask, our focus on differences across developing-country studies in features for which the competitive model and institutional factors are more likely (or not) to predict negative employment effects is, to the best of our knowledge, unique.

2.3 Studies Included

We reviewed 61 papers on the employment effects of minimum wages in developing countries – all of the papers we identified that met our study criteria. To select these papers, we searched for papers in journals and on Google Scholar, covering all the regions in the developing world.[23] We searched using keywords related to minimum wages and developing countries. Our search was conducted from April 2017 to August 2017. We also consulted recent surveys (Belman and Wolfson, 2016; Broecke et al., 2017) to check for any papers we missed, which resulted in adding two additional papers from Belman and Wolfson (2016).[24] We focused mainly on recently published papers (published since 2000), because we wanted to analyze the burgeoning wave of minimum wage papers in developing countries; of the 61 in our survey, 93.4% were published after 2000.[25] Most of the papers are in English, but we also include papers in Spanish and Portuguese. We also restricted the analysis to papers that report employment elasticities with respect to the minimum wage, or for which we had enough information to compute these elasticities.[26]

We created a data set of all estimates from these papers, as well as information on the statistical significance of the estimates. However, because many papers present estimates that the authors do not view as credible (e.g., showing the estimates for panel data specification without the fixed effects), we also tried to extract the authors' main or preferred estimates from each study. Specifically, we read each paper in detail and selected preferred estimates following three rules. First, in some cases the authors specifically say that a subset of estimates are their preferred results. This kind of statement is based, for example, on the authors presenting specifications missing some controls (e.g., year fixed effects), while arguing that the controls are needed to correctly estimate the effects of minimum wages.

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[23] We define developing countries as those that the World Bank does not classify as a high-income country. Poland became a high-income country in 2009, but the data in the papers on Poland cover predominantly earlier data (1999 to 2011 in all papers except one that extends to 2013).

[24] We also added one paper published in this journal (Ham, 2018) that did not appear in our search but was identified by a reviewer.

[25] The only exceptions were four earlier, often-cited papers that appear in more than one meta-analysis: Bell (1997) for Mexico; Castillo-Freeman and Freeman (1992) for Puerto Rico; and Feliciano (1998) and Foguel (1998) for Brazil. Appendix Figure A1 shows the distribution of these studies by year (publication date). The figure shows that the plurality of these studies were published in this decade and most in the last two decades. Of course papers studying minimum wages in developing countries continue to be produced and published (e.g., Asmal et al. (2019), but we had to cut off the sample period for analysis for this version of our paper.

[26] Below, we discuss the studies we include and the elasticity calculations in more detail, and Appendix Table A4 lists all the studies and the elasticities.

Second, in the absence of such an explicit statement, authors often summarize what they say are their main findings, underscoring some specific estimates by referring only to these estimates in the abstract, the introduction, or the conclusion. Third, absent either of these conditions, if estimates are reported for many regions in a country, we select the estimate for the whole country as the preferred result. In applying these rules for selecting preferred estimates, rule one overrode rules two and three, and rule two overrode rule three. Thus, for instance, if the authors point out that their preferred result is for region A, we use region A as the main result instead of the estimate of the whole country. However, in the spirit of a meta-analysis, we do not impose (or even offer) our subjective assessments of which studies are more credible, and do not discard studies or estimates that could plausibly be viewed as less credible or plausible.[27]

Finally, it is important to mention that studies sometimes report estimates for different groups or sectors, like all workers and more vulnerable workers, or the formal and the informal sector. We capture all of these estimates, but also flag – when the authors do – the subset of these estimates preferred by the authors, based on the rules above.

We believe that in analyzing the set of estimates from a research literature, it makes sense to focus on the preferred estimates. For example, suppose there are two papers estimating the effect of policy X, and both authors believe that one needs to instrument for policy X to get the causal effect. If one paper presents only the instrumental variables (IV) estimate, while the other presents both the OLS and the IV estimate, then why give weight to the OLS estimate in summarizing the evidence? Neither author believes the OLS effect is of interest, and the second author chose to include it for some other reason – perhaps to confirm the expected direction of the bias in the OLS estimate, for which the IV corrects.[28] At the same time, we understand that the selection of preferred estimates potentially allows for an element of subjectivity compared to simply capturing all estimates in the surveyed papers; our use of a set of rules for identifying authors' preferred estimates is intended to mitigate any concerns regarding our decisions about which estimates to study.

Across the 61 studies, there are 1,250 total estimates. There are 15 studies that report the effects of the minimum wage on the probability of being employed (or something closely related),

[27] We are not arguing that this is necessarily the preferred approach for interpreting a broad literature. Indeed, in the U.S. context, Neumark and Wascher (2007) offer reasons why a narrative review (with some emphasis on what appear to be more credible estimates) may be preferred. On the other hand, they also argue that a narrative review may be more effective at highlighting some of the reasons for differences across studies attributable to the groups studied or other theoretical predictions. The present paper adopts the latter perspective to some extent – focusing on explaining differences in results across studies, albeit without discarding estimates.
[28] An example of the latter is Mayneris et al. (2014), who report both OLS and IV estimates, but take a clear stance that there may be endogeneity bias in their approach that requires instrumenting for the minimum wage variable.

rather than an elasticity, but for which we could recover estimates of elasticities. We compute these elasticities using reported means of employment rates and the minimum wage if the paper reported them; if these were not reported, we used alternative data sources to obtain these averages and estimate the elasticities. The data sources and calculations for this subset of studies are described in Appendix Table A3.[29]

Table 2.1 reports descriptive information on the estimated minimum wage-employment elasticities in the studies we surveyed. Across all the estimates in the surveyed studies, the average estimated elasticity is −0.061, the maximum elasticity is 4.51, and the minimum is −4.73. We identified 229 preferred estimates, using the rules discussed above. The average elasticity for this subset of estimates is −0.102, with a maximum of 2.19 and a minimum of −2.53. The standard deviation is 0.497, very similar to the standard deviation for all estimates (0.451). Note that the authors' preferred estimates exclude some more extreme elasticity estimates.

Table 2.1 Summary of Estimated Elasticities from Surveyed Studies and Authors' Preferred Estimates

	Mean	Median	Minimum	Maximum	Standard dev.	Obs.	Skewness	Kurtosis
All estimates	-0.061	-0.012	-4.73	4.51	0.451	1,250	-1.77	39.35
Authors' preferred estimates	-0.102	-0.048	-2.53	2.19	0.497	229	-0.04	13.37

Figure 2.1 provides histograms for the two sets of estimates, to provide more evidence on their distributions. We plot only estimates between −1 to 1 to make the figure easier to read.[30] Panel A provides the histogram for the full set of estimates, and Panel B for the preferred estimates. The negative means and medians of the estimates are clear for both sets of estimates, as is the fact that there clearly are positive estimates. Note also that the medians are closer to zero.

2.4 Classifying Studies/Estimates, and Predictions for Employment Effects

The key question we assess is whether there are systematic differences across studies and estimates that explain the variation in estimated employment effects. In particular, we classify the estimates in the studies in our survey by specific features of the estimates. We then ask whether features of

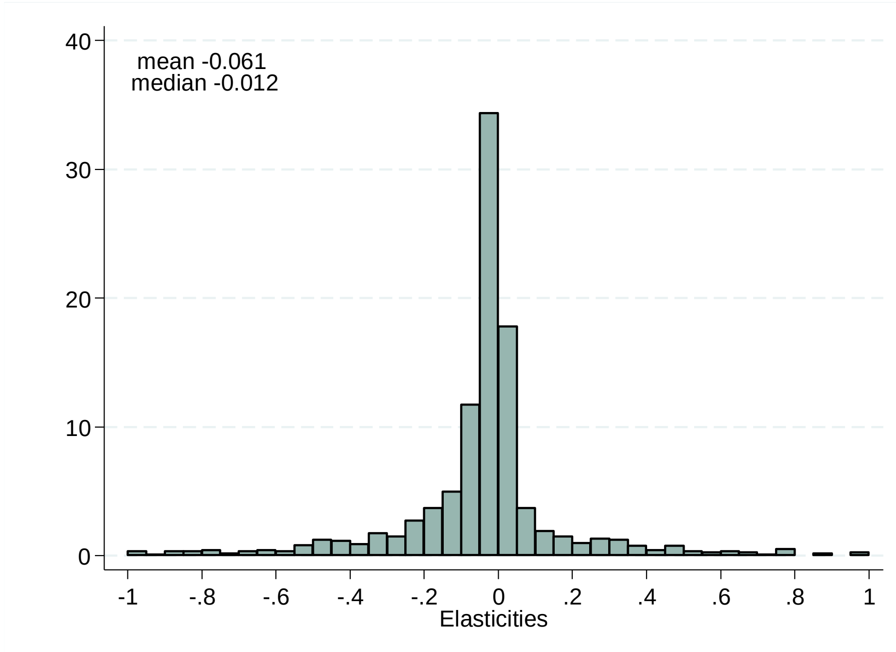
[29] For studies for which we had to compute elasticities, we use the statistical significance of the reported employment effect.
[30] We do not do this trimming in any of the figures or estimates that follow, where we use all the elasticities preferred by the authors, even if the elasticity appears to be an extreme value.

estimates more likely to predict negative effects either based on economic theory – specifically, the competitive model of the labor market – or because of institutional factors, in fact do so. As an example, the competitive model of the labor market would predict that less-skilled workers are more adversely affected by a higher minimum wage. We classify estimates based on four features. Appendix Table A4 lists the studies we use, the preferred estimates as discussed earlier, and the classification of studies and estimates – which we now discuss in detail.

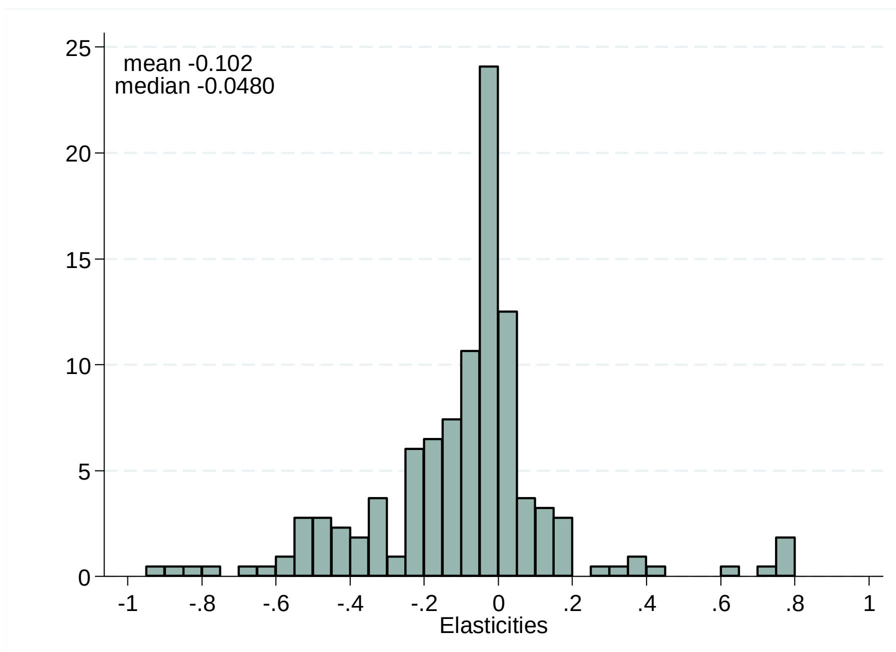
2.4.1 Binding minimum wages

The first feature we use to classify estimates is whether minimum wages are binding. There are different ways to measure the bindingness of minimum wages. One measure sometimes used in the minimum wage literature is a projected fraction affected, an estimate of the fraction of workers earning below the minimum wage before an increment. However, only 12 of 61 papers that we reviewed report this measurement. Instead, we use as a proxy for bindingness evidence of a positive effect of the minimum wage on average wages – evidence that is reported much more commonly (in 44 of the 61 papers). A potential limitation of this "binding" measure is that the effect on wages depends on the employment response. But we are less concerned with the precise magnitude than with whether the study provides evidence that wages of employed workers are increased, and we do not think that anyone's reading of the minimum wage literature is that the employment response to a minimum wage is ever so strong that it would obscure evidence of a positive effect of the minimum wage on wages. Indeed, this is consistent with the evidence we report below; most studies that test for an effect on wages find such an effect, consistent with binding minimum wages.

A. All estimates



B. Authors' Preferred Estimates



Note: We drop from the histograms (but include in the means and medians) the observations that are larger than 1 in absolute value to eliminate outliers and because most of the observations are between −1 and 1.

Figure 2.1 Histogram of Estimated Elasticities in Surveyed Studies and Authors' Preferred Elasticities

If the study reported a statistically significant positive effect of the minimum wage on wages, or evidence of a spike in the wage distribution at the minimum wage (based on visual inspection of figures as described by the authors), we classify the corresponding employment estimate as pertaining to a binding minimum wage. If evidence was reported and does not indicate a positive effect on wages, we classify the study as pertaining to a non-binding minimum wage. Our third category is "no data," meaning that the study did not report evidence on effects on wages; this third category is retained in our analysis, rather than dropping these observations.[31] We would expect more evidence of adverse effects of minimum wages on employment when minimum wages are binding, at least under the competitive model.[32]

2.4.2 Sector

The second feature we use to classify estimates is whether the estimate was for the formal sector, the informal sector, or both (total employment). In the formal (also called "covered") sector, minimum wage laws apply, in principle at least. Minimum wage laws do not cover the informal sector. The informal sector can be defined by firms that operate illegally, by self-employed workers and, as in Chun and Khor (2010) and Del Carpio et al. (2015), by small firms that enforcement authorities do not visit. In developing countries, both sectors can be sizable. The distinction between the effects for the formal and informal sector in developing countries is important. A high share of jobs is estimated to be informal: 46.8% of jobs in Latin America (ILO, 2015a), 66% in Sub-Saharan Africa (ILO, 2015b), and 68.2% in Asia-Pacific (ILO, 2018).

Some papers do not report if their estimates cover the formal sector, the informal sector, or both sectors. However, we were able to classify these papers by analyzing the data used. For example, for Mexico there are two main employment surveys – the Employment and Occupation Survey, and Social Security Administrative Data. The former has data on both sectors; hence, if the author uses total employment from this survey, we know that the estimates cover both sectors. The latter survey only has data for formal-sector workers, and thus we know that estimates using this survey are for the formal sector. All the estimates could be classified along this dimension.

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[31] There are no studies that report the fraction affected but not the evidence on bindingness that we use, and all of the studies that do report a positive fraction affected also show a positive effect on wages. Thus, we would not classify additional studies by using the fraction affected.

[32] It is possible that there is a "file-drawer" problem (e.g., Franco et al., 2014), such that studies that do not detect, in initial analyses, an effect of the minimum wage on wages of low-wage workers are not pursued further, because of the strong expectation that – whatever the effects on employment – minimum wages should push up wages at the bottom of the wage distribution. This may constrain our ability to garner evidence on how the employment effects of minimum wages estimated in different studies vary with whether or not the minimum wage is binding.

The prediction from the standard two-sector competitive labor market model is that a higher minimum wage reduces employment in the formal sector, because in the formal sector minimum laws (and other labor regulations) apply and are more likely to be enforced. However, employment in the informal sector may increase, depending on informal sector wages and the expected value of search for formal-sector work while employed vs. not employed in the informal sector (Harris and Todaro, 1970; Mincer, 1976). However, some recent work has highlighted the potential for different effects in the informal sector. For example, Gindling (2018) argues that some evidence points to wage increases in the informal sector from "lighthouse effects" that may arise because employers have to compete for workers with the formal sector, leading to minimum wages constraining the wages employers pay in the informal sector and hence reducing employment there.[33] Other studies, in contrast, have found no effect on wages in the informal sector (Papps, 2012; Carneiro and Corseuil, 2001). Thus, we should expect adverse employment effects of minimum wages in the formal sector – at least under the competitive model – whereas the prediction for the informal sector is perhaps less clear.

2.4.3 Enforcement

Our third feature of estimates is the degree of enforcement of the minimum wage law, which we break into three categories. Countries with no enforcement are those whose laws do not penalize violations of the minimum wage law. Countries with weak enforcement have low-cost fees for a violation. And countries with strong enforcement are those that specify severe penalties for not abiding by the law, like time in prison or shutdown of the company. The prediction, of course, is that minimum wages should have more impact generally, including reducing employment (according to the competitive model), when minimum laws are more strongly enforced. All the estimates are classified in one of these three categories.

The classification of enforcement is developed and described in Munguía Corella (2019) and the first chapter. He systematizes labor codes and minimum wage laws by country, and constructs an indicator for the degree of enforcement, using the ILO's "Database of National Labour, Social Security and Related Human Rights Legislation" (NALEX).[34] NALEX compiles records of labor laws for 196 countries and 160 territories. As an illustrative example, Ghana does not have any penalty specified in its Labor Act of 2003; the Act established a Tripartite Committee that oversees the

[33] Alternatively, lighthouse effects could reflect a reference price, a signal for bargaining, or the impact of fairness concerns – all influences on wages outside of the usual competitive model. See the discussion and related references in Boeri et al. (2011).
[34] See http://www.ilo.org/dyn/natlex/natlex4.home?p_lang=en.

minimum wage rate, but does not specify what happens when an establishment fails to abide by the law. Hence, Ghana is classified as having "no enforcement." In contrast, in Bolivia fines are costly (up to 1,447 USD per violation), and the authorities can shut down an establishment in case of repeated violations. Hence Bolivia is classified as having "strong enforcement." Given the constraints of the data used to classify enforcement, the degree of enforcement is assigned at the country level and does not change over time.

There are some potential challenges in the analysis classifying estimates based on enforcement. First, the enforcement measure captures potential penalties. It is possible that in some countries, even if the law is stringent, actual implementation is weak, owing to weak institutions in the country, a lack of labor inspectors, or corruption among the enforcement authorities. However, in a more standard panel data analysis of the effects of minimum wages in developing countries, Munguía Corella(2019) finds stronger adverse employment effects when the law dictates stronger enforcement, without regard to how well labor laws are enforced (although also finding that enforcement has stronger effects in countries with more effective labor market regulations, based on a World Bank index). Thus, the enforcement variable should provide some information about a country's commitment to its minimum wage laws. Second, the enforcement of the minimum wage could be endogenous. For instance, if, in some countries, the minimum wage is destroying low-skill employment, workers (or policymakers) might adopt weak enforcement "on the ground," despite what the law says, to mitigate the adverse effects, making it difficult to estimate the exogenous effect of enforcement.[35]

2.4.4 Vulnerability/low-skill

Finally, the fourth feature of estimates we use in our classification is whether the estimate is for low-skilled or "vulnerable" workers, or instead for all workers. We classify studies or estimates for vulnerable workers as those estimated for young adults, for women, or for unskilled workers. The competitive model of labor markets, of course, predicts that we should find stronger evidence of adverse employment effects of minimum wages in data on vulnerable workers because their wage is more likely to directly affected by the minimum wage. However, if the minimum wage is very low, it is possible that it is not binding even for low-wage, vulnerable workers. All the estimates are classified

[35] Clemens and Strain (2020) find evidence of this in the U.S. context, reporting that subminimum wage payments when minimum wages increase rise the most in states with relatively strong minimum wage enforcement. Because minimum wage violations in the United States are driven by worker complaints, they interpret this as workers "enforcing less" when the higher minimum wage is more likely to cost jobs.

as pertaining to either vulnerable workers or all workers.

2.5 Differences in Estimated Employment Effects: Evidence

We now turn to our analysis exploring how estimated employment effects vary with features of the estimates. In particular, we focus on whether the evidence is more consistent with negative employment effects for estimates based on one or multiple features that predict more adverse employment effects of minimum wages, and conversely whether there is less evidence of negative effects when these features are absent.

2.5.1 Differences in estimates: one-way comparisons

We begin, in Table 2.2, with univariate comparisons across estimates. Table 2.2 reports the number and percent of estimated employment elasticities with respect to the minimum wage that are negative and significant, insignificant, or positive and significant, for estimates with each of the four features by which we classify them: binding minimum wages, sector, enforcement, and type of workers; we classify estimates as significant based on a significance level of 5%.[36] This table is based on the authors' preferred estimates of the employment elasticity, summarized in the second row of Table 2.1 and in Panel B of Figure 2.1.

To better understand what is reported in Table 2.2, consider a specific example. Bhorat et al. (2014) analyze the effects on formal-sector wages and employment in South Africa. Their results indicate that the elasticity of wages with respect to the minimum wage is between 0.176 and 0.22 (statistically significant). Hence, these results are classified as "binding." For employment effects, they have two preferred elasticities (based on different econometric models); both are negative but only one is statistically significant. Hence, this study results in one negative and significant elasticity and one insignificant elasticity reported in the "Binding" row of Panel A in Table 2.2, and one negative and significant elasticity and one insignificant elasticity reported in the "Formal" row in Panel B. Because South Africa has weak penalties, this study is also coded as having one negative and significant elasticity and one insignificant elasticity in the "Weak" row in Panel C. And finally, these estimates cover all workers, rather than just vulnerable workers, and hence this study results in one negative and significant elasticity and one insignificant elasticity in the "All workers" row in Panel D. In the "Total"

[36] The conclusions were very similar using a 10% significance level, because very few estimates are significant at the 10% level but not the 5% level.

column, the rows in each panel add to the total number of estimates (229), because all the estimates are classified by each of the four features.

Table 2.2 One-Way Classification of Estimation Results by Features of Estimates, Authors' Preferred Estimates

	Negative and significant	Insignificant	Positive and significant	Total
A. Binding				
Binding	63 (38.2%)	91 (55.2%)	11 (6.7%)	165 (100.0%)
Not binding	3 (27.3%)	8 (72.7%)	0 (0.0%)	11 (100.0%)
No data	20 (37.7%)	23 (43.4%)	10 (18.9%)	53 (100.0%)
B. Sector				
Formal	53 (38.4%)	75 (54.3%)	10 (7.2%)	138 (100.0%)
Informal	16 (33.3%)	23 (47.9%)	9 (18.8%)	48 (100.0%)
Both	17 (39.5%)	24 (55.8%)	2 (4.7%)	43 (100.0%)
C. Enforcement				
Strong	29 (46.0%)	29 (46.0%)	5 (7.9%)	63 (100.0%)
Weak	28 (26.9%)	71 (68.3%)	5 (4.8%)	104 (100.0%)
No enforcement	29 (46.8%)	22 (35.5%)	11 (17.7%)	62 (100.0%)
D. Workers				
Vulnerable	37 (45.7%)	38 (46.9%)	6 (7.4%)	81 (100.0%)
All workers	49 (33.1%)	84 (56.8%)	15 (10.1%)	148 (100.0%)

Notes: Each cell reports the number of results and the row percent (in parentheses). Each category adds to the total of 229 preferred estimates. We classify results as significant if the p-value ≤ 0.05.

Panel A of Table 2.2 reports results based on whether the minimum wage is binding, non-binding, or there are no data on wages with which to classify the study and its estimates. There is somewhat more evidence of negative employment effects when minimum wages are binding (or are likely to be binding – as discussed below). For the estimates based on binding minimum wages, 38.2% of the elasticities (63 estimates) indicate negative and significant effects on employment. Only 6.7% of the results (11) with a binding minimum wage report positive and significant elasticities. In 55.2% of the cases (91) the estimated employment elasticity is insignificant. Thus, for binding minimum wages, if the estimated elasticity is significant, the evidence points much more strongly to adverse employment effects than to positive employment effects, although the share of negative and significant employment elasticities is lower than the share of insignificant elasticities.

There is only a small number of estimated elasticities from studies where the minimum wage is non-binding (11), and nearly three-quarters of them (72.7%) report an insignificant employment elasticity. However, the remainder (27.3%) of the estimated employment elasticities are negative and significant.

There is a sizable number of studies with no information on whether the minimum wage is binding (53 estimated elasticities). Among these, the results are very similar to the estimates based on studies reporting that the minimum wage is binding, with 37.7% of the estimated employment elasticities negative and significant, and 43.4% insignificant. Given the distribution of estimates (and studies) as having binding or non-binding minimum wages in the first two rows – with nearly all indicating that minimum wages are binding – it seems likely that in most of the unclassifiable studies the estimated minimum wage effect is in fact for a binding minimum wage. For instance, as shown in Appendix Table A5, China has four studies classified as "no data," but it has four that are classified as binding, and only one classified as non-binding, so it seems plausible that the minimum wage is binding in the first four. Similarly, Brazil has three studies classified as "no data," 12 classified as binding, and none classified as non-binding. Thus, it seems reasonable to view the results in the "No data" row of Table 2.2 as largely reinforcing the conclusion that estimates of the effects of binding minimum wages point to disemployment effects, although to be more agnostic we continue to treat these two groups of studies separately, and to study binding minimum wages we focus on the estimates for which we can explicitly classify the data as pointing to a minimum wage that is binding.

Panel B reports results for estimates classified by sector – formal or informal. The results tend to point to evidence of negative employment elasticities in both sectors. However, there is more evidence of positive effects for estimates based on the informal sector, and a little more evidence of negative effects for the formal sector. For the formal-sector estimates, 38.4% of the estimated elasticities (53 estimates) point to negative employment effects, while only 7.2% (10) point to positive employment effects; 54.3% of estimates (75) are insignificant. For the informal sector, the percentage of positive and significant employment elasticities is more than double that for the formal sector (18.8% vs. 7.2%), although still, more estimates are negative or insignificant (33.3% negative, and 47.9% insignificant). For estimates covering both sectors, the percentage of estimates that are negative and significant is similar, and the percentage of insignificant estimated elasticities is higher.

Panel C disaggregates the estimated elasticities based on enforcement. In this case, the results for strong vs. weak enforcement indicate more evidence of negative employment effects with strong enforcement, but the comparisons with no enforcement appear to be counterintuitive. In particular, the elasticities for minimum wage laws with strong enforcement are negative and significant in 46.0% of cases (29 estimates), compared to 29.6% (29) with weak enforcement; but the percentage is slightly higher (46.8%) with no enforcement. Thus, there is not a clear pattern of a greater percentage of insignificant elasticities the weaker is enforcement. These results may reflect some of the challenges we discussed earlier with respect to measuring enforcement and assessing its "effect" on the estimated minimum wage effect.

Finally, Panel D turns to results disaggregated by type of worker. Estimates for vulnerable workers point more clearly to disemployment effects – with 45.7% of such estimates (37) negative and significant, compared to a lower percentage (33.1%) for estimates computed instead for all workers.[37] Correspondingly, there is a lower percentage of estimates with positive effects when looking at vulnerable workers compared to all workers (7.4% vs. 10.1%), and the percentage with insignificant results is lower for vulnerable workers (46.9% vs 56.8%).

Thus, based on the univariate comparisons, for three of the four classifications of estimates we use – binding minimum wages, sector, and type of worker – we find some evidence consistent with minimum wages doing more to reduce employment where there is a stronger prediction of negative employment effects, and for the formal/informal-sector distinction, more evidence of positive effects in the informal sector. These results are consistent with expectations from the competitive model (while not necessarily contradicting other models), including the two-sector model. We next turn to evidence that more sharply delineates studies and estimates by simultaneously considering multiple features of these estimates.

2.5.2 Differences in estimates: multi-way comparisons

The one-way comparisons we have presented thus far could mask relationships between study features and estimated elasticities, for four reasons. First, we may not be isolating the effect of a particular features of an estimate, because estimates can vary along multiple dimensions at once. Second, given that each of the features we study – bindingness, formality, enforcement, and vulnerability – can matter independently for whether minimum wages reduce employment, it follows that estimated employment effects may be more negative if *more* features of an estimate predict negative effects – based on the competitive model or institutional factors (and more so if they interact). Third, we have taken no account of the estimated magnitudes of the elasticities. And fourth, related to the last point, the signs of insignificant estimates are also of interest.[38] Hence, we now present analyses that incorporate all of this information. For these analyses, we present evidence in sets of figures, rather than tables, because the figures make the evidence much clearer. In the next subsection, we turn to some regression

[37] This same pattern of variation is often observed within studies. In Appendix Table A4 see, for example, Baranowska-Rataj and Magda (2015), Feliciano (1998), and Maloney and Nuñez (2004).

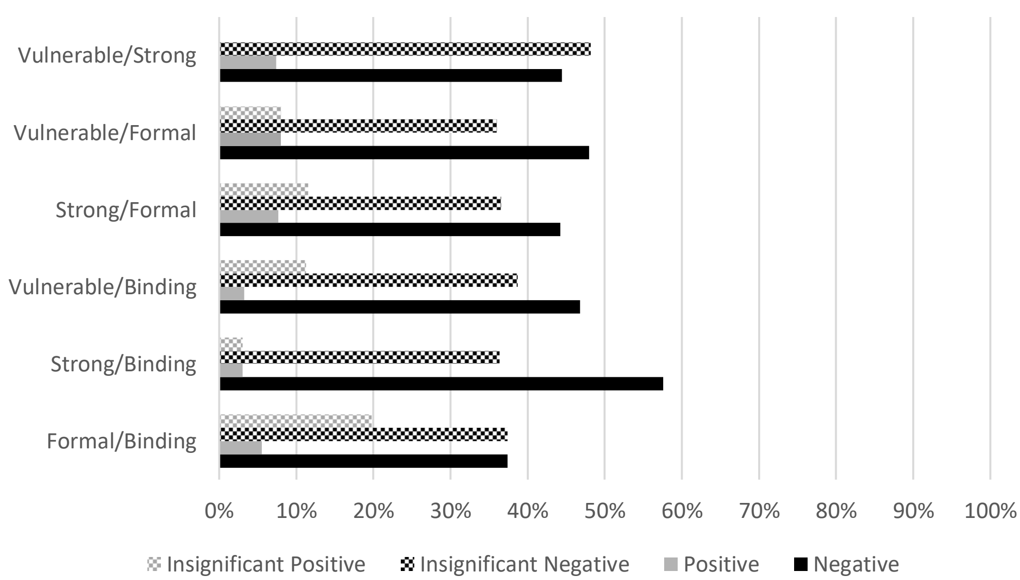
[38] However, we created a version of Table 2.2 in which we broke out the insignificant negative and the insignificant positive estimates. There was not much systematic difference across the different types of estimates; in other words, the differences associated with whether the estimate is negative *and significant* are more pronounced. (Results available upon request.) However, in the more-refined analyses that follow, we look at estimates distinguished in this way.

estimates that refine the analysis further.

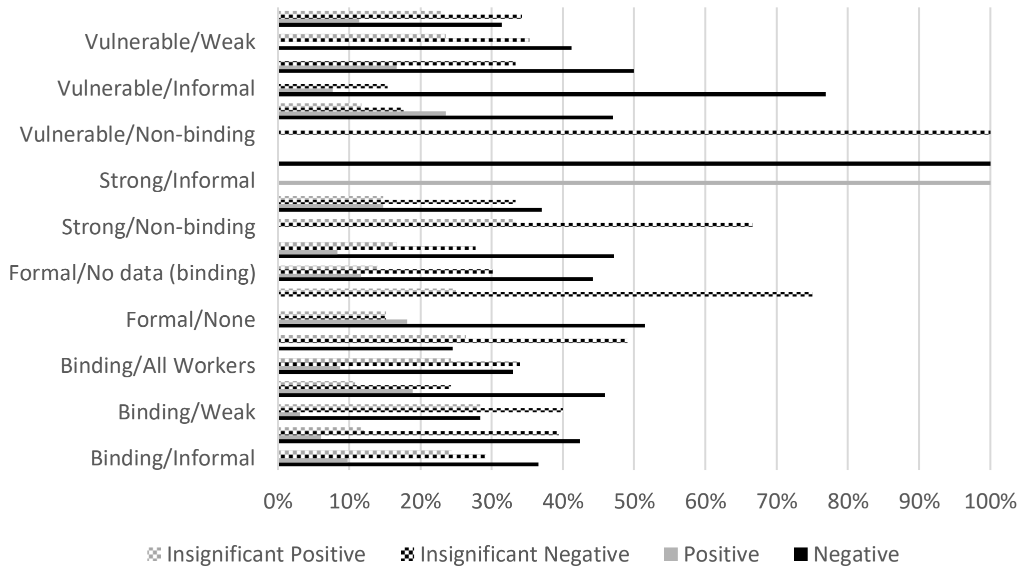
We begin with two-way comparisons based on pairs of features that more strongly predict negative employment effects based on the competitive model and institutional factors – for example, estimates covering vulnerable workers with strong enforcement, or estimates for the formal sector where minimum wages are binding. These are reported in Panel A of Figure 2.2. Note that the third and fourth features are not specified (similar to in our one-way comparisons in Table 2.2), so two features predicting stronger negative effects means two or more features. Thus, for example, when we summarize the estimates for vulnerable workers with strong enforcement, we do not specify formal vs. informal sector or whether the minimum wage is binding. We report (as we do in the remaining panels of Figure 2.2) the percentage of estimates that are positive but insignificant ("insignificant positive"), negative but insignificant ("insignificant negative"), positive and significant ("positive"), and negative and significant ("negative").

Figure 2.2 Results by Features of Estimates, Authors' Preferred Estimates, Sign and Significance

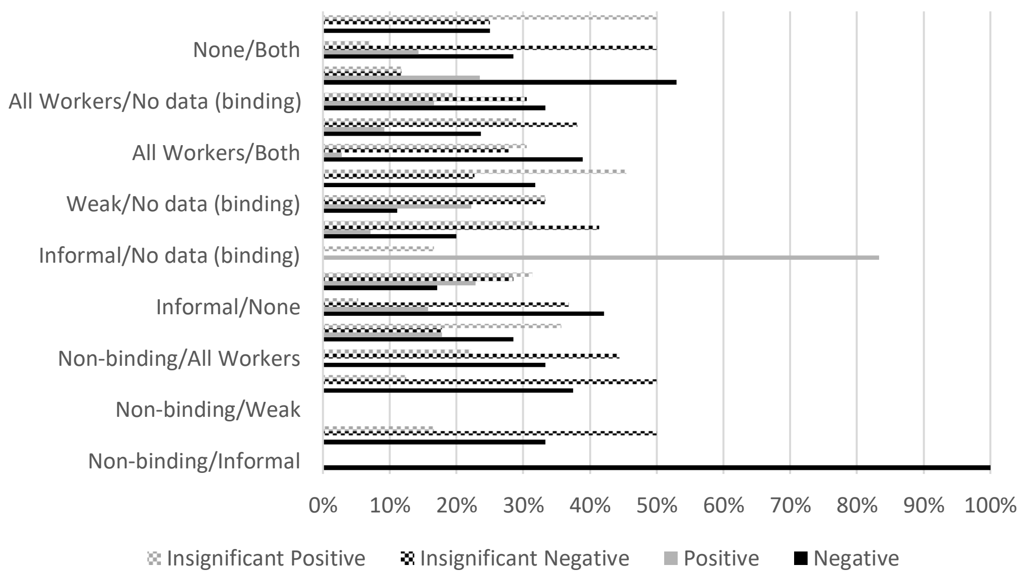
A. Both features more strongly predict negative effects



B. One feature more strongly predicts negative effects



C. Neither feature more strongly predicts negative effects



Note: Results labeled "Positive" or "Negative" have p-values ≤ 0.05. "None" refers to no enforcement, and "Weak" to weak enforcement. "Both" refers to covering the formal and informal sectors combined.

In Panel B, we report these percentages for estimated elasticities for which only one feature of the estimates in each possible pair of features predicts negative employment effects. Thus, for example, corresponding to the vulnerable/strong estimates in Panel A, we have two sets of estimates in Panel B – vulnerable/none (no enforcement) and vulnerable/weak. We thus learn how removing the strong enforcement feature from the vulnerable/strong pair affects the estimates. And Panel C does this for pairs in which neither feature in the pair predicts negative employment effects. Corresponding to what we said above, in Panel B one or more features more strongly predict negative employment effects, and in Panel C at most two features more strongly predict negative employment effects (or alternatively two or more features do *not* predict more negative effects). Appendix Table A6 reports the total number of estimates for each pair shown in the figure and reports similar information for the figures that follow.

Figure 2.2 shows a few things. Looking first at Panel A, when two (or more) features of an estimate more strongly predict negative effects, the estimated elasticity is much more likely to be negative. This is reflected in the black bars (for negative effects) being, in all cases, much longer than the gray bars, indicating higher percentages of estimated elasticities that are negative. In all cases but one, fewer than 20% of estimates are positive – summing across the solid gray bars for negative and significant elasticities, and the patterned gray bars for negative and insignificant elasticities. This contrasts with Panels B and C – when only one, or neither, feature in the pair considered predicts stronger negative effects. In Panel B, the differences between the black and gray bars – corresponding, respectively, to negative estimates and positive estimates – are less pronounced, and in some cases there are not many fewer positive than negative estimates (whether significant or not).[39] This weaker evidence of negative effects when fewer features more strongly predict negative employment effects is even more apparent in Panel C, for which neither feature in the pair predicts stronger negative employment effects (meaning that at least two of the four features we consider do not more strongly reflect negative employment effects). Indeed, while Panel B still indicates a preponderance of negative elasticities, in Panel C there are multiple cases with a larger share of estimates that are positive (e.g., weak/no data (binding) and informal/all workers).

One might also ask, from this figure, if there is evidence about which features of estimates are more strongly associated with finding a negative employment effect. However, because the other features of estimates not in each pair considered can vary, this can be misleading. We come back to more explicit evidence on this question below.

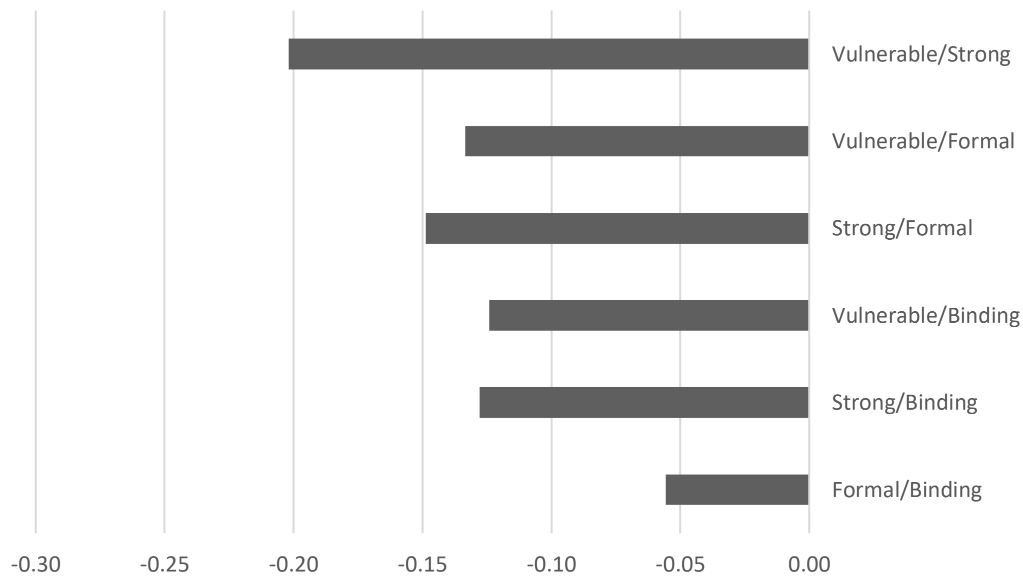
Figure 2.3 presents three panels for the same pairs of features, but this time reporting the average magnitude of the elasticity. In Panel A, for pairs in which both features of estimates more

[39] There is one case – "Strong/Informal" – where "all" the estimates are positive and significant. But Appendix Table A6 shows that there is only one elasticity in this category.

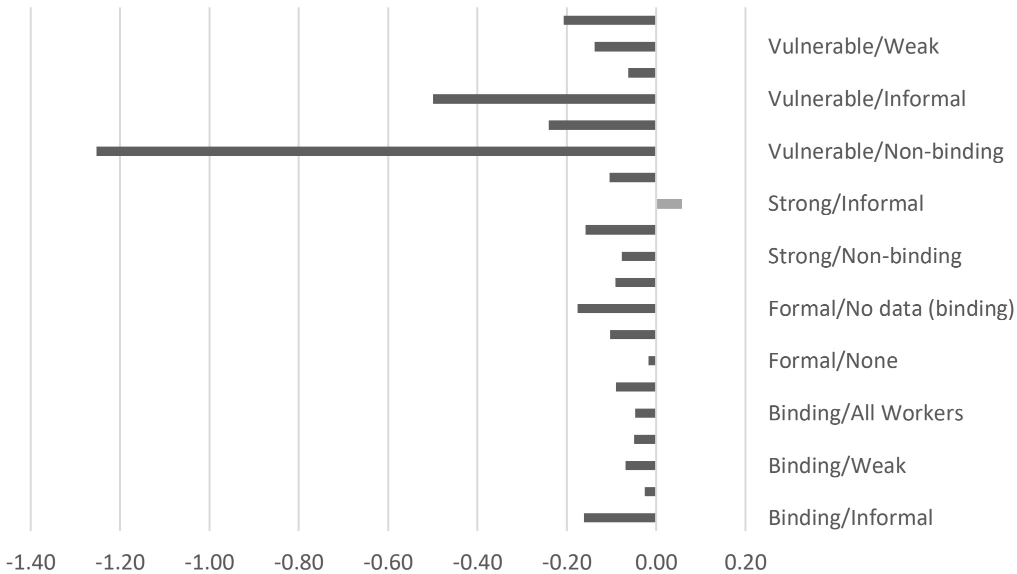
strongly predict negative employment effects, the elasticities are negative in every case, with one in the range of −0.06 range, four in the range of about −0.12 to −0.15, and one around −0.20. In Panel B, the average elasticity is negative in all cases but one (strong/informal). But in many cases the elasticities are closer to zero (and some quite close), although there are some cases with larger negative elasticities. (However, the most extreme case, for "vulnerable/non-binding," is based on only two estimates.) Finally, in Panel C, when neither feature predicts stronger negative employment effects, there are more positive elasticities.

Figure 2.3 Results by Features of Estimates, Authors' Preferred Estimates, Average Elasticities

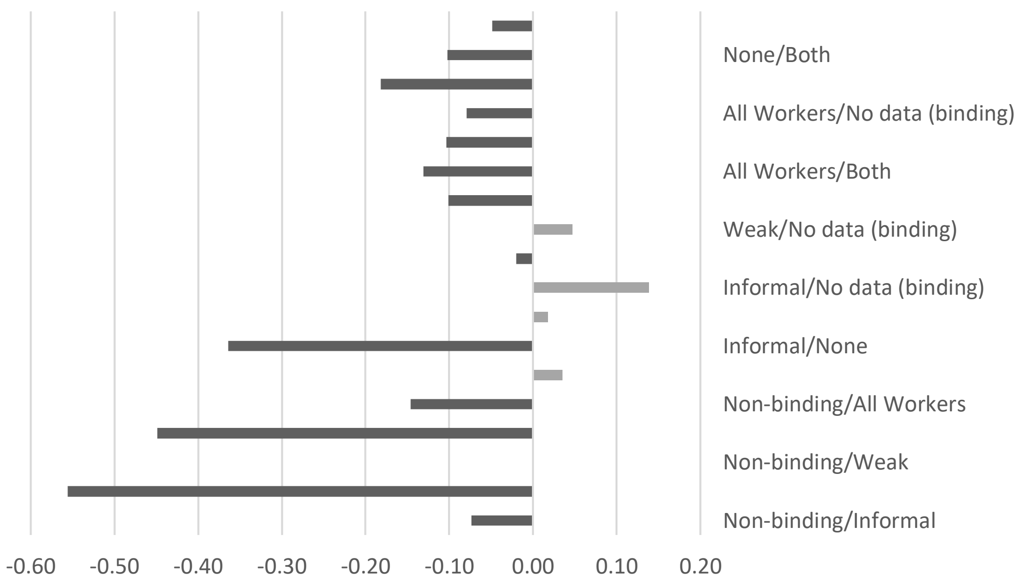
A. Both features more strongly predict negative effects



B. One feature more strongly predicts negative effects



C. Neither feature more strongly predicts negative effects



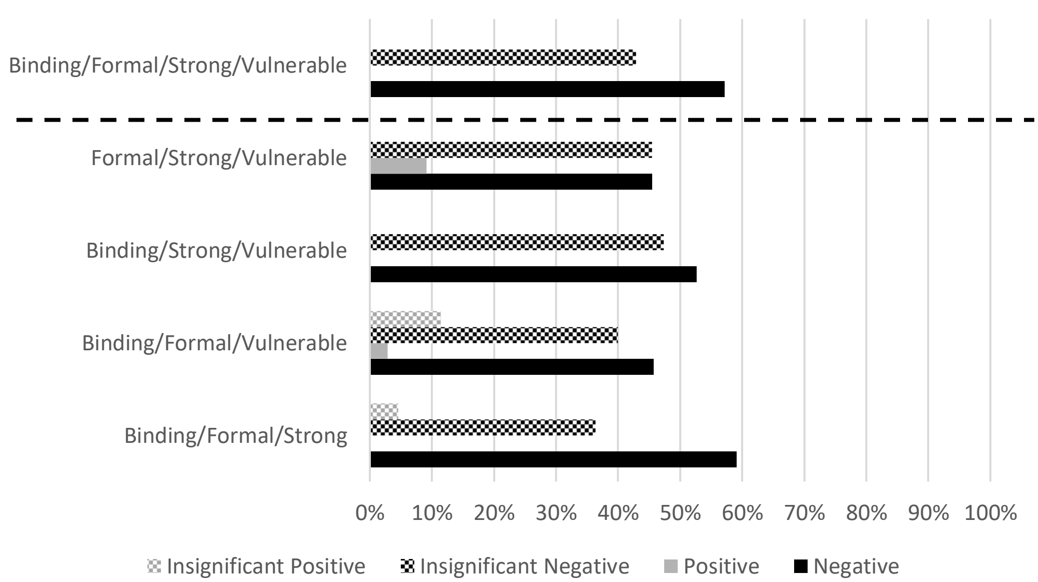
Note: See notes to Figure 2.2.

Thus, the evidence from Figures 2.2 and 2.3 suggests that when more features of estimated elasticities more strongly predict negative employment effects, the estimates are more likely to be negative. However, when we look at only pairs of features of estimates, the information can be quite noisy because the other two features of estimates not included in the pair are not specified. Hence, we next look at sharper evidence – based on whether at least three features of estimated elasticities, or all four features, more strongly predict negative employment effects based on the competitive model and institutional factors. This evidence paints an even clearer picture: when many features of an estimate more strongly predict negative employment effects, the evidence points quite unambiguously in that direction. In contrast, when many features do not more strongly predict negative employment effects, the evidence is much more mixed.

Figure 2.4 presents the evidence on the sign and significance of the estimates, for estimates for which three or more features more strongly predict negative employment effect. In Panel A, the first set of bars (above the horizontal dashed line) are for all four features. For these estimates, all of the estimates are negative, with 57.1% significant and 36.7% insignificant. The remaining sets of bars are for estimates for each set of three features that more strongly predict negative employment effects.[40] It is clear that for these estimates, nearly all of the estimates are negative, and more are statistically significant than not.

Figure 2.4 Results by Features of Estimates, Authors' Preferred Estimates, Sign and Significance

A. Three or four features more strongly predict negative effects



[40] Following what we did before, we report results for each combination of three features of estimates that more strongly predict negative employment effects, without specifying the fourth feature – which hence may or may not more strongly predict negative effects.

B. Three or four features do not more strongly predict negative effects

Note: Entries with no estimates are not shown. Entries above the dashed line are for four-way classifications of features of estimates. Results labeled "Positive" or "Negative" have p-values ≤ 0.05. See notes to Figure 2.2.

Panel B goes in the opposite direction, summarizing results for sets of features – in threes or all four – that do not more strongly predict negative employment effects. In this case, for which most (or none) of the features more strongly predict negative effects, there is no clear pattern of more negative than positive elasticities, and there are many sets of features for which there are more positive than negative effects (e.g., informal/weak/all workers and informal/weak/no data (binding). Note that for the bars above the dashed line, for estimates for which none of the four features more strongly predict negative effects, there are very few elasticities (see Appendix Table A4); hence the percentages reported for this set of bars, including the couple of cases of 100% negative elasticities, are not very reliable.

Figure 2.5 presents similar evidence, but for the magnitudes (average elasticities). Not surprisingly, the estimated magnitudes are all negative in Panel A, for estimates for which all or most features more strongly predict negative employment effects. In contrast, the evidence in Panel B, for estimates for which most features do *not* more strongly predict negative employment effects, is very mixed, with one-third of the sets of estimates on average positive. Note that all of the larger positive magnitudes (and six of the seven positive ones overall) correspond to estimates for the informal sector.

Overall, we view the evidence in Figures 2.4 and 2.5 as providing a quite clear message: When studies of the employment effects of minimum wages in developing countries have many (or most) features that more strongly predict negative employment effects, based on the competitive model and institutional factors, the evidence is a good deal more likely to point to negative employment effects.

2.5.3 Differences in results across features of estimates: meta-regressions

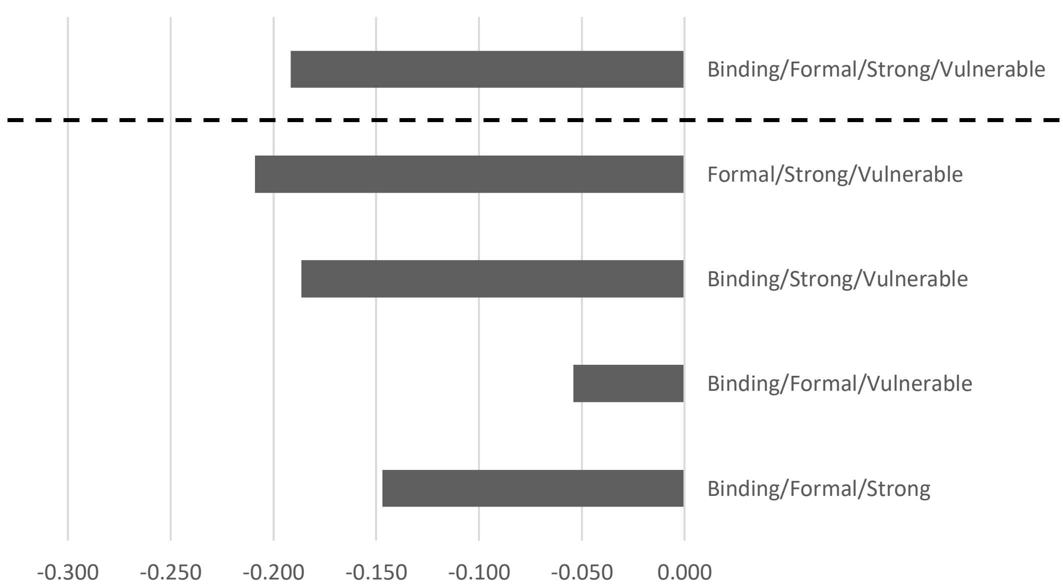
Finally, we turn to regression analysis of the estimates – or meta-regressions. We estimate models with three different dependent variables: a dummy variable for whether the estimate is negative; a dummy variable for whether it is negative and significant; and the estimated elasticity. For the first two cases, we use a linear probability model. We begin with simple specifications in which the regressors are mutually exclusive variables for whether zero, one, two, three, or four features of the estimates more strongly predict negative employment effects based on the competitive model or institutional factors. That is, for each of our outcomes, we estimate regression models of the form:

$$Y_j = \beta_0 SF_j^0 + \beta_1 SF_j^1 + \beta_2 SF_j^2 + \beta_3 SF_j^3 + \beta_4 SF_j^4 + \varepsilon_j \ , \qquad (1)$$

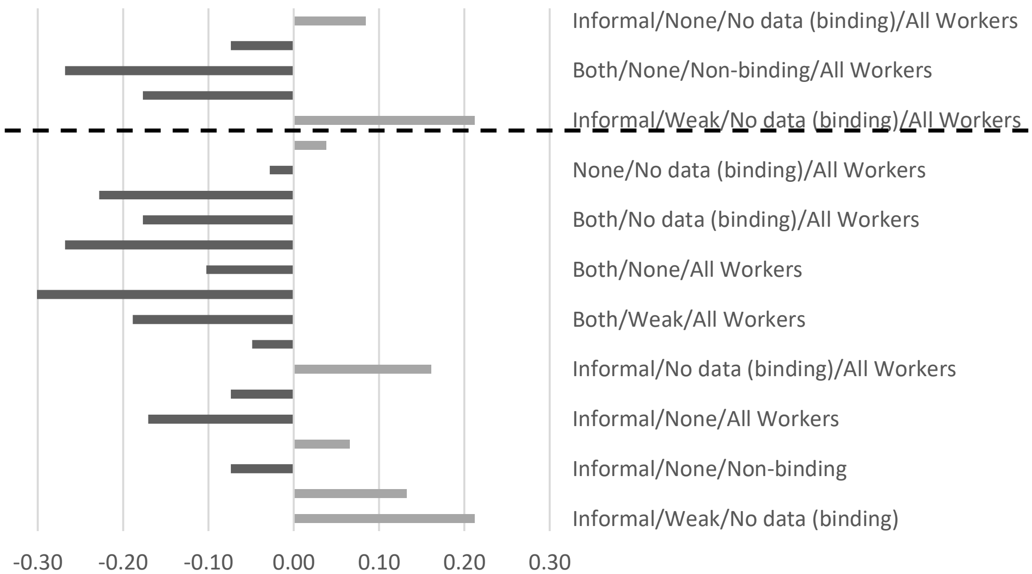
where there is no constant, j indexes estimates, the $SF^{\#}$ are dummy variables for the number of estimate features predicting stronger negative employment effects, and the Y_j are the alternative dependent variables.

Figure 2.5 Results by Features of Estimates, Authors' Preferred Estimates, Average Elasticities

A. Three or four features more strongly predict negative effects



B. Three or four features do not predict stronger negative effects



Note: Entries with no estimates are not shown. Entries above the dashed line are for four-way classifications of features of estimates. See notes to Figure 2A.

SF_j^0 is equal to one if none of the features of the estimates is classified as a stronger predictor of negative effects of the minimum wage on employment – a study that estimates the impact for the informal sector, on total employment (instead of vulnerable workers), for a country with weak enforcement, where the minimum wage is not binding. SF_j^1 is equal to one if the estimate is classified to have one feature that predicts stronger negative employment effects, and the other three do not. SF_j^2 is a dummy equal to one when two features predict negative employment effects, and so on.

This analysis provides some advantages relative to the preceding figures in terms of summarizing the evidence, at the cost of losing some of the richness of those figures. The regression estimates average over the sets of features of estimates we considered in the figures, which can increase precision but mask heterogeneous effects of study features. We are also able to do statistical inference on the results. And the regression analysis avoids the ambiguity of the whether the unspecified features of the estimates in the sets of two or three features of estimates considered in the figures do or do not more strongly predict negative employment effects – because the dummy variables $SF^{\#}$ are defined to be mutually exclusive. This meta-regression differs from a regression using dummies for each category – one dummy for binding, one for formal sector, etc. We prefer this more restrictive specification because there are very few observations for some combinations of features (see Table A6 in the

Appendix), although we describe richer specifications below.

The estimates of equation (1) are reported in Table 2.3.[41] The sample includes all 229 preferred estimates from the 61 studies, and we cluster the standard errors by study. In general, we see more systematic evidence of the conclusions we drew from the figures: when more features of estimates more strongly predict negative employment effects, the estimates are more consistent with negative employment effects. The estimates in column (1) are for the dichotomous outcome of whether the estimated elasticity is negative. There is a positive monotonic relationship between the number of features of estimates that more strongly predict negative employment effects and the probability that the estimated elasticity is negative. (Indeed, for four such features, there is no variation, as we saw in the top set of bars in Figure 2.5.)

We see very similar evidence in column (2) – where the outcome is a negative and significant elasticity. There is just one deviation from monotonicity, for the difference between zero and one feature of estimates. The estimated coefficients are smaller than in column (1), implying that there is a stronger relationship between the number of features of estimates that more strongly predict negative employment effects and finding a negative employment effect without regard to significance, than finding a negative and significant one.

Finally, in column (3), for the actual estimated elasticities, the evidence is not quite as clean with regard to a monotonic relationship, reflecting the variability in the estimates. (Here the signs are flipped because the dependent variable is the elasticity, not a dummy for whether the elasticity is negative.) Moreover, the average estimated elasticity is significant only for cases where two features of estimates more strongly predict a negative employment effect, although the point estimate is larger when all four features of estimates more strongly predict a negative employment effect (−0.192 vs. −0.125). As reflected in the counts of estimates with different numbers of features more strongly predicting negative employment effects (Appendix Table A6), this difference in statistical significance likely reflects at least in part the small number of estimates for which all four features more strongly predict negative employment effects.

Note that Table 2.3 also reports the statistical significance of the estimated differences based on the number of features that more strongly predict negative employment effects. For example, under the "Two estimate features" heading, the row labeled "Two = One (p-value)" is the p-value for the test of

[41] One might be concerned that the evidence for Brazil drives the results because we have 15 studies for this country (see Appendix Table A5). However, the estimates are very similar excluding the studies of Brazil (Appendix Table A7).

equality of the estimated coefficients of "Two estimate features" and "One estimate feature," or β2 and β1 in equation (2). Despite the generally quite clear relationships indicating that when there are more such features estimated employment effects are more likely to be negative, these differences often are not significant. They are, however, in a number of cases in columns (1) and (2), for tests of the difference in coefficients when all four features of estimates more strongly predict negative employment effects, vs. fewer features.

Next, we modify this framework to test more explicitly which features of estimates are more likely to lead to evidence of negative employment effects, or a larger negative elasticity. For the variables corresponding to one, two, or three features of estimates (from equation (1)) we alternatively define these to include or to exclude each estimate feature. For example, to ask whether evidence that the minimum wage is binding leads to stronger evidence of negative employment effects, we break each of the variables SF_j^1, SF_j^2, and SF_j^3 into two separate variables, based on whether or not the estimate is for a binding minimum wage. In this example, denoting these, for SF_j^1, as SF_j^{1B} and SF_j^{1NB}, equation (1) becomes:

$$Y_j = \beta_0 SF_j^0 + \beta_1{}^B SF_j^{1B} + \beta_1{}^{NB} SF_j^{1NB} + \beta_2{}^B SF_j^{2B} + \beta_2{}^{NB} SF_j^{2NB} + \beta_3{}^B SF_j^{3B} + \beta_3{}^{3NB} SF_j^{1NB} + \beta_4 SF_j^4 + \varepsilon_j \ . \qquad (2)$$

Note that the variables corresponding to zero features or four features are unaffected by this change, because they cannot be broken up this way. For this specification, evidence of more negative estimates for SF_j^{1B} than for SF_j^{1NB} (or similarly for SF_j^{2B} vs. SF_j^{2NB} or SF_j^{3B} vs. SF_j^{3NB}) would indicate that estimates for binding minimum wages – for the same number of estimate features more strongly predicting negative employment effects – are more likely to find evidence of negative employment effects. Hence, we also report tests of equality for these pairs of coefficients, for each study feature considered separately.

We report these results in Table 2.4, for the same outcomes as in Table 2.3 – a negative elasticity, a negative and significant elasticity, and the estimated elasticity itself. Each set of three columns considers one of our four features of estimates, with the variables for one, two, and three study features broken into separate dummy variables for whether or not that specific feature is included. The simplest way to interpret this evidence is to compare the estimated coefficients between the "includes feature" row and the "excludes feature" row, for a given number of features of estimated elasticities that more strongly predict negative employment effects.

Consider first the estimates in columns (1)-(3), for binding minimum wages. Column (1) reports results for whether the estimate is negative, comparing estimates that do and do not come from binding minimum wages. For estimates for which two features more strongly predict negative minimum wage effects, the estimated coefficient is larger in the "excludes feature" rows (0.724 vs. 0.711) – i.e., when the two estimate features that more strongly predict negative employment effects do *not* include binding minimum wages. In contrast, for estimates for which three features more strongly predict negative employment effects, the coefficient is larger when one of these features *is* binding minimum wages (0.824 vs. 0.750). In column (2) as well – where the outcome is a negative and significant employment effect, the relative magnitudes of these coefficients do not exhibit a consistent pattern. However, in column (3) – for the actual magnitude of the elasticity – the average elasticity is always larger negative for the features of estimates that exclude binding minimum wages. The table also reports the p-values for the tests of equality of these pairs of coefficients. There is never significant evidence of differences in columns (1)-(3); the lowest p-value if 0.24 (for three features of estimates, in column (3)).

Table 2.3 Meta-Analysis Regressions, Based on Counts of Features of Estimates More Strongly Predicting Negative Employment Effects

	(1)	(2)	(3)
Variables: number of features of estimates that more strongly predict negative employment effects	Negative estimate (LPM)	Negative and significant estimate (LPM)	Estimated elasticity
No estimate features	0.538***	0.385**	-0.074
	(0.190)	(0.152)	(0.112)
One estimate feature	0.647***	0.353***	-0.086
	(0.087)	(0.084)	(0.057)
One = No (p-value)	0.614	0.853	0.925
Two estimate features	0.709***	0.417***	-0.119***
	(0.052)	(0.079)	(0.042)
Two = One (p-value)	0.561	0.557	0.595
Two = No (p-value)	0.393	0.849	0.711
Three study features	0.810***	0.476***	-0.060
	(0.091)	(0.105)	(0.118)
Three = Two (p-value)	0.267	0.593	0.662
Three = One (p-value)	0.154	0.338	0.867
Three = No (p-value)	0.185	0.614	0.927
Four estimate features	1	0.643***	-0.192
	(0)	(0.058)	(0.127)
Four = Three (p-value)	0.040	0.183	0.433
Four = Two (p-value)	0.000	0.023	0.590
Four = One (p-value)	0.000	0.006	0.452
Four = No (p-value)	0.018	0.117	0.490
Joint test: Four = Three = Two = One (p-value)	0.000	0.033	0.880

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. There are 233 observations.
Note: LPM = linear probability model. The variables are defined to be mutually exclusive. For the LPMs, standard errors are clustered by study. Note that for the estimates in column (1), there is no variation in the dependent variable for the "Four estimate features" variables, which is why there is no variation in the estimated coefficient.

Table 2.4. Meta-Analysis Regressions, Testing Specific Features of Estimates More Strongly Predicting Negative Employment Effect, Conditional on Number of Such Features

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity
Feature:	Binding			Formal sector			Strong enforcement			Vulnerable workers		
Variables: number of estimate features that more strongly predict negative employment effects												
No estimate features	0.538***	0.308***	-0.074	0.538***	0.308***	-0.074	0.538***	0.308***	-0.074	0.538***	0.308***	-0.074
	(0.191)	(0.111)	(0.113)	(0.191)	(0.111)	(0.113)	(0.190)	(0.111)	(0.113)	(0.191)	(0.111)	(0.113)
One estimate feature (includes feature)	0.634***	0.293***	-0.038	0.800***	0.300*	-0.062**	-	-	-	0.250	0.000	-0.452
	(0.102)	(0.088)	(0.059)	(0.156)	(0.153)	(0.025)				(0.239)	(0.000)	(0.496)
One estimate feature (excludes feature)	0.643***	0.214**	-0.174	0.600***	0.267***	-0.075	0.636***	0.273***	-0.072	0.667***	0.294***	-0.043
	(0.133)	(0.107)	(0.147)	(0.095)	(0.081)	(0.067)	(0.082)	(0.071)	(0.055)	(0.088)	(0.077)	(0.048)
Equal coefficients for one estimate feature (p-value)	0.960	0.575	0.405	0.286	0.849	0.859	0	0	0.193	0.132	0	0.416
Two estimate features (includes feature)	0.711***	0.368***	-0.101***	0.675***	0.351***	-0.105*	0.714***	0.429***	-0.097**	0.828***	0.586***	-0.269**
	(0.063)	(0.089)	(0.036)	(0.061)	(0.082)	(0.054)	(0.077)	(0.118)	(0.042)	(0.088)	(0.139)	(0.117)
Two estimate features (excludes feature)	0.724***	0.483***	-0.188*	0.821***	0.536***	-0.181**	0.714***	0.390***	-0.135**	0.671***	0.329***	-0.070**
	(0.085)	(0.148)	(0.107)	(0.088)	(0.128)	(0.085)	(0.064)	(0.096)	(0.055)	(0.059)	(0.071)	(0.033)
Equal coefficients for two estimate features (p-value)	0.898	0.511	0.445	0.182	0.184	0.482	1	0.799	0.578	0.151	0.078	0.121
Three estimate features (includes feature)	0.824***	0.441***	-0.018	0.784***	0.405***	-0.045	0.857***	0.429***	-0.158**	0.794***	0.353***	-0.058
	(0.100)	(0.130)	(0.142)	(0.102)	(0.117)	(0.134)	(0.099)	(0.143)	(0.065)	(0.109)	(0.120)	(0.146)
Three estimate features (excludes feature)	0.750***	0.250*	-0.239*	1.000***	0.400	-0.172**	0.762***	0.381**	0.038	0.875***	0.625**	-0.069
	(0.222)	(0.128)	(0.122)	(0.000)	(0.307)	(0.077)	(0.150)	(0.169)	(0.225)	(0.111)	(0.239)	(0.070)
Equal coefficients for three estimate features (p-value)	0.764	0.300	0.243	0.038	0.989	0.417	0.598	0.830	0.407	0.603	0.308	0.949
Four estimate features	1.000	0.571***	-0.192	1.000	0.571***	-0.192	1.000	0.571***	-0.192	1.000	0.571***	-0.192
	(0.000)	(0.039)	(0.127)	(0.000)	(0.039)	(0.127)	(0.000)	(0.039)	(0.127)	(0.000)	(0.039)	(0.127)
Observations	229	229	229	229	229	229	229	229	229	229	229	229

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are clustered by study. There are 61 clusters and 229 observations.

Note: The table reports estimates of equation (2). LPM = linear probability model. The variables are defined to be mutually exclusive. For columns (7)-(9), "-" indicates that there are no estimates in the corresponding cell. Note that for the estimates in columns (1), (4), (7), and (10), there is no variation in the dependent variable for the "Four estimate features" variables, which is why there is no variation in the estimated coefficient.

Columns (4)-(6) report the same kind of evidence, but this time distinguishing estimates by whether they are for the formal sector or not. In this case, too, the evidence for whether the estimated coefficient is negative or negative and significant is not unambiguously in one direction. However, in column (6) the estimated elasticity is always larger negative when formality is excluded. Again, none of these pairwise differences in estimates are statistically significant (except in one case in column (4), for an estimated coefficient that has no variation).

The estimates in columns (7)-(9) consider differences depending on whether the estimate features include or exclude strong enforcement. In this case, there is no clear difference. Finally, the estimates in columns (10)-(12) focus on whether the estimate is for vulnerable workers. In this case, again, there is not clear evidence that the evidence of negative employment effects, or the magnitude of the negative effect, differs systematically based on whether one of the estimate features is a focus on vulnerable workers.[42]

Note that the specification in Table 2.4 is different from what might be viewed as the most standard type of meta-regression that simply includes, on the right-hand side, dummy variables for the different study features. A regression like that would take no account of whether (for example) studies with binding minimum wages tend to have only one study feature that more strongly predicts negative employment effects, while studies focusing on the formal sector tend to have more features that more strongly predict negative employment effects. If studies are unlikely to detect negative employment effects unless multiple features of the study more strongly predict negative employment effects, then there are important interactions between specific study features and the number of features that more strongly predict negative employment effects, which the specifications in Table 2.4 could reveal.

Nonetheless, we have estimated versions of the more standard meta-regression, and report the results in Table 2.5. In the first three columns, we omit the weakest study feature in terms of predicting negative employment effects (non-binding, no enforcement, informal sector, and all workers). In the next three columns we use a more parsimonious model, retaining only the strongest

[42] The estimates in column (12) provide a nice illustration of why the evidence from the columns for whether there is a negative estimated effect or a negative and significant estimated effect can be more reliable than the evidence for the estimated elasticity, as the latter can be sensitive to outliers. For the estimates for studies with one feature that more strongly predicts negative employment effects, the coefficients in columns (10) and (11) are larger for the studies that do not focus on vulnerable workers (0.667 and 0.294). But the estimated elasticity (column (12)) is larger (negative) for the studies that do focus on vulnerable workers (−0.452). There are only four studies in this category (focus on vulnerable workers, and no other features that more strongly predict negative employment effects), and one of these has an extreme estimated elasticity (−1.99).

such study feature (binding, strong enforcement, formal sector, and vulnerable workers).[43] In this table, the clearest evidence is that studies focusing on vulnerable workers are most likely to provide evidence of negative employment effects, and there is also some evidence of this (although a good deal weaker) for studies of countries with strong enforcement. However, Table 2.4, which compares results based on study features for studies including the same number of features that more strongly predict negative employment effects, suggests we have to be a bit cautious about this interpretation. In Table 2.4, we find stronger evidence of negative effects for estimates with two features that more strongly predict negative employment effects when the estimates are for vulnerable workers, in all three columns ((10)-(12)); the p-values for equal effects are fairly small, although only one, in column (11), is below 0.1. But for estimates with other numbers of features that more strongly predict negative employment effects, the estimated effects are larger when the vulnerable worker feature is excluded.[44]

Table 2.5 Standard Meta-Analysis Regressions, Testing Specific Features of Estimates More Strongly Predicting Negative Employment Effect

	(1)	(2)	(3)	(4)	(5)	(6)
	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity	Negative estimate (LPM)	Neg. and sign. est. (LPM)	Estimated elasticity
Binding	-0.017	0.252	0.294	0.105	0.045	0.103
	(0.126)	(0.165)	(0.225)	(0.073)	(0.091)	(0.086)
No data on binding	-0.186	0.164	0.243			
	(0.133)	(0.166)	(0.198)			
Strong enforcement	0.092	-0.031	-0.023	0.148**	0.119	-0.023
	(0.105)	(0.117)	(0.128)	(0.068)	(0.097)	(0.066)
Weak enforcement	-0.083	-0.252*	0.028			
	(0.117)	(0.140)	(0.102)			
Formal sector	0.135	0.021	0.065	0.041	-0.005	0.043
	(0.127)	(0.094)	(0.176)	(0.087)	(0.067)	(0.101)
All sectors	0.159	0.069	0.079			
	(0.152)	(0.153)	(0.190)			
Vulnerable workers	0.117	0.099	-0.130**	0.124*	0.112	-0.122**
	(0.071)	(0.115)	(0.058)	(0.068)	(0.106)	(0.057)
Minimum wage (baseline)	0.636***	0.218	-0.384	0.536***	0.274***	-0.153
	(0.151)	(0.172)	(0.254)	(0.086)	(0.078)	(0.102)
Observations	229		229	229	229	229

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered by study. There are 61 clusters and 229 observations.

Note: LPM = linear probability model.

[43] To be clear, standard regressions in meta-analyses usually include other types of regressors, such as for the data used, the sample size, perhaps the precision, etc.

[44] We also estimated meta-regressions with all the possible two-, three-, and four-way interactions of study features. Not surprisingly, given the large number of highly collinear variables, and the small number of observations in some of the cells (see Appendix Table A4), the estimates of these regressions were quite imprecise. Results are available from the authors upon request. The models in Tables 2.3, 2.4, and 2.5 are restricted versions of this model.

To summarize, Tables 2.3, 2.4, and 2.5 consider three different but related kinds of evidence. Table 2.3 focuses simply on the number of features of estimates – of the four we consider – that more strongly predict negative employment effects based on the competitive model or institutional factors. Table 2.4 tries to disaggregate this evidence, paying attention not only to the counts of estimate features, but also asking whether particular features of estimates among these four features are more consistently associated with evidence of negative employment effects, conditional on the number of features that more strongly predict negative employment effects. And Table 2.5 presents a more standard type of meta-regression that focuses on study features but without reference to how many other study features more strongly predict negative employment effects. In general, we do not find strong evidence pointing to particular features of estimates that generate stronger evidence of negative employment effects. There is some evidence of this for studies focusing on vulnerable workers, in Table 2.5, but this is not robust in Table 2.4. However, the evidence (from Table 2.3) is quite clear that estimated employment elasticities based on a greater number of features that more strongly predict negative employment effects are, in fact, more likely to be negative, or negative and significant. And such estimates, to a limited but lesser extent, are more likely to take on larger negative values.

One potential caveat to our interpretation of the evidence is that it is conceivable that the study features noted or documented by a study's authors were chosen (or emphasized) to rationalize a particular result.[45] For example, a researcher failing to find a negative employment effect might be compelled to study whether the minimum wage was in fact binding, and provide evidence that is was not, whereas a researcher finding (and expecting) a negative employment effect might not. Or a researcher might first estimate employment effects for all workers, but after not finding a negative employment effect decide to look at more vulnerable workers, leading to finding a negative effect. In these examples, researchers who believe in the competitive model could end up highlighting features of the data, country, etc., which help rationalize the results in terms of the competitive model – what we might term "analysis bias" as opposed to "publication bias." We cannot decisively rule this out, although our sense is that the problem is not likely to be severe. First, some of our study features are beyond the researcher's control (like enforcement, or whether the data break out formal- and informal-sector workers). Second, for the analyses that reflect research decisions about what to explore (whether the minimum wage is binding, estimating effects for the formal and informal sector, and isolating effects for vulnerable workers), we would argue that these issues are very standard in the research

[45] This possibility was suggested by a reviewer.

literature on minimum wages in developing countries, suggesting most researchers would present these analyses as long as the data are available.[46]

2.6 Conclusions

The goal of this paper is to see whether we can make sense of the mixed evidence on the employment effects of minimum wages in developing countries. Although estimated effects tend to be negative, there is considerable heterogeneity, with many non-negative estimates. We try to distinguish between two explanations. One is that there simply is no clear evidence that minimum wages reduce employment in developing countries, in which case we should see heterogeneous estimates even across similar studies or estimates looking at workers most likely to be adversely affected by minimum wages (because, e.g., they are low skill, or work in the formal sector), and in contexts where negative effects are more likely (e.g., when minimum wages are more binding). Alternatively, the heterogeneity in estimated minimum wage effects may reflect heterogeneity in estimates along dimensions more likely or less likely to predict negative employment effects – e.g., estimates for binding minimum wages for low-skill workers vs. estimates for weakly enforced minimum wages in the informal sector, and estimates for which more features more strongly predict negative employment effects. To try to distinguish between these explanations, we conduct different versions of meta-analyses of the estimates from a large set of studies of minimum wage effects in developing countries.

We conclude that the evidence is much more consistent with the second explanation. That is, we find that the estimated employment effects of minimum wages in developing countries are more likely to be negative, and larger negative, when estimates focus on data and sectors for which the competitive model predicts disemployment effects and in institutional settings in which we would expect the minimum wage to have more adverse impact. Specifically, there is more consistent evidence of negative employment effects for estimates for which multiple features of the estimates – including when the minimum wage is binding, where minimum wage enforcement is stronger, for the formal sector, and when the data focus on vulnerable (lower-wage) workers – predict negative employment effects. To be precise, the evidence is less clear on whether a particular one of these features that characterizes a study is more strongly associated with negative employment effects (although there some evidence that disemployment effects are more likely to emerge from studies of vulnerable – i.e.,

[46] In addition, for the particular example of whether a study indicates that minimum wages are binding, there are plenty of estimates showing negative employment effects that do not present evidence on whether minimum wages are binding.

lower-wage – workers). The difficulty of pinning down exactly which study features matter the most for whether the evidence points to negative employment effects likely arises because studies can vary on many dimensions (corresponding to all of these features). But the evidence is clearer that when all or most features of a study predict negative employment effects, the study is in fact more likely to find negative employment effects.

One implication of this conclusion is that the apparently mixed evidence is a result of many studies focusing on data, sectors, or institutional settings in which negative employment effects are less likely. As such, many of these studies may be uninformative about the effects of minimum wages when the competitive model and institutional factors more strongly predict negative employment effects: studies of binding minimum wages, with strong enforcement, focusing on vulnerable workers, in the formal sector. On the other hand, the implication is that in some developing country settings negative employment are in fact less likely – e.g., for informal sector employment. However, a further implication is that precisely when minimum wages in developing countries could potentially deliver the most benefits – when minimum wages are binding and enforced, and when they apply to vulnerable workers in the formal sector – the disemployment effects are most apparent, implying that minimum wages in developing countries may present more of a tradeoff between higher wages and lower employment than might be apparent from a simpler look at the evidence across studies of employment effects in developing countries. Hence, in assessing the wisdom of minimum wage increases in developing countries, it is important also to weigh evidence on other outcomes, such as whether higher minimum wages in developing countries raise incomes of low-income families – benefits that might offset the costs of some job losses for vulnerable workers. Gindling (2018) suggests that, overall, minimum wages tend to reduce poverty in developing countries, but only modestly.

Finally, one dimension we do not explore is whether monopsony power is sometimes relevant. There are some cases of positive estimates (although not many) with features for which the competitive model and institutional factors predict negative employment effects. (These positive estimates are more prevalent in studies with only one feature for which the competitive model and institutional factors predict negative effects; see, e.g., Figures 2.2 and 2.4). Monopsony is a potential explanation, but not the only one; for example, the standard two-sector competitive model predicts positive employment effects in the informal sector. Testing whether monopsony can sometimes explain a positive effect of the minimum wage on employment is hard. Recent work for the United States (Azar et al., 2019; Munguía Corella, forthcoming) tries to do this using disaggregated, sub-national variation in measures of labor market concentration and worker mobility, and finds some evidence consistent with monopsony power in more-rural, less-dense counties. There is no way to apply this type of analysis to the "study-level" or "estimate-level" observations we use in the present paper, but exploring whether monopsony power sometimes generates positive employment effects of the minimum wage in developing countries would be useful.

Still, at this point our view is that there is no clear reason, based on the existing evidence, to conclude that competitive models of the labor market do not do a good job of characterizing low-wage labor markets in developing countries. Evidence of negative employment effects tends to emerge where the competitive model predicts it should, although this conclusion does not apply to every study, and different conclusions more consistent with monopsony could hold for some countries or more likely sub-regions of countries.

3. Minimum Wages in Monopsonistic Labor Markets

3.1 Introduction

Policies that introduce minimum wages are often controversial. An extensive literature has studied the effects of minimum wages on employment. While most of the evidence points toward a negative impact, there is also plenty of new evidence of zero or even positive effects. Theoretically, adverse effects on employment are expected within a competitive labor market. However, under a less competitive market, where firms have monopsony power, wages can be lower than the optimal level, and a minimum wage can have ambiguous effects. The literature has primarily focused on the average effect of the minimum wage on employment. Still, almost no studies have empirically analyzed whether these effects depend on the degree of monopsony in the labor market.

Many studies have used the monopsony model to explain non-negative results, including those conducted by Card and Krueger (1994), Katz and Krueger (1992), Allegretto et al. (2011, 2013), and Dube et al. (2010, 2016). However, none of these mentioned papers empirically test whether their results are due to labor market concentration or monopsony (Neumark, 2019).

There is one exception, namely, a working paper by Aznar et al. (2019), where the authors analyze the effect of minimum wages in three occupations (stock clerks, retail salespeople, and cashiers) in the U.S. They also construct an HHI of employment, and their data comes from the website CareerBuilder.com. They estimate the interactions of the minimum wage and HHI. Their results are in line with this paper as well. However, their results are limited to only a few occupations and do not cover all the U.S. counties, while this paper looks at a broader set of occupations and covers all U.S. counties.

What are the effects of minimum wages under monopsonistic labor markets? According to the monopsony model, the effect of a minimum wage is ambiguous if the labor market has monopsonistic characteristics, and outcomes will depend on the minimum wage level and the supply and demand of each firm. In this model, the minimum wage effects depend on the elasticity of labor supply; if the labor supply is inelastic, then the monopsony power is higher, and minimum wages might have positive effects on employment.

I address this question by empirically identifying how minimum wages' effects depend on the monopsony power of the market, where monopsony is measured by labor market concentration or labor mobility. To measure the degree of concentration, I construct a Herfindahl-Hirschman Index (HHI) that measures the concentration of total industrial employment at a county-cluster-quarter level for the U.S. using the Quarterly Workforce Indicators (QWI). I propose different methods to measure the relevant labor market (in clusters) and its concentration. The QWI is not a survey, but actually, it is data from almost all the firms in the U.S. Therefore, it has the advantage that represents most of the universe of the employment in the country, and report data at different industrial levels; hence, it is possible to calculate the concentration by industry. The drawback is that it is not possible to measure the HHI at firm level. To measure labor mobility, I calculate total workers' flows across industries using the Current Population Survey (CPS). The CPS is a sample of the most representative counties in the U.S., but it has the benefit that it follows workers across time. Thus, it is possible to estimate flows between industries but, as in the case of the HHI, this estimation is only possible at industry level.[47] In addition, I build clusters of industries for the HHI using the labor mobility of workers. Clusters are created when a certain number of flows of workers between industries are registered. Hence, clusters of industries share a demand for similar labor skills, which is more reasonable than assuming that workers can only work in one industry.

I estimate interactions of the minimum wage and the monopsony variable (HHI or mobility) to separate the minimum wage effect on teenage employment depending on the degree of concentration and labor mobility at a county-time level. In all the scenarios, the minimum wage has negative effects in competitive labor markets, and the effect is positive in high concentration or low-mobility counties. In monopsonistic labor markets, increases in minimum wages can be constrained by supply or demand; thus, the effect can be positive or negative. Therefore, I estimate the effect on highly monopsonistic labor markets for different levels of the minimum wage bindingness. I measure the level of bindingness with the minimum wage share relative to the county's average in a specific period. The estimation allows me to capture the effect in counties where the minimum wage "bites" the equilibrium wage[48], in other words, where firms are more likely to be demand constrained.

The results indicate that minimum wages have an elasticity of -0.418 under perfect competition, which is much higher than the usual literature results. By contrast, the elasticity for full concentration is 0.04 (HHI=1) and 0.293 for low mobility, but neither is significant. The effects are positive for HHI higher than 0.9. There are only 0.12% of total teenage workers in counties where the minimum wage has positive effects, but it is also true that in 44.19%, the effects are not significant. The results are consistent across different specifications and with controls for possible external shocks

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[47] In the main results, I only using flows of workers that did not move to another county (geographical area). However, the results do not change, if I include this group of workers (see Appendix Table A11).
[48] Assuming that the average wage is a raw proxy for the equilibrium wage.

to the HHI. In addition, at full concentration and zero mobility of the workers, the effect on employment is more negative if the minimum wage is more binding, aligning with the monopsony theory.

This paper's main contributions to the literature are (1) studies the heterogeneous effects of minimum wage in the labor market monopsony power and (2) identifying the effect depending on the equilibrium wage (demand- or supply-constrained). I distinguish different effects depending on how close the minimum wage is to the average wage.

The rest of the paper is organized as follows. Section 3.2 presents a summary of the literature. Section 3.3 describes the data. Section 3.4 explains the construction of the concentration index and the labor mobility variable. Section 3.5 lays out the identification strategy, which includes the effects of minimum wages on local labor markets with different monopsony degrees. Section 3.6 presents the results, and Section 3.7 concludes.

3.2 Literature Review

This section summarizes the literature on the effects of minimum wages and monopsony in labor markets. I focus on papers that analyze minimum wage effects in concentrated labor markets and industries with monopsony power. For a more extensive review of minimum wage effects on employment, see Neumark (2019) and Dube (2019).

For this review, I sort studies into two groups: (1) theoretical approaches to the impact of minimum wages under a monopsony model, and (2) empirical methods to measure minimum wages' effects under monopsony.

Robinson (1933) proposed the monopsony theory. The model explains how the labor market works when the supply curve is not perfectly elastic, and firms are not wage takers. More recent developments related to this topic are presented in the papers of Bhaskar and To (1999) and Bhaskar et al. (2002), and particularly in Manning (2003). These studies examine how monopsonistic labor markets work and provide detailed explanations of different situations that can arise within them. Monopsony in the labor market can arise through concentration, giving firms higher markups and the power to set the wage level. Monopsonistic behavior also results from frictions and the heterogeneous preferences of the workers. For instance, a reduction in wages may not affect employment if frictions hinder workers' ability to change jobs, such as specific laws or contracts. Another example is workers' preference for jobs closer to home, so increasing wages in remote locations does not affect the labor supply. Note that the friction creates a non-perfectly elastic labor supply curve (as also shown in Card et al., 2018).

In the case of the minimum wage and similar policies, the monopsony model is about the supply curve elasticity and the equilibrium wage. Manning's (2003) model predicts that minimum wages' effect is ambiguous under monopsonistic labor markets. There are three possible scenarios: (1) firms are unconstrained because the minimum wage is not binding; (2) firms are supply-constrained, and increases in minimum wages have positive effects on employment, and (3) firms are demand-constrained, and if the minimum wage is high, it has negative effects on employment. Hence, minimum wages have ambiguous effects within monopsonistic labor markets, depending on how high the minimum wage is and the degree of competition. For example, an increase in the minimum wage could have positive effects on employment if the wage is below the wage of perfect competition equilibrium (the supply curve determines the impact), and it could have adverse effects if the wage is higher than the perfect competition equilibrium wage (the demand curve determines the wage). I identify these scenarios by estimating the minimum wage's effects in highly concentrated labor markets for different minimum wage binding levels.

A few papers have analyzed the effect of minimum wages in less competitive labor markets. Three papers are relevant because they include estimations of the minimum wage effects under monopsony. One is by Neumark and Wascher (1994b), who propose an approach to estimating the minimum wage effects on competitive model with two regimes and monopsony model with three regimes. They estimate the effects in a three-regime endogenous switching regression model. Their estimations indicate that a small fraction of the observations lie in the supply curve (third regime of the monopsony model), making employment increase with a rise in the minimum wage.

The second study is by Wessels (1997), who looks at the specific case of servers in the restaurant industry. Tips are a percentage of a meal's total cost; therefore, as restaurants hire more servers, marginal revenues per serving fall. Restaurant owners must raise the hourly wage to retain the workforce, which implies that they face a rising supply curve of labor. The author proposes a quadratic specification and measures quartiles' effects using dummy variables to estimate the effect of the minimum wage and capture the positive and negative parts of the impact over the supply and demand curves. He finds that the minimum wage has a positive impact on the linear term and negatively affects the quadratic term, which is very indirect evidence that the monopsony model predictions apply in the servers' labor market.

A recent working paper by Azar et al. (2019) analyzes the minimum wage effect on the employment of stock clerks, retail salespeople, and cashiers. They also construct an HHI of employment, and their data comes from the website CareerBuilder.com. Their approach is similar to the one used in this paper. They estimate the interactions of the minimum wage and HHI. Their results are in line with this paper as well: they find that minimum wages have positive effects in concentrated labor markets (elasticity of 0.2). However, their results are limited to only a few occupations and do not cover all the U.S. counties, while this paper looks at a broader set of occupations and covers all U.S. counties.

Finally, it is important to briefly mention that there has been a proliferation of new papers that focus on estimating the effect of monopsony on average wages using labor market concentration as a proxy (Azar et al., 2017; Benmelech et al., 2018; Lipsius, 2018; Rinz, 2018; and Abel et al., 2018). Moreover, other studies have calculated the firm's supply elasticity to measure the effect of monopsony (Falch, 2010; Hirsch et al., 2010; Staiger et al., 2010; Webber, 2016; Dube et al., 2018). In both approaches, the authors find that monopsony power in the labor market is associated with lower average wages, consistent with the results in this paper. However, none analyze the effect on employment, nor interactions with minimum wages.

This paper fills in an important gap in the literature by directly investigating how the effects of minimum wage change with market concentration and labor force mobility. In particular, I estimate the effects of the minimum wage when the labor market is monopsonistic or competitive. I estimate the effects in monopsonistic labor markets depending on how much the minimum wage bites the equilibrium wage. Unlike Azar et al. (2019), which focuses on a particular sector, I examine all industries in the U.S. I also group similar industries within a county to allow workers to change jobs across industries and create a more credible labor market (clusters). Thus, it is a more flexible approach that allows workers to change jobs within industries.

3.3 Data

The U.S. labor market data used comes mainly from two sources: the QWI and the CPS. The QWI data are produced through a partnership between the U.S. Census Bureau and the state Labor Market Information (LMI) offices. It provides a public-use aggregation of the matched employer-employee Longitudinal Employer Household Dynamics (LEHD) database. The data are compiled from administrative records collected by 50 states and the District of Columbia for both jobs and firms. The unit of observation in the QWI is the worker–employer pair. The microdata's primary source of information is the almost universal employer-reported Unemployment Insurance (U.I.) records, which cover around 98% of all private-sector jobs. The U.I. records provide details on employment, earnings, industry, and place of work. Data from the Census Bureau are used to either match or impute workers' demographic information.

Most states entered the QWI program between the late 1990s and the early 2000s. In the 1990s, fewer than five states were in the program, while 42 states had come online by the 2000s. Therefore, the period of the analysis in the paper is from 2000 to 2016 for every quarter. I use information about employment, earnings, county, and age range. The data in the QWI is presented by industry at different levels of aggregation. The industries are classified using the North America Industry Classification System (NAICS), which is the standard classification of economic activities used by Canada, Mexico, and the U.S. The NAICS groups together economic units that have a similar process of production. It has six levels of aggregation. The first two digits of the code designate the sector, the third designates the subsector, the fourth digit designates the industry group, the fifth digit designates the NAICS industry, and the sixth digit designates the national industry. The QWI presents the employment and earnings by age, race, and sex at the 4-digit level (industry). Hence it is possible to aggregate teenage employment (ages between 14 and 19) at a 3-digit level (subsectors).

The QWI and the NAICS allowed me to construct the industrial employment HHI for subsectors by county and quarters. The unit of observation in the QWI is industry, county, and quarters. Once the HHI is estimated, all the data are aggregated at the county level. Aggregation is done following the QWI documentation and weighting the HHI by the total employment in each county-quarter. The analysis and estimations are conducted at the county-quarter level to make the results comparable with other papers that analyze minimum wage effects on teenage employment using the QWI (Allegretto et al., 2013; Dube et al., 2016; Meer and West, 2016; and Thompson, 2009).

The CPS is a voluntary survey of about 60,000 households that are selected each month. In contrast with the QWI, the CPS information comes from the households, and it has a monthly frequency, while in the QWI, the data comes from establishments, and it has a quarterly frequency. However, the CPS's monthly data can be aggregated into quarters, and the survey is representative of all U.S. employment.

The CPS allowed me to estimate the flows of the total workers across industries from 2000 to 2016,[49] making it possible to calculate all the industry switches by worker. These flows are used to calculate the labor mobility between industries and calculate industrial clusters for the HHI. As in the case of the HHI, the labor mobility is calculated at the industry level, and then, it is aggregated to the county-quarter level.

To construct the HHI and the mobility, I assume that the relevant labor market occurs within a county, since, in the U.S., labor mobility between counties is limited, it has decreased significantly over the past few years, and job flows often occur in the same geographic area (Moretti, 2011; Molloy et al., 2014). According to the CPS, only 21.15% of the workers moved to a different county during the period of analysis. Thus, in my primary estimations, I dropped the workers that change their location (county) once they change jobs. However, I present the estimation including all the workers (even if they move to another county) in the Appendix for mobility. The results are similar; the effect of the minimum wage on low labor mobility counties is more favorable than in counties with more mobility.

The HHI and the flows between industries are calculated using total employment instead of teenage employment. The reason is that total employment better reflects the monopsony power of each industry. For instance, suppose there are 10 industries, but teens work at only one of them in the data. The HHI or mobility for all workers is very low but is high for teens. Nevertheless, presumably, teens could work in other industries. This suggests the HHI or mobility should be estimated for all workers, not just teens. Nevertheless, in the Appendix, I added all the estimations using an HHI and mobility constructed with teenage employment for completeness.

Lastly, I use data from the Census Bureau to calculate the total population and teenage population. These two variables, plus total employment, are used as controls. I utilize the correspondence codes and Vaghul and Zipperer's (2016) minimum wage data set to recover minimum wages by counties.

[49] Households are treated as follows: contacted for four consecutive months, out of sample for the next eight months, back in the sample for the following four months, and then retired from the sample.

3.4 Measurements of Monopsony

I construct two monopsony measurements; one is labor market concentration, and the other the mobility of workers across industries. Labor market concentration is measured with an HHI of the total industrial employment at the county-cluster-period level using the QWI.

The concentration of the labor market is a proxy for monopsony. In concentrated markets, workers have fewer job opportunities. Thus, firms have more monopsony power to set wages. The mobility measures how often workers switch to different industries when they change jobs. Mobility is also a good predictor of monopsony; if there is low mobility among firms, it implies that there are frictions resulting in monopsony power. Theory predicts that wages and employment must be lower in monopsonistic labor markets than in competitive ones, and in the case of policies like the minimum wage, its effect on employment is ambiguous (Manning, 2003). Note that I am calculating both variables at the industry level, which does not necessarily translate into the same conclusion for concentration and labor mobility at a firm level. This issue is addressed in section 3.4.3.

Using market concentration (HHI) as a proxy of monopsony aligns with the new research about monopsony effects in the U.S. (Azar et al., 2017; Benmelech et al., 2018; Abel et al., 2018; Lipsius, 2018; Rinz, 2018; Aznar et al., 2019). Other studies, such as those by Webber (2016) and Dube et al. (2018), directly estimate the labor supply elasticity to measure monopsony.

Low mobility of workers among firms is likely a proper measurement of monopsony as well. For instance, if workers cannot move freely among jobs (because of lack of job opportunities or the presence of friction, among other factors), then the supply elasticity to the firm is positive, which is the definition of monopsony. I identify workers' flows between different industries, and I calculate the percentage of workers who do not switch industries when they change jobs as a proxy to monopsony.

3.4.1 Construction of the HHI for Employment

In this section, I detail how to calculate the HHI. First, I estimate the HHI at the industry level by calculating how much of an industry's total employment is taken by one specific cluster. If an industry in a specific county has very few clusters capturing most employment, the HHI is high. The higher the HHI, the higher the monopsony power of the employers in the area. Once I calculate the HHI at industry levels, I estimate the average HHI by county.

Ideally, to measure monopsony correctly, I need to estimate the labor supply elasticity to each firm in each geographic area. However, it is challenging to obtain firm-level data and determine the supply elasticity. I explained this shortcoming in greater detail in section 4.3. Another possibility is to use occupations instead of industries to measure the demanded skills in the labor market. One problem is that data on occupations are not compatible with the QWI, but more important is that flows between industries and occupations show that workers more frequently change their occupation than their industrial sector. If workers can change their occupation more easily than their industry, that means that occupation does not measure specific skills accurately, and industry is a better proxy for demanded skills.

I use the QWI to calculate the HHI, and I define the labor market by geographic area and cluster of industries. The data for the industries is presented as a 4-digit NAICS code (industry), and, using data from the CPS, I use flows of workers between industries to define clusters.

One objection to calculate the HHI only with NAICS codes is that it assumes that a worker can only have a job in the same 3-digit NAICS industry. For instance, it is not credible that a restaurant worker cannot find a job in a business within a similar industry, such as a convenience store. Therefore, I use CPS to estimate flows between industries. I follow workers between 2000–2016 to calculate the number of times that a worker switches industry, and I compute all the movement between industries. The flows between industries are used to calculate clusters of industries for the HHI. However, the clusters are created when a certain number of workers' relative flows [50] between industries are registered.[51] Thus, if it is common that workers switch between NAICS 4233 (Lumber and Other Construction Materials Merchant Wholesalers) and 3311 (Iron and Steel Mills and Ferroalloy Manufacturing), this forms a new cluster or a new labor market, which consists of the union of both industries. See Figure 3.1 for more details.

I follow different criteria to calculate the industry clusters. I create a network of industries connected by links, where each industry is a node. I need to restrict the number of relative flows to define a link because if I use a small number of relative flows, all the industries became one whole cluster. Thus, I define a link as those relative flows of workers above the mean[52] between industries in the whole period and all counties. Once a link is defined, I allow that all the industries connected

[50] The relative flows are the total flows between two or more industries divided by the total employment in the industries connected.
[51] The assumption is that if two or more industries have many flows between each other, they likely demand the same skills in labor, and therefore, they are the same labor market.
[52] I try different cutoffs for the number of relative flows. If I consider nodes with fewer than the mean, it results in one cluster of industries (all the industries are connected). Hence, using the mean of flows can be interpreted as the minimum number of flows needed to have at least two clusters of industries.

by a link become one cluster. Next, I followed a rule: I only use the top three connections for each industry (i.e. one industry with another three) or any other industry with at least in the 90th percentile of the number of relative flows. This allows me to capture only the most important connections. For instance, the industry 4239 (Miscellaneous Durable Goods Merchant Wholesalers) has more relative flows with 562 (Waste Management and Remediation Services), 2213 (Water, Sewage and Other Systems), and 2123 (Nonmetallic Mineral Mining and Quarrying). However, there are many flows with other industries, such as the industry 4219 (Miscellaneous Durable Goods Wholesalers).

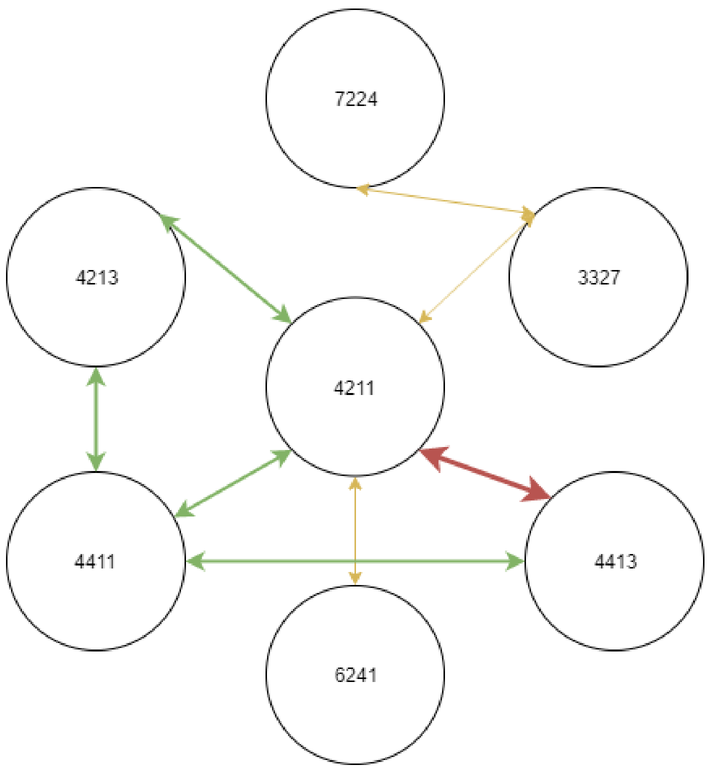

Figure 3.1 Creation of Clusters of Industries or Labor Markets

Notes: The red link indicates that the two industries have more relative flows than any other pair (top pair). Green links indicate a strong relationship (the top 3 pairs or more than 90th of relative flows between industries); the sum of red and green links defines the preferred classification. Yellow links are week connections; the sum of yellow, green, and red links define the flexible method.

Thus, in these cases, I added more industries to the cluster until the next candidate has less than the 90th percentile of the relative flows. Using this classification results in 22 clusters of industries. This is my preferred classification, but to check the robustness of the classification, I define another three classifications (see Appendix D) to make clusters. One flexible classification that allows a cluster to be formed by all the links between industries, one that allows only the top two stronger connections make a cluster, and one that uses the NAICS code to define the labor market (no clusters are formed). All these classifications are tested in the robustness section 3.6.3.

Table 3.1 Average Number of Establishments by HHI

	HII		Low Mobility	
	Mean	Median	Mean	Median
Monopsony=1	5.38	3.18	581.35	361.81
90th	10.96	7.75	847.61	564.40
10th	1,815.18	611.20	2,135.22	826.06
5th	2,261.91	708.59	1,689.81	708.81

Note: I calculate the average and the median number of establishments if the HHI=1 and Mobility=1, as well as for the 90th, 10th, and 5th percentiles of both variables across observations (county-time).

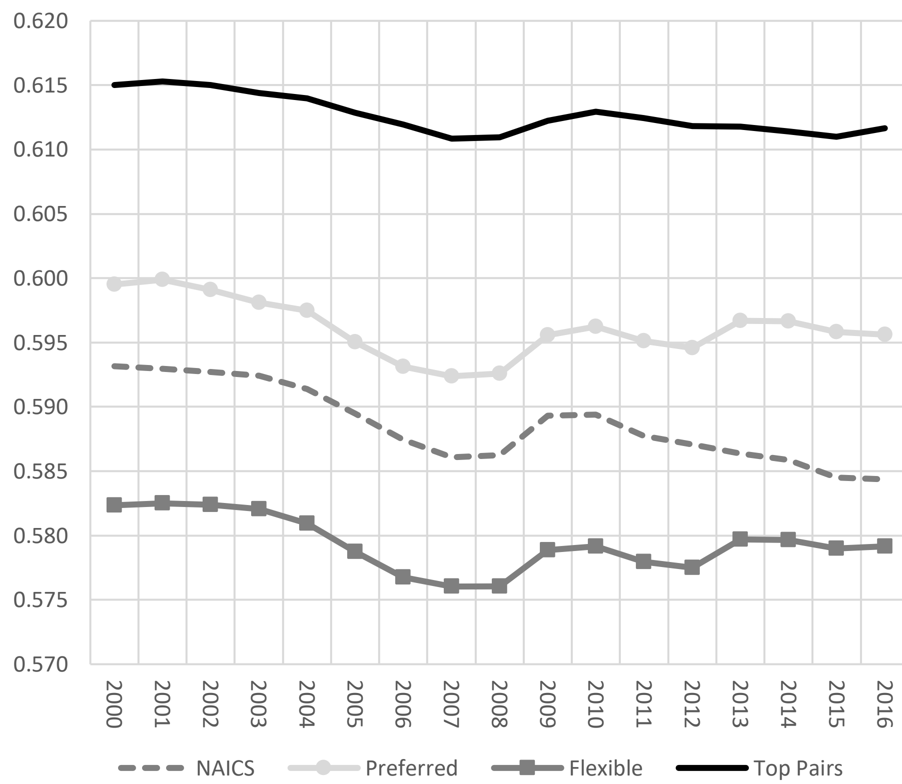

Figure 3.2 Evolution of the HHI in the U.S.: 2000–2016

Note: The HHI is estimated by averaging industries and counties by year (weighted by population).

Once I have defined the labor market into clusters, it is possible to calculate concentration by 3-digit NAICS code (subsector level), area (county) and period (quarters), where the share is

$$s_{i,j,a,t} = \frac{emp_{i,j,a,t}}{\sum_{i=1}^{N} emp_{i,j,a,t}} \qquad (4.1)$$

and *emp* is the total employment of the cluster (4-digit code) i, which is part of cluster j, in area a at period t. NAICS codes are designed to aggregate from 4-digit to 3-digit; for instance, all the codes below 111 (Crop Production) are related: 1112 is for Vegetable and Melon Farming, and 1113 is for Fruit and Tree Nut Farming. However, in the case of clusters, I create new codes for clusters that are related by the flows of workers. For instance, a created/new code 988 includes two industries, 4851 Urban Transit Systems and 5615 Travel Arrangement and Reservation Services. These two industries are part of different NAICS subsectors (485 and 561 respectively), but for the HHI, I aggregate them into one cluster.

The HHI is aggregated as follows:

$$HHI_{j,a,t} = \sum_{i=1}^{N} s_{i,j,a,t}^{2} \qquad (3.4.2)$$

Once I have the $HHI_{j,a,t}$ at clusters, I calculate the average concentration at the county level using the QWI documentation to aggregate the data.

To contextualize the behavior of the measurements of monopsony, I show the evolution of the different classification of concentration in time. All the HHI have similar patterns: concentration has increased in recent years, and with small declines in 2012 and 2016. Hence, the increase in the HHI may explain why studies using recent data are more often finding non-negative effects of minimum wages on employment (See Table A8 for basic statistics of the HHI index).

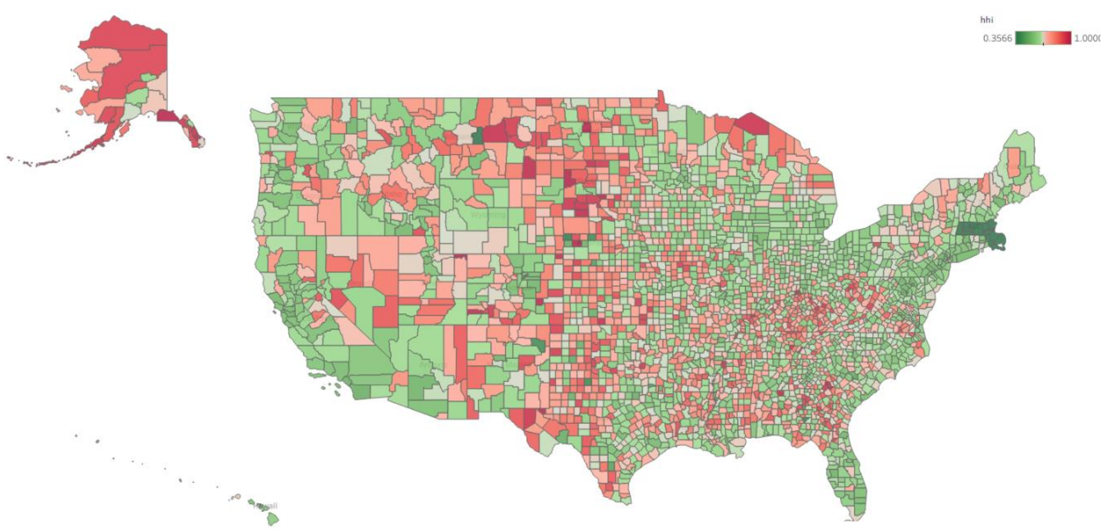

Figure 3.3 HHI in the U.S. across Counties: 2000–2016

Note: The HHI is estimated by averaging industries and year by county (weighted by population). I use the hybrid method for the estimation of the HHI.

Next, I show a map of the U.S. to illustrate the regional differences. Green indicates more competitive labor markets, whereas red indicates the opposite (more concentrated markets). There is much heterogeneity in the U.S. and also within states. In general, however, there are more green areas than red areas. The key is the relative differences between areas. The map shows that there is more relative concentration in rural areas than in urban ones. However, this does not mean that changes in the HHI affect more the employment in rural areas.

3.4.2 Construction of Labor Mobility

The second variable is labor mobility between industries. The labor mobility is also estimated at the county level, and I am using the CPS to estimate it. Labor mobility is a good proxy for monopsony: low mobility of workers implies higher monopsony power of the employers.

To calculate labor mobility, I estimate the percentage of workers who do not change industries when they change jobs (equation 3.4.3). I prefer to calculate it this way so that it can be compared with the HHI. A low mobility=1 means that there is zero mobility of workers out of the industry, which implies high monopsony power.

$$Labor\ Mobility_{i,a,t} = \frac{remained_{i,a,t}}{remained_{i,a,t} + moved_{i,a,t}} \qquad (3.4.3)$$

As I mention, I am assuming that the relevant labor market is local, i.e., that the workers stay in the same county when they switch industries. Hence, I dropped all the workers that change their location (county) once they change jobs. However, I also present estimations for mobility including all the workers (even if they move to another county) in the Appendix. The results are very similar.

One possible issue is that low industrial labor mobility does not necessarily translate into low firm labor mobility. An industry can be composed of many competitive firms, in which case the monopsony power of each of them would be very limited. Each worker could look for a job among the many firms without having to transfer to another industry. However, as it is showed in the next section, the industrial labor mobility is actually capturing firm's concentration.

3.4.3 Drawbacks of the Measurements of Monopsony

The measurements of monopsony have two drawbacks. First, I am not estimating the supply elasticity, which is the best proxy for monopsony. Second, the HHI and the labor mobility are estimated at the industrial level rather than at the firm level.

For the former issue, even if it is true that elasticities are the best proxy of monopsony, its calculation requires more disaggregation of the data. A recent paper by Aznar et al. (in progress)[53] estimates the relationship between supply elasticity and labor market concentration (measured as an HHI). A higher concentration of employers is negatively associated with supply elasticity, which suggests that concentration is a contributing factor to firm-level wage-setting power. Therefore, I consider that using the HHI is a good proxy for monopsony: It is highly correlated with the labor supply elasticity, and in addition, highly concentrated markets present monopsonistic characteristics.

The second issue is that I do not have firm-level data to construct the HHI and the labor mobility. The HHI calculated here assumes that a high concentration at the 4-digit industry or cluster level implies that the workers in that cluster have very few potential employers: hence, the firm has monopsony power. This assumption may be questionable because a higher concentration in an industry does not necessarily mean that there is a lesser number of firms. The same applies for the labor mobility variable. However, both indexes roughly capture the level of concentration by firm. For instance, the estimated elasticities of wages to the HHI in this paper are very similar to those in the papers by Benmelech et al. (2018), Lipsius (2018), and Rinz (2018), which indicates that the HHI used here is an excellent proxy for the one that is constructed from firm-level data.

Besides, I can test how well both proxies approximates the real level of concentration of firms by calculating the number of establishments across industries and counties for different degrees of HHI and mobility.[54] Table 3.1 shows the average and the median number of firms by some percentiles of the HHI and the mobility.

A high HHI implies that fewer firms are available in the industry-county-time observations: for instance, if the HHI=1, the median of firms is 3.18, and the mean is 5.38. In contrast, if the HHI is small (5th percentile), the median of firms is 708.59, and the mean is 2,261.91. These numbers imply that even if the industrial HHI does not precisely measure concentration at the firm level, it is at least highly correlated. For the case of labor mobility, a similar pattern emerges. If the variable is equal to

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[53] https://www.aeaweb.org/conference/2019/preliminary/1059, retrieved on November 2nd, 2019.

[54] I have data on the number of establishments by county and NAICS code. The information source is the County Business Patterns from the Census Bureau. The correlation is very robust for different classification of the clusters.

one (which means that there is no mobility among industries), the median of firms is way lower than when there is more mobility.

3.5 Empirical Strategy

I use two approaches to estimate the effect of the minimum wage in monopsonistic labor markets. First, I calculate interactions between the minimum wage and the two measures of monopsony (HHI and labor mobility) using two-way fixed effects (geographic area and time). I estimate the effect on teenage workers because this group of workers has a wage closer to the minimum wage[55]. The difference in the impact of minimum wage on employment between monopsonistic markets and competitive ones must be positive, regardless of whether the firms are demand- or supply-constrained. Thus, I expect a positive sign in the coefficient of the interactions. I estimate most of the results using two-way fixed effects in order to make them comparable with the existing literature. Finally, I also report the total effect of the minimum wage for different values of the HHI.

Second, I estimate the effects depending on how much the minimum wage bites the average wage. To measure the bindingness, I calculate the share of the minimum wage relative to the average wage for each county-quarter ($\frac{MW}{Avg\ Wage}$). The objective is to measure the interaction of the minimum wage and the two monopsony proxies at different levels of the minimum wage. The more the minimum wage bites the equilibrium wage in a county, the more negative must be the effects on employment compared to in counties where the minimum wage is well below the average wage.

3.5.1 Baseline Specifications

The first specification is the interaction of the minimum wage and the two variables of monopsony. The coefficient of the interaction measures how the impacts of minimum wage depend on the degree of labor market monopsony.

$$\mathrm{y_{it}} = \alpha + \beta_1 \mathrm{Ln(MW)_{it}} + \beta_2 \mathrm{M_{it}} + \beta_3 \mathrm{M_{it}} * \mathrm{Ln(MW)_{it}} + \gamma \mathrm{X_{it}} + \phi_i + \tau_t + \varepsilon_{it} \qquad (3.5.1)$$

[55]It is important to mention that the results only apply to teenage workers (low-skilled workers), which are also those for whom a binding minimum wage is more relevant. Therefore, I estimate the effects to prime-age workers (22-54 years old). This estimation serves as a falsification test. The minimum wage effects and its interactions with monopsony are not significantly different from zero. See Table A11. HHI and mobility are calculated for the total number of workers. There is no information for mobility by age. However, it is plausible to assume that industries with a high concentration of total employment have a monopsony power that also affects teenagers. To see the same effects using HHI with only teenage employment, see Appendix Table A10.

y_{it} is the variable of interest (log of the teenage employment[56]) in county i in the period t. MW_{it} is the minimum wage, and M_{it} is the monopsony variable (HHI or labor-mobility). β_1 measures the effect of the minimum wage under perfect competition[57] ($M = 0$), and β_3 is the estimation of the difference between the effect of the minimum wage in monopsonistic labor markets ($M = 1$) and the effect on a competitive market ($M = 0$). Technically, β_3 measures such a difference, so the difference depends on the level of concentration. β_2 is the effect of monopsony on employment. X_{it} is a vector of covariates: log of the total population, log of the teenage population, and total employment in the private sector. Finally, the fixed effects by geographic area (ϕ_i) and time (τ_t) are included in the equation.

The following specification adds interaction of the minimum wage and industry for the HHI variable. In this case, the data are not aggregated, and the unit of observation is industry (cluster), county, and time. It is possible that the HHI partly reflects product market power, and the interaction effect of the minimum wage with HHI might not reflect only monopsony power.[58] Hence, there is a potential omitted variable (product market power × minimum wages) that correlates with HHI and minimum wage interaction. Thus, the interaction of minimum wages and industry is necessary to minimize this potential bias.

$$y_{jit} = \alpha + \beta_1 \mathrm{Ln}(MW)_{jit} + \beta_2 HHI_{jit} + \beta_4 HHI_{jit} * \mathrm{Ln}(MW)_{jit} + \beta_5 Industry_{jit} * \mathrm{Ln}(MW)_{jit} + \\ + \gamma X_{it} + \phi_i + \tau_t + \psi_j + \varepsilon_{jit} \qquad (3.5.2)$$

j is the industry or cluster, and $Industry_{jit}$ is a dummy variable to separate the minimum wage effect by industry. β_5 measures the minimum wage effect by industry with respect to the dropped industry (β_1) under perfect competition. β_4 is the average effect of the minimum wage under monopsony with respect to all the industries. Finally, the ψ_j term is included in the equation to control for industry fixed effects.

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[56] I am using employment because the QWI measures employment instead of employment rate as the CPS. However, this is controlled with the log of teen population variable.
[57] Note that HHI=0 and low mobility=0; both measure perfect competition. If the low mobility variable is equal to zero, it means that all the workers move to different industries every time they change jobs.
[58] Note that the response of employment to a higher minimum wage might vary by industry (depending on a set of variables that, according to Marshall's Laws, affect the elasticity of labor demand).

3.5.2 Minimum Wages and Different Degrees of Concentration

My second identification aims to estimate the effect of the minimum wage under full monopsony (either HHI=1 or low-mobility=1) with different degrees of minimum wage bindingness. The objective is to identify the effect of the minimum wage depending on the wage of equilibrium. For example, an increase in the minimum wage could have positive effects on employment if the wage is below the wage of perfect competition (the supply curve constrains the impact), and it could have adverse effects if the wage is higher than the perfect competition level (the demand curve constrains the wage). As I explained, the bindingness is measure as the minimum wage relative to the average wage in each county-quarter. I estimate the effects as follows:

$$\mathrm{y}_{\mathrm{dit}} = \alpha + \beta_1 \mathrm{Ln}(\mathrm{MW})_{\mathrm{dit}} + \beta_2 \mathrm{M}_{\mathrm{it}} + \beta_3 \mathrm{Ln}\ (\mathrm{HHI})_{\mathrm{it}} * \mathrm{Ln}(\mathrm{MW})_{\mathrm{dit}} + \gamma \mathrm{X}_{\mathrm{it}} + \phi_i + \tau_{\mathrm{t}} + \varepsilon_{\mathrm{it}} \quad (3.5.3)$$

Equation (3.4.3) describes the specification for interactions where d is the number of deciles of the minimum wage bindingness ($\frac{MW}{Avg\ Wage}$). I estimate equations for d=1,2,…,10 separately and evaluate all the variables in the mean with $\mathrm{M}_{\mathrm{it}} = 1$, and I calculate the marginal effect of the $\mathrm{Ln}(\mathrm{MW})_{\mathrm{dit}}$ on $\mathrm{y}_{\mathrm{dit}}$. The results are reported for all the coefficients in Figure 3.5. Theoretically, if the minimum wage is very low, there must be no effects on teenage employment, because it is not relevant (very few workers earn less than the minimum wage). However, under full monopsony, results must be positive for a certain level of bindingness, and the effect must be less positive (and even negative) if the minimum wage is too high.

3.5.3 Controlling for Possible Multicollinearity and External Shocks

Two potential problems arise in my specification. First, it is possible that multicollinearity maybe introduced if minimum wage changes affect HHI. I verify if they are correlated by estimating the relation between the HHI and the minimum wage. There is not a significant correlation between minimum wages and HHI (see Appendix). Second, the HHI effect can be confounded with an external shock. For instance, if a shock reduces the number of firms in a country, the employment will decrease and the HHI will increase, creating the false interpretation that the HHI is affecting the employment. To reduce this possible bias, I propose two specifications.

First, I estimate an equation that uses the period average of the HHI instead of the variation over time (Equation 3.5.4), second, I estimate the average HHI using the first 2 years (2000 and 2001)

(Equation 3.5.5), and then I use this average to calculate the effects over the period 2002–2016. These approaches reduce not only the possible bias of external shocks but also the possible effects of the minimum wage on HHI.

$$y_{it} = \alpha + \beta_1 Ln(MW)_{it} + \beta_2 \overline{HHI}_i + \beta_3 \overline{HHI}_i * Ln(MW)_{it} + \gamma X_{it} + \phi_i + \tau_t + \varepsilon_{it} \quad (3.5.4)$$

$$y_{it} = \alpha + \beta_1 Ln(MW)_{it} + \beta_2 \overline{HHI}_{00-01,i} + \beta_3 \overline{HHI}_{00-01,i} * Ln(MW)_{it} + \gamma X_{it} + \phi_i + \tau_t + \varepsilon_{it} \quad (3.5.5)$$

$\overline{HHI}_i$ is the average of the HHI in the period 2000–2006 by county, and $\overline{HHI}_{00-01,i}$ is the average only for the year 2000–2001 by county. Note that both variables vary between counties but do not variable in time.

Note that another approach would be to use simple lags instead of the average of a previous period. However, simple lags may be less effective depending on the shock. If the effects of the shock are persistent and last more than a quarter, simple lags will not be enough to reduce the bias.

3.6 Results

In this section, I first present the effect of the concentration index and labor mobility on teenage wages to verify that the measurement is consistent with the theory and the previous literature. Second, I show the impact of the interactions of the minimum wage and the monopsony variables on teenage workers. Third, I present a specification that estimates the effects of minimum wages under a monopsony labor market for different levels of minimum wage. The objective of the last item is to verify if minimum wages have different effects depending on the equilibrium wage (supply- or demand-constrained). Lastly, I estimate the interactions using alternative measures of HHI to account for possible bias.

3.6.1 Effects of Labor Market Concentration on Teenage Workers' Wages

In Table 3.2, I estimate the effect of the HHI on wages. In all the tables, I present in column (1) the effects using the HHI and in column (2) using the labor mobility.

Table 3.2 Effects of the HHI and Low Mobility on the Log of Teenage Wages

Dependent Variable: Ln (Wage)	(1) HHI	(2) Low Mobility
HHI	-0.0993***	
	(0.0254)	
Low Mobility		-0.127
		(0.161)
Constant	8.128***	6.784***
	(0.716)	(1.391)
Observations	199,168	18,121
R-squared	0.718	0.888

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1
Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, the log of teenage population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 means full concentration. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more relative flows of workers (top three links with highest flows with more than 90th percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. See Section 4 for more details.

Both monopsony variables have negative effects on the average wages of teenage workers among all the columns. However, it is only significant for the HHI. An increase of one standard deviation in the HHI implies a change in the elasticity of -0.099. These numbers are similar to those found in the papers by Benmelech et al. (2018) and Lipsius (2018)[59] although, they are calculating the HHI at the firm level. These results also confirm that the measurement of the industrial HHI is highly correlated with the firm HHI. The result also aligns with the monopsony theory. When firms have more monopsonistic power, the equilibrium wage should be lower than in perfect competition.

3.6.2 Impacts of Minimum Wages in Concentrated Labor Markets

In this section, I estimate the impact of minimum wages interacted with the monopsony variables. The effects on teenage employment are presented in Table 3.3. The specifications of each column are the same as in Table 3.2. The first row measures the effect of the monopsony variable in employment. Both the HHI and labor mobility are negative and significant. These results are also consistent with theory, as they predict that the higher the monopsony power, the lower the employment level. The

[59] Benmelech et al. (2018) estimate an elasticity of the HHI on wages of -0.017; however, they estimate effects for all firms. In the case of Lipsius (2018), the effect is much higher (a -0.07 elasticity to wages).

interaction (in the second row) measures the differentiated effect of minimum wages when HHI=1 or low mobility=1, that is, the difference between full monopsony and competitive markets. The elasticity is positive and significant across both columns. This result is also consistent with theory, as firms hiring teenage workers are more likely to be constrained by the minimum wage. It can be inferred that most of the firms are supply-constrained because interaction elasticity is positive.

In the next rows I present the effect of the minimum wage on employment for different levels of monopsony. HHI=0 or low mobility = 0 is equivalent to the effects of the minimum wage on the employment under perfect competition. For the HHI, the elasticity is -0.418 and significant. The elasticity is more negative than the usual elasticity estimates in the literature because the effects are estimated in the more competitive labor markets of the U.S. In contrast, if the HHI=1, the effect is positive (insignificant). The higher the HHI, the less negative is the effect of the minimum wage on teenage employment. It is also important to note that the effect becomes insignificant around an HHI of 0.5. The population weighted mean of the HHI for all the U.S. is 0.595. Thus, monopsony may be explaining recent insignificant effects on teenage employment. For the case of the lower mobility, the same pattern arises: the lowest the mobility, the effects of the minimum wage on employment are less negative. However, it is important to note that the effect is never significantly different from zero.

To compare the HHI elasticities with Azar et al. (2019), an increase of one standard deviation in the HHI is associated with an increase in the employment elasticity of the minimum wage of 0.05, whereas in Azar et al. (2019) the increase to the employment elasticity is around 0.2. Thus, the results are similar; however, it is important to point out that the sample is very different. I measure the effect across all the U.S. and all industries; in contrast, Azar et al. (2019) are only estimating for a few occupations.

The results imply that, under monopsonistic labor markets, raising the minimum wage can be a good policy to increase the income of those workers that are at the bottom of the income distribution without dealing with a high opportunity cost. However, it is also crucial to understand that, in areas where the labor market is more competitive, an increase in the minimum wage can hurt employment.

Table 3.3 Effects of the Log of the MW Interacted with the HHI and Low Mobility on the Log of Teenage Employment

Dependent Variable: Ln (Teen Emp)	(1) HHI	(2) Low Mobility
Monopsony Variable (HHI or LM)	-0.833**	-0.915***
	(0.324)	(0.235)
Monopsony x Ln (MW)	0.459**	0.476***
	(0.180)	(0.124)
Elasticity of the MW depending on Monopsony		
Monopsony = 0	-0.418***	-0.183
	(0.112)	(0.146)
Monopsony = 0.2	-0.326***	-0.0876
	(0.0931)	(0.148)
Monopsony = 0.4	-0.234***	0.00755
	(0.0858)	(0.155)
Monopsony = 0.6	-0.142	0.103
	(0.0930)	(0.165)
Monopsony = 0.8	-0.0507	0.198
	(0.112)	(0.179)
Monopsony = 1	0.0411	0.293
	(0.138)	(0.194)
Constant	-0.193	-1.786
	(0.741)	(1.553)
Observations	199,231	18,126
R-squared	0.988	0.989

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1
Notes: All specifications include two-way fixed effects (county and time). Control variables are log of the total population, the log of teenage population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 means full concentration. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more flows of workers (top three links with highest flows with more than 90th percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. See Section 4 for more details.

To better understand the effects on employment, using the coefficients of Table 3.3, I calculate in Table 3.4 the share of the teenage population that should have negative and positive effects depending on the level of HHI. Minimum wages have negative and significant effects on most of the teenage employment (55.81%) in the U.S. and the positive effects (insignificant) in only 0.12% of the employment. However, this also means that the effect of the minimum wage is not significantly different from zero for 44.19% of teenage employment.

Table 3.4 Percentage of the Teenage Employment by the Significance of the Minimum Wage Effects Depending on the Monopsony Variable

	Share of the total Teenage Employment
Negative Significant	55.81%
Negative	44.06%
Positive	0.12%
Positive Significant	0.00%

Notes: I am using the "Hybrid" classification, but the results are very similar to the other classifications. The calculations are computed as follows: (1) I estimate the coefficients with the regression models, (2) use the coefficients to estimate the MW effects on the teenage employment, (3) determine at what level of HHI the MW effect is negative, negative significant, positive, and positive significant; (4) aggregate the employment by HHI, and (5) calculate the shares of employment where the MW has negative, negative significant, positive, positive significant effects. The estimation is using the coefficient of the regression model of column (1) in Table 3.3.

3.6.3 Impacts of Minimum Wages on Employment in Monopsonistic Labor Markets: Different Levels of Minimum Wage

In a monopsony model, the minimum wage effect depends on the equilibrium wage and not only on the degree of monopsony. The prediction of the monopsony model is ambiguous, even if the labor market has monopsonistic characteristics. For example, an increase in the minimum wage could have positive effects on employment if the wage is below the wage of perfect competition (the supply curve constrains the impact), and it could have adverse effects if the wage is higher than the perfect competition level (the demand curve constrains the wage). In order to examine this possibility, I estimate the minimum wage effects for different levels of the minimum wage bindingness. To make it more comparable among counties and to better measure the bindingness, I compute the average wage in the county divided by its minimum wage (i.e., degrees of bindingness) and run regressions by deciles of the bindingness.[60]

Instead of presenting all the estimations, I plot the marginal effect of the minimum wage, either HHI=1 or low-mobility=1, which means that we are comparing the effect of the minimum wage in full monopsony labor markets but at a different degree of the minimum wage bindingness.[61]

As shown in Figure 3.4, the HHI and labor mobility have a similar pattern. At very low levels of bindingness (i.e., the minimum wage is almost irrelevant), the elasticity is positive but very small

[60] In the Appendix, Figure A2, I also present the results for quintiles. The interpretation of the result is the same for deciles and quintiles.

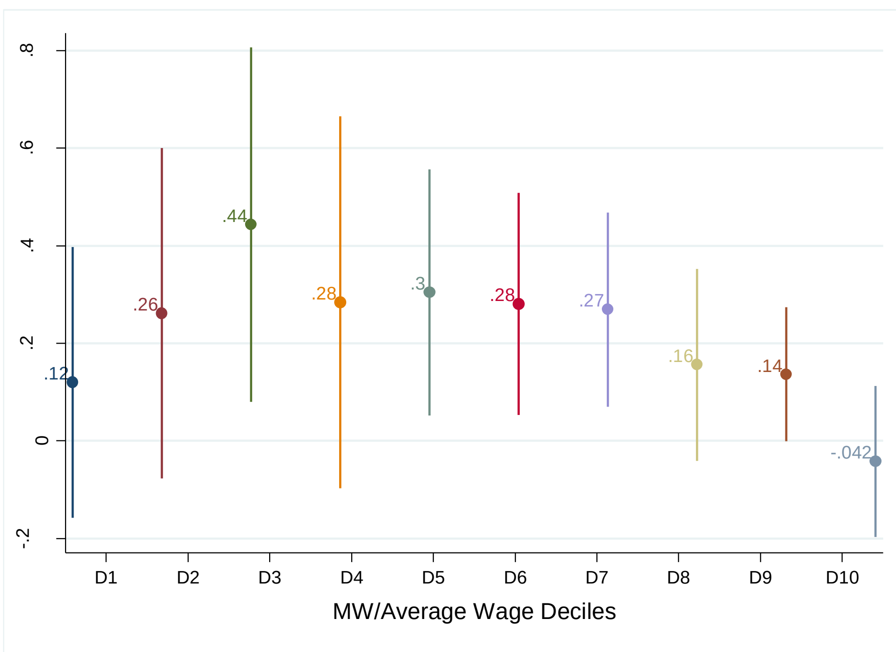
[61] I estimate the same figures for different levels of the HHI and low mobility, and for all the HHI cluster classifications. Results are very similar at higher levels of monopsony. Results available upon request.

and insignificant. The elasticity is close to zero because, at low levels, an increase in the minimum wage has almost a null effect on employment. In contrast, at the second decile of bindingness, the minimum wage has a more substantial positive and a significant effect on teenage employment. As the minimum wage bites the average wage to a greater extent, elasticity gets smaller, which means that the minimum wage is getting closer to the equilibrium wage. In both monopsony variables, the elasticity became negative in the 10^{th} decile, but it is insignificant.

3.6.4 Robustness Tests

In this section, I test for different issues that affect the main results. First, I test if the results hold if I use different classification to form the clusters for the HHI. Second, I test for a potential omitted variable. The effects of the minimum wage and monopsony on teenage employment are robust to different minimum wages' elasticities depending on the industry and its market power. Third, I test for possible external shocks that affect both the employment and the HHI.

(1) HHI



(2) Low Mobility

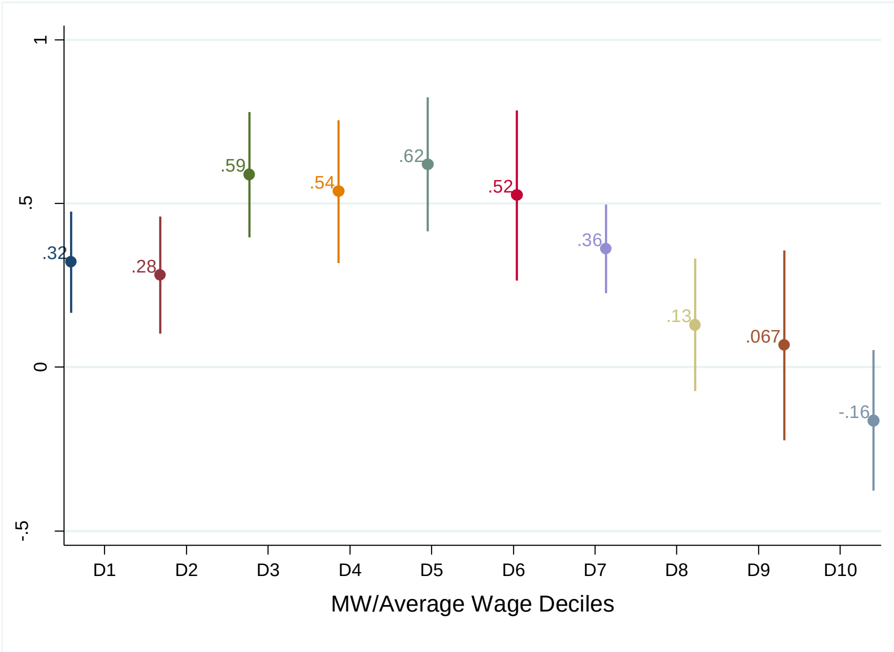


Figure 3.4 Effect of the Minimum Wage under Monopsony by Deciles

Note: I calculate MW/Average Wage and split the estimation in deciles. The higher the decile, the more binding is the minimum wage. All the estimations are evaluated with HHI or mobility equal to one. HHI=1 indicates full concentration. Mobility=1 implies that the worker remains in the same industry for all the periods.

Testing Different Cluster Classifications

In Table 3.5, I estimate elasticities of the minimum wage interacted with the HHI as in Table 3.3, but instead, I used different classification methods for the clusters. Column (1) defines the labor market as a 3-digit code of NAICS and counties; the assumption is that workers cannot move (or at least that it is difficult to do so) to other industries and counties, and the NAICS code defines their labor market. Column (2) defines the labor market as the flexible classification, which allows all the links, creating only two big clusters. Column (3) defines the labor market with the "top pairs" classification that only allows to form clusters of the two more connected industries.

The elasticities are very consistent with those in Table 3.3. The HHI coefficient is negative and significant, and the interaction is positive and significant. For all the cases, if the HHI = 0, the minimum wage effect on the employment is negative and higher than the average in the literature. Also, if the HHI=1, the effect on employment is positive and insignificant.

Table 3.5 Robustness Check: Effects of the Log of the MW Interacted with All the Classifications of Clusters for the HHI on the Log of Teenage Employment

Dependent Variable: Ln (Teen Emp)	(1) NAICS	(2) Flexible	(3) Top Pairs
Monopsony Variable (HHI or LM)	-0.972***	-0.920***	-0.998**
	(0.341)	(0.336)	(0.377)
Monopsony x Ln (MW)	0.527***	0.504**	0.523**
	(0.189)	(0.191)	(0.204)
Elasticity of the MW depending on Monopsony			
Monopsony = 0	-0.458***	-0.438***	-0.461***
	(0.133)	(0.125)	(0.145)
Monopsony = 0.2	-0.353***	-0.338***	-0.357***
	(0.109)	(0.102)	(0.117)
Monopsony = 0.4	-0.247***	-0.237***	-0.252**
	(0.0958)	(0.0912)	(0.0990)
Monopsony = 0.6	-0.142	-0.136	-0.147
	(0.0961)	(0.0952)	(0.0959)
Monopsony = 0.8	-0.0365	-0.0354	-0.0427
	(0.110)	(0.113)	(0.109)
Monopsony = 1	0.0690	0.0653	0.0619
	(0.134)	(0.139)	(0.134)
Constant	-0.119	-0.140	-0.0320
	(0.708)	(0.711)	(0.717)
Observations	199,231	199,231	199,231
R-squared	0.988	0.988	0.988

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1
Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, log of teenage population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 means full concentration. Column (1) defines the labor market by 3-digit NAICS code. In column (2), the cluster is defined by all the links; for instance, if industry A is connected to industry B, and industry B is connected to industry C, then A and C are connected. Column (3) only considers as a cluster the pair of industries with more relative flows between each other. See Appendix B for more details.

Testing for Possible Omitted Variable

My analysis is centered on heterogenous minimum wage effects at the county level of concentration, but one concern is that the effects are different depending on the industry. It is possible that the HHI partly reflects product market power, and the interaction effect of the minimum wage with the HHI might not reflect only monopsony. There is a potential omitted variable (product market power interacted with minimum wages) that correlates with the interaction of HHI and the minimum wage. Thus, including the interaction of industry with minimum wage accounts for this.

In Table 3.6, I estimate the effects with an interaction of the minimum wage and industries to allow for different effects by industry, with three-way fixed effects (time, county, and industry). I present the specification using HHI and labor mobility first, but I also add the other three different measures of HHI to see if the results are consistent. In addition, instead of reporting all the coefficients for each industry interaction, I report only the minimum wage average effect (i.e., evaluating at the average value of all the variables including the dummies and with HHI=0).

Table 3.6 Effects of the Log of the MW Interacted with the HHI and Low Mobility on the Log of Teenage Employment, Allowing Different Effects of MW by Industry

Dependent Variable: Ln (Teen Emp)	(1) HHI	(2) Low Mobility	(3) NAICS	(4) Flexible	(5) Top Pairs
Ln (MW)	-0.233	0.0907	-0.160	-0.190	-0.142
	(0.130)	(0.261)	(0.129)	(0.133)	(0.140)
HHI or Low Mobility	-0.714***	-0.119*	-0.585***	-0.712***	-0.335***
	(0.120)	(0.0691)	(0.117)	(0.126)	(0.121)
HHI or Low Mobility x Ln (MW)	0.389***	0.0685*	0.314***	0.365***	0.257***
	(0.0615)	(0.0357)	(0.0602)	(0.0625)	(0.0654)
Constant	-0.496	-7.681**	0.384	1.147***	-1.400***
	(0.333)	(3.136)	(0.371)	(0.350)	(0.370)
Observations	2,201,021	18,001	1,954,252	1,921,138	2,603,089
R-squared	0.818	0.970	0.831	0.831	0.793

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1
Notes: All specifications include three-way fixed effects (county, time, and industry). Control variables are the log of the total population, log of teenage population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 means full concentration. In addition, all the specifications include interactions of Ln (MW) by industry. The coefficient reported for Ln (MW) is the effect evaluated in the average of each industry for HHI=0. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more relative flows of workers (top three links with highest flows with more than 90th percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. Column (3) defines the labor market by 3-digit NAICS code. In column (4), the cluster is defined by all the links; for instance, if industry A is connected to industry B, and industry B is connected to industry C, then A and C are connected. Column (5) only considers as a cluster the pair of industries with more relative flows between each other. See Section 4 for more details.

The interaction of the monopsony variable and minimum wages is still positive and significant for all the measures of HHI and for the labor mobility, which indicates that the results are very robust, even when controlling by the possible bias of the markup and using three-way fixed effects. The first row shows the average effect of the minimum wage under perfect competition (all the industrial dummies are evaluated in the mean, and the control variables as well). The minimum wage is negative and insignificant. It is not significant, perhaps because the data are at the industry level, and the effects vary considerably among industries.

Possible Multicollinearity and External Shocks

Two possible concerns about the estimations are that the HHI or the mobility are correlated with the minimum wage. One is that the minimum wage may be correlated with the HHI or the labor mobility (because it can affect the employment level and the flows). The data suggest it is not the case because there is no significant relationship between HHI-mobility and the minimum wage (See Table A9 in the Appendix).

Table 3.7 Effects of the Log of the MW Interacted with the HHI and Low Mobility on the Log of Teenage Employment, Average of the HHI in Different Periods

Panel A: Using the period average

Dependent Variable: Ln (Emp)	(1) HHI	(2) Low Mobility
Ln (MW)	-0.501***	-1.123***
	(0.142)	(0.192)
HHI or Mobility (average) x Ln (MW)	0.575**	1.905***
	(0.238)	(0.376)
Constant	-0.846	-2.918**
	(0.780)	(1.098)
Observations	200,052	26,657
R-squared	0.988	0.988

Panel B: Using the average from 2000-2001

Dependent Variable: Ln (Emp)	(1) HHI	(2) Low Mobility
Ln (MW)	-0.306**	-0.999***
	(0.131)	(0.294)
HHI or Mobility (average) x Ln (MW)	0.330	1.711***
	(0.200)	(0.514)
Constant	-0.967	-2.906
	(0.930)	(1.896)
Observations	168,219	14,036
R-squared	0.988	0.988

Robust clustered standard errors in parentheses by states.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, log of teenage population, and total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 means full concentration. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more relative flows of workers (top three links with highest flows with more than 90^{th} percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. See Section 4 for more details. The HHI and mobility do not vary over time; thus, the coefficients are dropped due to collinearity with time. In Panel A uses HHI average of all the period (2000–2016) and in Panel B the average from 2000 to 2001.

Another concern more specific to the HHI is that it can be affected by an external shock. For instance, suppose some firms are closed in an area due to a shock. Then, HHI will rise, and employment will be lower; thus, it is not possible to attribute the effect to the HHI. To deal with this problem, I follow two approaches. First, I estimate the coefficients of Table 3.3, but instead, I use the period average of the HHI. Second, I estimate the average HHI[62] for the first two years (2000 and 2001), and I then use this average to calculate the estimates over the period 2002–2016. These approaches reduce not only the possible bias of external shocks, but also the possible effects of the minimum wage on HHI.

In Table 3.7, I present the results for both approaches. Panel A uses the average of the monopsony variables during 2000–2016, and Panel B during 2000–2001, while the regressions are for the period 2002–2016. Note that the HHI variable is not included because it does not vary in time, so it is collinear with the fixed effects. In both specifications, the results are very similar to the ones in Table 3.3. Hence, in general, we can disregard the external shocks as explaining the effect of the HHI on employment. The main conclusions are still valid, and the monopsony proxies explain the heterogeneous effects of the minimum wage on employment.

3.7 Conclusions

The effects of minimum wages have been controversial, and there are a considerable number of papers that find no negative effects on employment. These papers propose monopsony as one plausible explanation. This paper contributes to the minimum wage literature by focusing on estimating minimum wages effects when the labor markets are far from competitive.

I identify these effects by estimating the impact of the policy on the employment of teenage workers, which is a group that is more likely to be affected.

The main finding of the paper is that minimum wages have mixed effects on employment depending on the degree of monopsony of the labor markets and its equilibrium wage. As theory predicts, minimum wages have negative effects on competitive areas, but they have positive effects on the more monopsonistic areas (where firms have the power to set the wage). However, the range where minimum wages have positive effects is relatively small.

Another contribution is the estimation of the effect on monopsonistic labor markets for different levels of the minimum wage. As predicted in the monopsony model, when firms are unconstrained because the minimum wage is not binding, there are insignificant effects. In contrast, increases in minimum wages have positive effects on employment at higher levels of monopsony in

[62] For robustness, I also added a column for labor mobility. However, it is hard to think of a scenario wherein a shock affects the labor mobility and the employment at the same time.

firms that are supply-constrained Also, at a very high level of bindingness, the minimum wage has negative and insignificant effects on employment, even if there is high concentration or low labor mobility.

There are some areas of potential improvement for this paper. For instance, using data at the firm level could enhance the precision of the estimates because the measurement of concentration would be more accurate. Additionally, having firm-level data would allow the estimation of the supply elasticity to the firm, which is a more direct measure of monopsony. These two issues do not bias the results significantly. The effects of the HHI on wages in this paper are consistent with the literature that uses HHIs calculated directly from firms.

Moreover, the industry clusters for labor markets relax the assumption that workers can only work in one sector and allow workers to move to other industries. In other words, the labor market is determined by a cluster of industries that demand similar skills from workers instead of assuming that an occupation or an industry determines the labor supply. The HHI and labor mobility are close measurements of monopsony; therefore, results are still valid.

This paper's results suggest that minimum wages can be tied closely to local labor market conditions. Usually, minimum wages are set in large areas without taking into consideration the local labor market. In the U.S., minimum wages are set by the state and federal level, but in many countries, the policy is implemented nationwide. This is important because a minimum wage policy can improve and correct market problems like monopsonies, but it is also important to be aware that it can hurt workers in more competitive areas.

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Appendix A: Figures

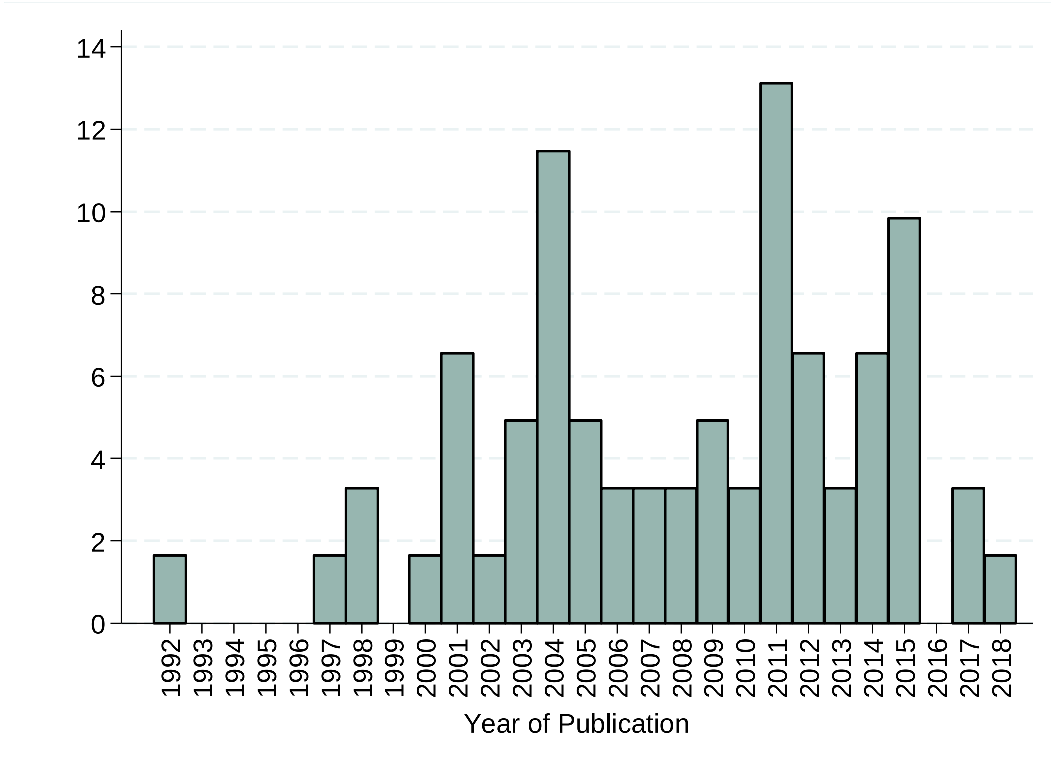
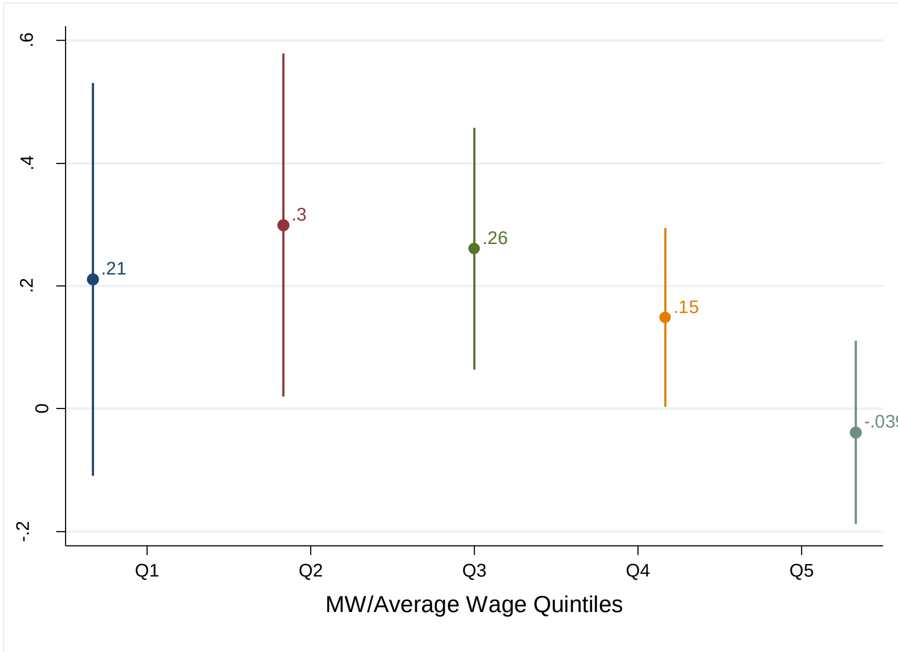


Figure A1: Histogram of Surveyed Studies by Year

(1) HHI



(2) Low Mobility

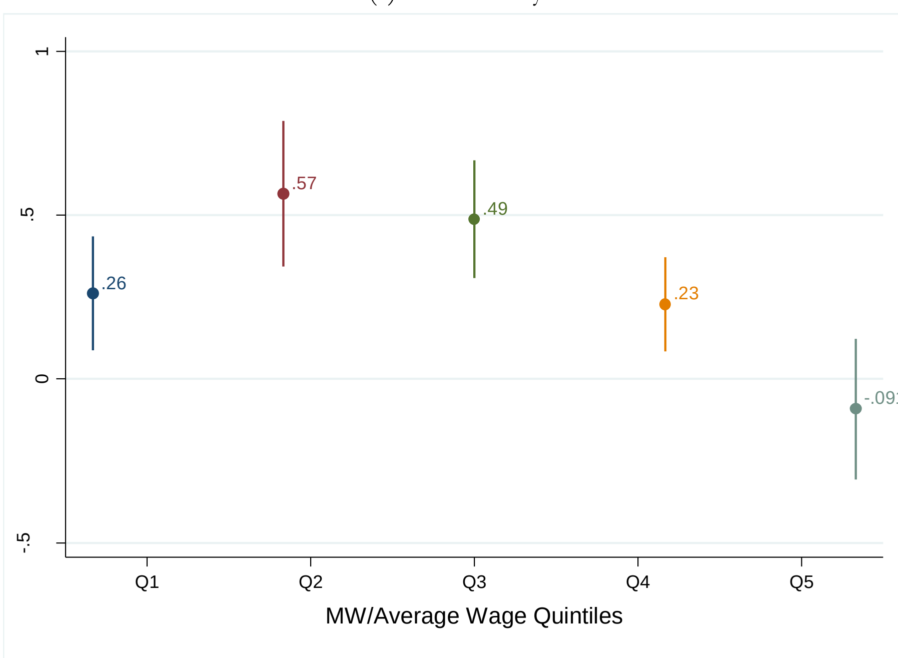


Figure A2. Effect of the Minimum Wage under Monopsony by Quintiles

Note: I calculate MW/Average Wage and split the estimation in quintiles. The higher the quintile, the more binding the minimum wage is. All the estimations are evaluated with HHI or mobility equal to one. HHI=1 indicates full concentration. Mobility=1 implies that the worker remains in the same industry for all the periods.

Appendix B: Additional Tables

Table A1: Classification of Countries' Labor Codes and Minimum Wage Laws

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices
Afghanistan	1	0	0	None	(7) No enforcement	Only MW for the government workers, there is not enforcement
Albania	0	1	0	Financial penalties	(1) FP	
Algeria	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Angola	1	0	0	None	(7) No enforcement	It supposed to be enforced in formal sector, but I didn't find law
Argentina	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Armenia	0	1	0	Financial penalties	(1) FP	Very weak enforcement (according to HR)
Azerbaijan	1	0	0	None	(7) no enforcement	Very weak enforcement (according to HR)
Bangladesh	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	Not effective enforcement
Belarus	0	1	0	Financial penalties	(1) FP	Very weak enforcement and very poor results
Belize	0	1	0	Financial penalties	(1) FP	Not effective enforcement. Some industries do not pay MW. No more than 150 dollars per offense
Benin	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	Good enforcement. Fined 90 times the MW per violation.
Bhutan	0	1	0	Financial penalties per worker or infraction	(2) FP and Infraction per Worker	
Bolivia	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	
Bosnia and Herzegovina	0	1	0	Financial penalties	(1) FP	
Botswana	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Brazil	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Bulgaria	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	High fines of enforcement (from 840 to $8400), still a lot of violations according to unions.
Burkina Faso	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Burundi	0	1	0	Financial penalties	(3) FP increase if repeat	Low enforcement, fines are low (3 to 12$).
Cambodia	1	0	0	None	(7) No enforcement	Not effective enforcement. It only applies to garment workers.
Cameroon	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Cape Verde	0	1	0	Financial penalties	(1) FP	Between $100 to $1,815. Good law enforcement.
Central African Republic	0	1	0	Financial penalties	(1) FP	
Chad	0	1	0	Financial penalties	(1) FP	Weak enforcement, too little inspectors.

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices
China	0	0	1	Financial penalties, suspension of business operations or rescission of business certificates and licenses	(5) FP, shut down of company	Not enough inspectors to enforce the law.
Colombia	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Comoros	1	0	0	None	(7) No enforcement	MW is only enforced by unions, but not the government
Congo	1	0	0	None	(7) No enforcement	
Congo, Democratic Republic of the	1	0	0	None	(7) No enforcement	MW has not changed since 2005, it is very low. Very weak enforcement.
Costa Rica	0	1	0	Financial penalties	(1) FP	
Croatia	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Cuba	0	0	1	Penalties with imprison for individuals and fines for corps	(4) FP and Imprison	
CÃ´te d'Ivoire	1	0	0	None	(7) No enforcement	Government enforce MW only with public workers, unions help in private. Very weak enforcement.
Djibouti	1	0	0	None	(7) No enforcement	Law weakly enforcement. MW is only por public workers, and private agreements between workers and company.
Dominican Republic	0	1	0	Financial penalties	(1) FP	
Ecuador	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	The Ministry of Labor continued its labor rights enforcement reforms by increasing labor inspections and increasing the number of workers protected by contracts. Extra benefits around 40% are mandatory to be payed to workers.
Egypt	1	0	0	None	(7) No enforcement	Just apply to government workers (working directly)
El Salvador	0	1	0	Financial penalties	(1) FP	
Equatorial Guinea	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	No sufficient inspector to enforce the MW effectively. Fine: 10 months of MW
Eritrea	1	0	0	None	(7) No enforcement	Only MW for the government workers, there is not enforcement
Ethiopia	1	0	0	None	(7) No enforcement	Some government institutions have MW. Law is not effective.
Fiji	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	Wear enforcement of the law, but high fines
Gabon	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	
Gambia	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	Effective enforcement, only 20% formal.
Georgia	1	0	0	None	(7) No enforcement	No legal framework for labor inspection
Ghana	1	0	0	None	(7) No enforcement	MW is below extreme poverty necessary income. There was widespread violation of the minimum wage law.
Guatemala	0	0	1	Penalties with imprison for individuals and fines for corps	(4) FP and Imprison	

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices
Guinea	1	0	0	None	(7) No enforcement	MW only for domestic workers. Extremely weak law enforcement
Guinea-Bissau	1	0	0	None	(7) No enforcement	80% is informal economy. Weak enforcement.
Guyana	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Haiti	0	1	0	Financial penalties	(1) FP	Fines between $19 and $57. No records or prosecutions for MW violations.
Honduras	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
India	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Indonesia	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Iran, Islamic Republic of	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Iraq	0	1	0	Financial penalties	(1) FP	
Jamaica	1	0	0	None	(7) No enforcement	
Jordan	0	1	0	Financial penalties	(1) FP	MW way below poverty line. Very poor enforcement of the law.
Kazakhstan	1	0	0	None	(7) No enforcement	Poor enforcement. Corruption (3 years without inspection some companies)
Kenya	1	0	0	None	(7) No enforcement	Very few inspectors.
Kyrgyzstan	0	1	0	Financial penalties	(1) FP	Fines between 7.2 and $70. Poor enforcement of the law.
Lao People's Democratic Republic	1	0	0	None	(7) No enforcement	The law is not always enforced.
Lebanon	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Lesotho	1	0	0	None	(7) No enforcement	Labor laws do not cover workers in agriculture or other informal sectors. Medium enforcement.
Liberia	0	1	0	Financial penalties	(1) FP	Enforcement of standards and inspection findings was not always consistent. Medium Enforcement. The process to persecute is very slow. Fines less than 500 local currency
Libya	1	0	0	None	(7) No enforcement	Huge number of informal foreign workers. No information available about inspectors.
Macedonia	0	1	0	Financial penalties	(1) FP	MW not effectively enforce.
Madagascar	1	0	0	None	(7) No enforcement	MW is not always enforced.
Malawi	1	0	0	None	(7) No enforcement	No effectively enforce.
Malaysia	0	0	1	Penalties with imprison for individuals and very high fines	(4) FP and Imprison	MW is less than poverty line. Many firms never get inspected. 22,500 per day they do not pay MW
Maldives	1	0	0	None	(7) No enforcement	
Mali	1	0	0	None	(7) No enforcement	Weak enforcement of the law
Mauritania	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices
Mauritius	0	1	0	Financial penalties	(1) FP	Medium enforcement, penalties not deter, not biding
Mexico	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	Imperfect Law
Moldova, Republic of	0	1	0	Financial penalties	(1) FP	MW below poverty line. Good enforcement of the MW law. Penalties between $250 and $1,200.
Mongolia	0	1	0	Financial penalties	(1) FP	MW below poverty line. Low number of inspectors.
Montenegro	0	1	0	Financial penalties	(1) FP	MW sometimes not payed. Long waiting time for resolutions.
Morocco	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Mozambique	0	1	0	Financial penalties	(1) FP	
Myanmar	0	0	1	Financial penalties and or imprison	(4) FP and Imprison	Don't cover businesses with fewer than 15 employees. Frequent violations occurred in private enterprises.
Namibia	1	0	0	None	(7) no enforcement	Only for domestic workers, unions help setting MWs but is not in the law.
Nepal	1	0	0	None	(7) no enforcement	Barely sufficient to meet subsistence needs. 10% formal. Applies for informal, but enforcement stronger in formal. Only 12 inspectors in the whole country.
Nicaragua	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	
Niger	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Nigeria	0	1	0	Financial penalties	(1) FP	
Pakistan	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Panama	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Papua New Guinea	0	1	0	Financial penalties	(1) FP	Inspections only by request. Insufficient to enforce comply. Penalties very low ($32).
Paraguay	0	1	0	Financial penalties	(1) FP	
Peru	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Philippines	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Romania	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	MW way below poverty line. Not effectively enforce.
Russian Federation	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	
Rwanda	1	0	0	None	(7) no enforcement	MW were not enforced.
Saint Lucia	0	1	0	Financial penalties	(1) FP	
Saint Vincent and the Grenadines	1	0	0	None	(7) no enforcement	Not binding.
Samoa	1	0	0	None	(7) no enforcement	75% informal.

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices
Sao Tome and Principe	1	0	0	None	(7) no enforcement	Enforcement of the standards seldom occurred
Senegal	1	0	0	None	(7) no enforcement	Ministry of Labor and Unions enforce MW. MW also covers the informal sector but was not enforced.
Serbia	1	0	0	None	(7) no enforcement	Companies with a trade union presence generally enforced MW. Smaller companies, not enforced.
Sierra Leone	0	1	0	Financial penalties	(1) FP	MW for informal sector too. Below poverty line. Lack of Enforcement. 91.9% informal economy.
Solomon Islands	1	0	0	None	(7) no enforcement	MW below extreme poverty line. Independent judiciary helped provide effective enforcement of labor laws
Somalia	1	0	0	None	(7) no enforcement	No enforcement of the law.
South Africa	0	1	0	Financial penalties increased in case repeated offence	(3) FP increase if repeat	
Sri Lanka	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	Authorities did not effectively enforce MW.
Sudan	1	0	0	None	(7) no enforcement	Only in the public sector. Private is set with bargaining
Suriname	0	1	0	Financial penalties	(1) FP	
Swaziland	1	0	0	None	(7) no enforcement	MW by industry. In general, good enforcement of the law
Syrian Arab Republic	0	1	0	Financial penalties	(1) FP	Little information about the enforcement of the law.
Tajikistan	0	1	0	Financial Penalties	(1) FP	Regulations are not enforced.
Tanzania, United Republic of	1	0	0	None	(7) no enforcement	No problems of enforcement, but law are international conventions
Thailand	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	MW way below poverty line. Enforcement is different per sector. Enforcement of MW was inconsistent.
Timor-Leste	0	1	0	Financial penalties	(1) FP	
Togo	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	
Tonga	1	0	0	None	(7) no enforcement	No MW law, just guidelines.
Tunisia	0	1	0	Financial penalties per workers or infraction	(2) FP and Infraction per Worker	
Turkey	0	1	0	Financial penalties	(1) FP	Government did not effectively enforce MW.
Turkmenistan	1	0	0	None	(7) no enforcement	Weak enforcement
Uganda	1	0	0	None	(7) no enforcement	Very low MW, they didn't change it in years. No enforcement of law.
Ukraine	0	1	0	Financial penalties	(1) FP	No effective enforcement of MW. Penalties impossible to verify effectiveness because there is not inspection.
Uzbekistan	1	0	0	None	(7) no enforcement	Only MW in the public sector
Vanuatu	0	0	1	Financial penalties per workers or infraction, imprison in case repeated	(4) FP and Imprison	Good enforcement, most firms complied.
Venezuela	1	0	0	Financial penalties	(1) FP	
Viet Nam	0	0	1	Financial penalties increased in case repeated offence, shut down, no subsidies	(5) FP, shut down of company	

Country	None	Weak	Strong	Type Penalty	ILO classification group	Comments from Country Reports on Human Rights Practices
Yemen	1	0	0	None	(7) no enforcement	MW only in the public sector.
Zambia	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	
Zimbabwe	1	0	0	Employers violating collective labor agreements (no necessary MW)	(6) No punishment but presence CB	

Table A2. Effect of Minimum Wages on Log of Total, Unskilled, Young Adult, and Female Employment Rates–Countries Grouped by Different Degree of Enforcement, Other Classification of Enforcement Based on Human Right Practices

Dependent Variable: Ln (Emp Rate)	(1)	(2)	(3)	(4)	(5)	(6)
	Total			Unskilled		
	No enforcement	Weak Enforcement	Strong Enforcement	No enforcement	Weak Enforcement	Strong Enforcement
Kaitz Index	-0.0200	0.0232	-0.0330	-0.0622	0.0688*	-0.00966
	(0.0382)	(0.0332)	(0.0330)	(0.0450)	(0.0343)	(0.0245)
Elasticities	-0.00573	0.00964	-0.0158	-0.0178	0.0285**	-0.00461
	(0.0109)	(0.0138)	(0.0158)	(0.0129)	(0.0142)	(0.0117)
Observations	277	414	157	277	414	157
R-squared	0.984	0.985	0.995	0.984	0.982	0.992
Number of Countries	30	35	17	30	35	17
Dependent Variable: Ln (Emp Rate)	(7)	(8)	(9)	(10)	(11)	(12)
	Young Adults			Female		
	No enforcement	Weak Enforcement	Strong Enforcement	No enforcement	Weak Enforcement	Strong Enforcement
Kaitz Index	-0.0681	0.0250	-0.0765	0.0300	0.0399	-0.100
	(0.114)	(0.0634)	(0.0624)	(0.0641)	(0.0587)	(0.0670)
Elasticities	-0.0195	0.0104	-0.0365	0.00859	0.0166	-0.048
	(0.0327)	(0.0263)	(0.0298)	(0.0183)	(0.0243)	(0.032)
Observations	277	414	157	277	414	157
R-squared	0.980	0.978	0.972	0.993	0.992	0.996
Number of Countries	30	35	17	30	35	17

Robust standard errors in parentheses clustered by countries
*** p<0.01, ** p<0.05, * p<0.1

Notes: Employment is in logs. Kaitz Index = (MW/Avg Wage). The panel is unbalanced, and it includes 82 countries (grouped in different samples), period 1994–2016, and 10.7 years on average. The incremental R^2 for country-specific trends is explaining around 0.08 for total employment, 0.10 for unskilled employment, between 0.15 to 0.5 for teenagers, and 0.29 for female workers. Two-way fixed effects and linear country-specific trends are used (specifications with other polynomial trends are available upon request). Unskilled employment is calculating using low and medium skill classifications (which include painters, carpenter, blue-collar workers among others), young adults are workers between 15 and 24 years old. Controls for total and unskilled workers includes log of population, log of the relative size of youth to the population for young adult workers, and log of the relative size of the female to the population for female workers. Log of the GDP and log of one lag of the GDP are included for all specifications. The classification of enforcement uses the report of Human Right Practices instead.

Table A3. Calculated Elasticities for Studies Not Estimating Elasticities

Study	Country	Minimum wage variable	Period	Avg. empl. rate	Avg. MW	Comments
Alaniz et al. (2011)	Nicaragua	ln(MW)	1998-2006	Varies by group	--	The paper provides the total number of workers, the proportion of each group in the total, and the sample size including the non-employed. We use this information to calculate the employment rate by group.
Arango and Pachón (2004)	Colombia	MW	1984-2001	0.74	202,778.4	The minimum wage variable is the ratio (minimum wage)/(median income), so the elasticity calculation requires the mean of this variable. We do not have that, but we have median income from the paper, and obtain the average minimum wage from ILO, for the period 1991-2001. The paper estimates the effects on paid and self-employed workers. We calculate the employment rate from Table 2, which reports the number of paid and self-employed workers and the total sample including non-workers.
Baranowska-Rataj and Magda (2015)	Poland	ln(MW)	2003-2011	0.78 for total, varies for the rest of the groups	--	We estimate the average employment rate by group to retrieve the elasticity. The paper reports total employment, the shares in each category (gender, type of worker, etc.), and the sample size.
Bhorat et al. (2014)	South Africa	ln(MW)	2000-2007	0.40	--	This paper studies the share of workers by industry. We calculate the average number of workers in the treatment (Table 1) and in the control (Table 2) per year and calculate the average employment rate (Treatment/Control+Treatment).
Carneiro and Corseuil (2001)	Brazil	ln(MW)	1995-1999	Varies by year	--	We use ILOSTAT data to calculate average the formal employment rate by year. We do not have data on informal employment in the same range of years, but we have the ratio of formal to informal employment and use this ratio to estimate employment by sector. The formal to informal ratio is estimated with 2009 data (the first year reported in ILO for Brazil), so we are assuming that this ratio was the same in the sample period.
Del Carpio et al. (2014)	Thailand	ln(MW)	1998-2010	Varies by group. For the total is 0.71.	--	We use information from ILOSTAT to calculate the employment rate by group. The average employment rate in this period for all workers is 0.71, and the rate varies across groups. We estimate employment rates by gender and age. However, we could not determine the rates by education level; thus, we applied the total employment rate (0.71) to retrieve the elasticity for education groups.
Dinkelman and Ranchhod (2012)	South Africa	ln(MW)	2001-2004	0.13	--	The paper reports the sample size and the number employed (Table 1). We the information to calculate the average employment rate.
Gindling and Terrell (2007)	Costa Rica	ln(MW)	1988–2000	0.625	--	We use data from Table 2 in the paper to estimate the average employment rate for total workers.
Grau and Landerretche (2011)	Chile	ln(MW)	1996-2005	0.91	--	We do not have enough information from the paper, so we use data from ILOSTAT for the corresponding period. We estimate the employment rate by dividing the number of employed workers by the working-age population.
Ham (2018)	Colombia	ln(MW)	1996-2000	0.97 total employment 0.95 formal 0.99 informal	--	The paper provides the employment rates by sector in Table 2.
Hohberg and	Indonesia	ln(MW)	1997-2007	0.664	--	The paper reports the employment rates in Table 1.

Study	Country	Minimum wage variable	Period	Avg. empl. rate	Avg. MW	Comments
Lay (2015)						
Maloney and Nuñez Mendez (2004)	Colombia	ln(MW)	1997-1999	--	--	The authors use dummies for brackets of the initial individual wage relative to the minimum wage, to estimate the impact of a change in the minimum wage throughout the wage distribution. Hence, the non-employed are not included, and they estimate the effect of the minimum wage on the share in each bracket. We use the shares in the brackets to retrieve the elasticity (Table 2). Also, the authors estimate and report an average employment elasticity of −0.15. (This is not stated in any table; it is a calculation reported by the authors in the results section.) We use the average elasticity calculated by the authors and our estimations of the elasticities by brackets.
Menon and Meulen Rodgers (2017)	India	ln(MW)	1983-2008	Varies by group	--	We use data from ILOSTAT to estimate the employment rate of female and male workers in India with information by region (urban and rural). We only have data from the period 1994-2010.
Montenegro and Pagés (2004)	Chile	ln(MW)	1960-1998	Varies by group	--	The paper gives the number of workers, but does not provide information on workers by age, skill level, and gender. We estimate the employment rate by group using information from ILOSTAT. The data are from 1998 only (we could not find data before this year).
Strobl and Walsh (2003)	Trinidad and Tobago	MW	1996-1998	294.3 males 167 females	7	The authors study the effect of the implementation of the minimum wage on bound vs. not bound workers, based on wages prior to the minimum wage, by sex, for small and large firms. For each category, they report the percent change in the wage bill if all workers are topped up to the minimum wage, which we use to compute the percent change in the wage for bound workers. And they report the raw baseline rate of job loss for low-wage (bound) workers, by sex. We use these for both small and large firms. Thus, the elasticity is calculated as the marginal effect on job loss, multiplied by the ratio of the proportional change in the wage bill divided by the rate of job loss.

Note: We are estimating the employment rate elasticities. For example, in Alaniz et al. (2011), the estimated effect of the log minimum wage on the probability of being employed is −0.31 for all workers. The paper reports an employment rate in the sample of 0.58, so the elasticity of −0.53 results from dividing −0.31 by 0.58.

Appendix Table A4. Surveyed Studies, Estimated and Calculated Elasticities, and Classifications of Estimates (Authors' Preferred Estimates)

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
Alaniz et al. (2011)	Nicaragua	-0.898***	Yes	Formal	Strong	Vulnerable	Coefficients are unique for the categories.
		-0.834	Yes	Formal	Strong	Vulnerable	
		-0.533***	Yes	Formal	Strong	All Workers	
Alatas and Cameron (2008)	Indonesia	-0.20	Yes	Informal	None	All Workers	Different time periods.
		-0.459***	Yes	Informal	None	All Workers	
		-0.016	Yes	Informal	None	All Workers	
		-0.16*	Yes	Informal	None	All Workers	
		0.037	Yes	Formal	None	All Workers	
		0.032	Yes	Formal	None	All Workers	
		-0.41*	Yes	Informal	None	All Workers	
Arango and Pachón (2004)	Colombia	-0.407**	Yes	Both	Weak	All Workers	Heads and non-heads of households.
		-1.205***	Yes	Both	Weak	All Workers	
Baranowska-Rataj and Magda (2015)	Poland	-0.186***	N.d.	Formal	Strong	All Workers	Coefficients are unique for the categories.
		-0.365***	N.d.	Formal	Strong	Vulnerable	
Bell (1997)	Mexico	-0.027	Yes	Formal	Weak	Vulnerable	Different econometric models: with and without time fixed effects.
	Colombia	-0.182	No	Formal	None	All Workers	
		-0.337***	Yes	Formal	Weak	All Workers	
		-0.033*	Yes	Formal	Weak	Vulnerable	
Bhorat et al. (2014)	South Africa	-0.130***	Yes	Formal	Weak	All Workers	Different econometric models: with and without covariates.
		-0.082	Yes	Formal	Weak	All Workers	
Broecke and Vandeweyer (2015)	Brazil	-0.022***	Yes	Both	Weak	All Workers	Different units: regions and individuals. Different econometric models: with and without lags; different fixed effects.
		-0.014	Yes	Both	Weak	Vulnerable	
		-0.047	Yes	Both	Weak	Vulnerable	
		-0.026	Yes	Both	Weak	Vulnerable	
Carneiro (2004)	Brazil	0.018**	N.d.	Informal	Weak	All Workers	Coefficients are unique for the categories
		-0.005	N.d.	Formal	Weak	All Workers	
Carneiro and Corseuil (2001)	Brazil	2.097	Yes	Formal	Weak	All Workers	Different time periods..
		-0.551	Yes	Informal	Weak	All Workers	.
		0	Yes	Informal	Weak	All Workers	
		-2.530	Yes	Formal	Weak	All Workers	
		1.185	Yes	Formal	Weak	All Workers	
		0.718	Yes	Informal	Weak	All Workers	
		0	Yes	Informal	Weak	All Workers	
		-0.055	Yes	Formal	Weak	All Workers	
		-0.178	Yes	Formal	Weak	All Workers	
		0.754	Yes	Informal	Weak	All Workers	
Castillo-Freeman and Freeman (1992)	Puerto Rico	-0.54***	Yes	Formal	None	All Workers	Different time periods. .
		-0.91***	Yes	Formal	None	All Workers	
Chun and Khor (2010)	Indonesia	-0.112**	Yes	Formal	None	All Workers	Coefficients are unique for the categories.
		-0.027	Yes	Formal	None	Vulnerable	
Comola and Mello (2011)	Indonesia	0.087***	N.d.	Informal	None	All Workers	Different econometric methods of estimation: OLS and SUR.
		-0.053	N.d.	Formal	None	All Workers	
		0.082***	N.d.	Informal	None	All Workers	
		-0.052***	N.d.	Formal	None	All Workers	
		-0.028***	N.d.	Formal	None	Vulnerable	
		0.027***	N.d.	Informal	None	Vulnerable	
Del Carpio et al. (2015)	Indonesia	-0.069***	Yes	Informal	None	Vulnerable	Different vulnerable groups: low-

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
		-0.196***	Yes	Informal	None	Vulnerable	education and female workers.
		-0.034**	Yes	Formal	None	All Workers	
		-0.026*	Yes	Informal	None	All Workers	
		-0.043	Yes	Informal	None	Vulnerable	
Del Carpio et al. (2014)	Thailand	-0.171***	Yes	Formal	Strong	Vulnerable	Coefficients are unique for the categories.
		-0.078**	Yes	Both	Strong	All Workers	
		-0.041	Yes	Both	Strong	Vulnerable	
		-0.011	Yes	Formal	Strong	All Workers	
Dinkelman and Ranchhod (2012)	South Africa	-0.138	Yes	Formal	Weak	Vulnerable	Different econometric models: with and without covariates.
		-0.192	Yes	Formal	Weak	Vulnerable	
Dung (2017)	Vietnam	-0.527**	No	Both	None	All Workers	Different sectors. Type of workers: part-time and full-time.
		-0.157	No	Both	None	All Workers	
		-0.614***	No	Both	None	All Workers	
		-0.216*	No	Both	None	All Workers	
Fajnzylber (2001)	Brazil	-0.05***	Yes	Informal	Weak	Vulnerable	Different econometric models: with and without lags (formal): long-run and short-run (informal).
		-0.08***	Yes	Formal	Weak	Vulnerable	
		-0.05***	Yes	Formal	Weak	Vulnerable	
		-0.15***	Yes	Informal	Weak	Vulnerable	
		-0.10***	Yes	Formal	Weak	Vulnerable	
		-0.25***	Yes	Informal	Weak	Vulnerable	
		-0.35***	Yes	Informal	Weak	Vulnerable	
Fang and Lin (2015)	China	-0.148***	Yes	Formal	Strong	Vulnerable	Different vulnerable groups: females, young adults, and low-wage workers.
		-0.213*	Yes	Formal	Strong	Vulnerable	
		-0.088**	Yes	Formal	Strong	Vulnerable	
		-0.055***	Yes	Formal	Strong	All Workers	
		-0.062	Yes	Formal	Strong	Vulnerable	
Feliciano (1998)	Mexico	-0.406**	N.d.	Formal	None	Vulnerable	Different econometric models: with and without covariates, and OLS or IV.
		-0.522***	N.d.	Formal	None	Vulnerable	
		-1.107***	N.d.	Formal	None	Vulnerable	
		-0.074	N.d.	Formal	None	All Workers	
		0.005	N.d.	Formal	None	All Workers	
		0.014	N.d.	Formal	None	All Workers	
		-0.426***	N.d.	Formal	None	Vulnerable	
		-1.13**	N.d.	Formal	None	Vulnerable	
Foguel (1998)	Brazil	-0.135***	N.d.	Both	Weak	All Workers	Coefficients are unique for the categories.
		0.60***	N.d.	Informal	Weak	All Workers	
Foguel et al. (2001)	Brazil	0.018	N.d.	Informal	Weak	All Workers	Coefficients are unique for the categories.
		-0.011*	N.d.	Formal	Weak	All Workers	
Garza Cantú and Bazaldúa (2002)	Mexico	0.754***	N.d.	Formal	None	Vulnerable	Coefficients are unique for the categories.
		-0.204**	N.d.	Formal	None	All Workers	
Gindling and Terrell (2007)	Costa Rica	-0.109*	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
Gindling and Terrell (2008)	Honduras	-0.458***	Yes	Formal	Weak	All Workers	Large and small firms.
		0.392*	Yes	Formal	Weak	All Workers	
Grau and Landerretche (2011)	Chile	-0.312***	Yes	Both	Strong	Vulnerable	Different interactions.
		-0.339***	Yes	Both	Strong	Vulnerable	
Ham (2018)	Honduras	-0.471***	Yes	Formal	Weak	All Workers	Different econometric models: probit and multinomial logit
		0.276***	Yes	Informal	Weak	All Workers	

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
		0.34***	Yes	Informal	Weak	All Workers	
		-0.088***	Yes	Both	Weak	All Workers	
		-0.111***	Yes	Both	Weak	All Workers	
		-0.383***	Yes	Formal	Weak	All Workers	
Harrison and Scorse (2010)	Indonesia	-0.125***	Yes	Both	None	All Workers	Different sectors: one excludes textiles.
		-0.116***	Yes	Formal	None	All Workers	
		-0.123***	Yes	Both	None	All Workers	
Hernandez Diaz and Pinzon Garcia (2006)	Colombia	-0.245	Yes	Formal	Weak	Vulnerable	Coefficients are unique for the categories.
		-0.207	Yes	Formal	Weak	All Workers	
Hernandez and Lasso (2003)	Colombia	0.154	N.d.	Both	Weak	Vulnerable	Different vulnerable groups: young and low-skilled workers.
		-0.219	N.d.	Both	Weak	All Workers	
		0.005	N.d.	Both	Weak	Vulnerable	
Hertz (2005)	South Africa	-0.33	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
		-0.46	Yes	Formal	Weak	Vulnerable	
Hohberg and Lay (2015)	Indonesia	-0.074***	No	Informal	None	All Workers	Coefficients are unique for the categories.
		0.090***	Yes	Formal	None	All Workers	
Huang et al. (2014)	China	-0.033***	Yes	Formal	Strong	All Workers	Different regions.
		-0.017***	Yes	Formal	Strong	All Workers	
		0.058***	Yes	Informal	Strong	All Workers	
		-0.017***	Yes	Formal	Strong	All Workers	
Islam and Nazara (2000)	Indonesia	-0.059***	N.d.	Formal	None	All Workers	Coefficients are unique for the categories.
Kamińska and Lewandowski (2015)	Poland	-0.027	Yes	Formal	Strong	Vulnerable	Different vulnerable groups: young and low-wage workers divided in: full-time and part-time, and temporary and permanent workers.
		-0.005	Yes	Formal	Strong	Vulnerable	
		-0.016***	Yes	Formal	Strong	Vulnerable	
		-0.010	Yes	Formal	Strong	Vulnerable	
		-0.06***	Yes	Formal	Strong	Vulnerable	
		-0.101***	Yes	Formal	Strong	Vulnerable	
		-0.049***	Yes	Formal	Strong	Vulnerable	
Lemos (2004a)	Brazil	0.004	Yes	Formal	Weak	All Workers	Different econometric models: dynamic and with covariates.
		0.003	Yes	Formal	Weak	All Workers	
		-0.038	Yes	Formal	Weak	All Workers	
Lemos (2004b)	Brazil	-0.001	Yes	Both	Weak	All Workers	Coefficients are unique for the categories.
Lemos (2004c)	Brazil	-0.001	Yes	Formal	Weak	All Workers	Different econometric models: with and without lags of employment.
		0.010	Yes	Informal	Weak	All Workers	
		-0.017***	Yes	Informal	Weak	All Workers	
		-0.004**	Yes	Formal	Weak	All Workers	
Lemos (2005a)	Brazil	0.012	Yes	Both	Weak	Vulnerable	Different econometric models and different estimation methods: with and without lags; OLS and IV.
		-0.009	Yes	Informal	Weak	All Workers	
		0.002	Yes	Formal	Weak	All Workers	
		-0.003	Yes	Formal	Weak	All Workers	
		-0.005	Yes	Both	Weak	Vulnerable	
		-0.003	Yes	Formal	Weak	All Workers	
		-0.029	Yes	Formal	Weak	All Workers	
		-0.004	Yes	Formal	Weak	All Workers	
		-0.002	Yes	Formal	Weak	All Workers	
		-0.021	Yes	Informal	Weak	All Workers	

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
		-0.003	Yes	Formal	Weak	All Workers	
Lemos (2005b)	Brazil	-0.005*	Yes	Both	Weak	All Workers	Coefficients are unique for the categories
Lemos (2007)	Brazil	0.002	Yes	Both	Weak	Vulnerable	Different econometric models all workers: lags and no lags. Different vulnerable groups: young adults and female workers.
		-0.001	Yes	Both	Weak	All Workers	
		0.002	Yes	Both	Weak	Vulnerable	
		0.003	Yes	Both	Weak	All Workers	
Lemos (2009a)	Brazil	-0.062	Yes	Formal	Weak	All Workers	Different models: lags and no lags; with covariates and without covariates.
		0.026	Yes	Informal	Weak	All Workers	
		0.177*	Yes	Informal	Weak	All Workers	
		-0.126*	Yes	Formal	Weak	All Workers	
		0.147	Yes	Informal	Weak	All Workers	
Lemos (2009b)	Brazil	-0.045***	Yes	Both	Weak	Vulnerable	Different vulnerable groups: young adults and the affected fraction of workers (based on low wages).
		-0.096	Yes	Both	Weak	Vulnerable	
		-0.073	Yes	Both	Weak	All Workers	
Luo et al. (2011)	China	0.109***	N.d.	Formal	Strong	All Workers	Different sectors: manufacturing, construction, and wholesale.
		-0.236***	N.d.	Formal	Strong	All Workers	
		0.134***	N.d.	Formal	Strong	All Workers	
Magruder (2013)	Indonesia	-0.218***	Yes	Informal	None	All Workers	Different type of workers: full-time and self-employed. Different distance in difference-in-differences estimates: 15 and 30 miles.
		-0.090***	Yes	Informal	None	All Workers	
		0.104**	Yes	Formal	None	All Workers	
		0.127***	Yes	Formal	None	All Workers	
Majchrowska and Zólkiewski (2012)	Polonia	-0.08***	N.d.	Formal	Strong	All Workers	Different econometric models: Arellano-Bond and Blundell-Bond. Different time periods.
		-0.10***	N.d.	Formal	Strong	All Workers	
		-0.27*	N.d.	Formal	Strong	Vulnerable	
		-0.50***	N.d.	Formal	Strong	Vulnerable	
		-0.47	N.d.	Formal	Strong	Vulnerable	
Maloney and Nuñez Mendez (2004)	Colombia	-0.524***	Yes	Formal	Weak	Vulnerable	Workers with different levels of income: Workers earning between 0 and 0.5 MW, 0 and 0.7 MW and 0.7, and 0.9 MW.
		-0.345***	Yes	Formal	Weak	Vulnerable	
		-0.432***	Yes	Informal	Weak	Vulnerable	
		-0.15***	Yes	Formal	Weak	All Workers	
		-0.367***	Yes	Informal	Weak	Vulnerable	
		-0.205***	Yes	Informal	Weak	Vulnerable	
		-0.683***	Yes	Formal	Weak	Vulnerable	
Martinez et al. (2001)	Chile	-0.01	N.d.	Formal	Strong	All Workers	Different econometric methods: OLS and Stock-Watson. Different periods.
		0.04	N.d.	Formal	Strong	All Workers	
Mayneris et al. (2014)	China	-0.045	Yes	Formal	Strong	All Workers	Different regions: with and without the periphery.
		0.162	Yes	Formal	Strong	All Workers	
Menon and Meulen Rodgers (2017)	India	-1.996	No	Both	None	Vulnerable	Different regions: rural and urban. Different sectors: all industries and other industries.
		0.792***	Yes	Formal	None	All Workers	
		0.767***	Yes	Both	None	All Workers	
		0.175	No	Both	None	All Workers	
		-2.231***	Yes	Informal	None	Vulnerable	
		0.051	Yes	Informal	None	All Workers	
		1.793***	Yes	Both	None	Vulnerable	
		2.073***	Yes	Formal	None	Vulnerable	
		-0.067	Yes	Formal	None	All Workers	
		-0.787***	Yes	Informal	None	All Workers	

Study	Country	Elasticity	Binding	Sector	Enforcement	Vulnerable	Comments
		2.194	Yes	Formal	None	Vulnerable	
		-2.183	Yes	Informal	None	Vulnerable	
Miranda (2013)	Chile	-0.36***	N.d.	Formal	Strong	All Workers	Different sectors: all goods and only "tradable" goods.
		-0.28***	N.d.	Formal	Strong	All Workers	
Montenegro and Pagés (2004)	Chile	0.140***	N.d.	Formal	Strong	Vulnerable	Different vulnerable groups: female and young workers.
		0.095***	N.d.	Formal	Strong	Vulnerable	
Neumark et al. (2006)	Brazil	0.068	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
		-0.012	Yes	Formal	Weak	Vulnerable	
Ni et al (2011)	China	-0.032	N.d.	Formal	Strong	Vulnerable	Coefficients are unique for the categories.
		0.098	N.d.	Formal	Strong	All Workers	
Papps (2012)	Turkey	0.004	Yes	Formal	Weak	All Workers	Coefficients are unique for the categories.
		0.001	Yes	Formal	Weak	All Workers	
		-0.002	Yes	Informal	Weak	All Workers	
Pelek (2011)	Turkey	0.182	Yes	Informal	Weak	All Workers	Different measurements of the minimum wage: Kaitz index, real, and fraction between 0.95 and 1.05 times the minimum wage.
		0.008	Yes	Formal	Weak	Vulnerable	
		0.149***	Yes	Informal	Weak	All Workers	
		0.022	Yes	Formal	Weak	Vulnerable	
		0.024	Yes	Formal	Weak	Vulnerable	
		-0.029	Yes	Informal	Weak	All Workers	
		0.008	Yes	Formal	Weak	All Workers	
		0.024	Yes	Formal	Weak	All Workers	
Shi (2011)	China	-0.587***	N.d.	Formal	Strong	All Workers	Different sectors: construction and manufacturing.
		-0.128	N.d.	Formal	Strong	All Workers	
Strobl and Walsh (2003)	Trinidad	-0.048**	Yes	Both	Strong	All Workers	Different firm sizes.
		-0.151*	Yes	Both	Strong	Vulnerable	
		-0.016	Yes	Both	Strong	Vulnerable	
		-0.036*	Yes	Both	Strong	All Workers	
Suryahadi et al. (2003)	Indonesia	-0.112***	Yes	Formal	None	All Workers	Different vulnerable groups: female and young workers.
		-0.307***	Yes	Formal	None	Vulnerable	
		-0.307***	Yes	Formal	None	Vulnerable	
Wang and Gunderson (2011)	China	-1.042**	N.d.	Formal	Strong	All Workers	Different regions. Different types of firms: state-owned and private.
		-0.202	N.d.	Formal	Strong	All Workers	
		-0.156	N.d.	Formal	Strong	All Workers	
		0.356*	N.d.	Formal	Strong	All Workers	
		-0.178	N.d.	Formal	Strong	All Workers	
		-0.225	N.d.	Formal	Strong	All Workers	
		0.166	N.d.	Formal	Strong	All Workers	
Wang and Gunderson (2012)	China	-0.510	No	Formal	Strong	Vulnerable	Effects for different sectors of the economy like construction, retail, etc.
		0.430	No	Formal	Strong	All Workers	
		-0.150	No	Formal	Strong	All Workers	
Xiao and Xiang (2009)	China	-0.022**	Yes	Both	Strong	All Workers	Different estimation methods: difference-in-differences and levels.
		-0.001***	Yes	Both	Strong	All Workers	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: Vulnerable workers are young adults, less-skilled workers, female workers, or workers earning very close to the minimum wage. Informal sector includes small firms for the case of Indonesia (as suggested in some papers). Binding is defined based on evidence of positive wage effects. Most analyses are for the formal section, while some papers report results for the informal sector or the two sectors combined. Enforcement is defined by penalties in the law, following Munguía Corella (2019). For studies for which we had to compute elasticities, we use the statistical significance of the reported employment effect. For Neumark et al. (2006), the estimate for household heads is classified as for all workers, and the estimate excluding the household head is classified as for vulnerable workers. For Strobl and Walsh (2003), the estimated elasticity for small firms, for men, is statistically significant. They also report a significant coefficient estimate for the interaction of the minimum wage variable with an indicator for large firms, for women. However, this estimate is not statistically significant, and we have no way of assessing the significance of the overall effect of the minimum wage for women working at large firms (which is this interaction plus the estimated minimum wage effect), so we do not code this estimate as statistically significant.

Appendix Table A5. Classification of Studies by Country and Bindingness

Country	Number of studies	Binding	Not binding	No data
Brazil	15	12	0	3
Chile	4	1	0	3
China	9	4	1	4
Colombia	5	4	0	1
Costa Rica	1	1	0	0
Honduras	2	2	0	0
India	1	0.8	0.2	0
Indonesia	9	6.5	0.5	2
Mexico	3	0	1	2
Nicaragua	1	1	0	0
Poland	3	1	0	2
Puerto Rico	1	1	0	0
South Africa	3	3	0	0
Thailand	1	1	0	0
Trinidad	1	1	0	0
Turkey	2	2	0	0
Vietnam	1	0	1	0

Notes: In the second through fourth columns, we average the number of results by study, and then we sum by country. The non-integers result when there is variation in bindingness across estimates in a study. For India (Menon and Meulen Rodgers, 2017), the minimum wage is non-binding in the urban areas, but it is binding in the rural areas. For Indonesia (Hohberg and Lay, 2015), the minimum wage is non-binding for the informal sector and binding for the formal sector.

Appendix Table A6. Numbers of Estimates for Sets of Estimate Covered in Figures 2.2-2.5

Two estimate features	Number of estimates	Three estimate features	Number of estimates	Four estimate features	Number of estimates
Both predict stronger negative effects		***All predict stronger negative effects***		***All predict stronger negative effects***	
Formal/Binding	91	Binding/Formal/Strong	22	Binding/Formal/Strong/Vulnerable	14
Strong/Binding	33	Binding/Formal/Vulnerable	35	***None predict stronger negative effects***	
Vulnerable/Binding	62	Binding/Strong/Vulnerable	19	Informal/Weak/Non-binding/All Workers	0
Strong/Formal	52	Formal/Strong/Vulnerable	22	Informal/Weak/No data (binding)/All Workers	3
Vulnerable/Formal	50	***None predict stronger negative effects***		Informal/None/Non-binding/All Workers	1
Vulnerable/Strong	27	Informal/Weak/Non-binding	0	Informal/None/No data (binding)/All Workers	2
One predicts stronger negative effects		Informal/Weak/No data (binding)	3	Both/Weak/Non-binding/All Workers	0
Binding/Informal	41	Informal/Weak/All Workers	21	Both/Weak/No data (binding)/All Workers	2
Binding/Both	33	Informal/None/Non-binding	1	Both/None/Non-binding/All Workers	5
Binding/Weak	95	Informal/None/No data (binding)	3	Both/None/No data (binding)/All Workers	0
Binding/None	37	Informal/None/All Workers	13		
Binding/All Workers	103	Informal/Non-binding/All Workers	1		
Formal/Weak	53	Informal/No data (binding)/All Workers	5		
Formal/None	37	Both/Weak/Non-binding	0		
Formal/Non-binding	4	Both/Weak/No data (binding)	4		
Formal/No data (binding)	43	Both/Weak/All Workers	12		
Strong/All Workers	36	Both/None/Non-binding	6		
Strong/Non-binding	3	Both/None/No data (binding)	0		
Strong/No data (binding)	27	Both/None/All Workers	8		
Strong/Informal	1	Both/Non-binding/All Workers	5		
Strong/Both	8	Both/No data (binding)/All Workers	2		
Vulnerable/Non-binding	2	None/Non-binding/All Workers	7		
Vulnerable/No data (binding)	17	None/No data (binding)/All Workers	9		
Vulnerable/Informal	13	Weak/Non-binding/All Workers	0		
Vulnerable/Both	7	Weak/No data (binding)/All Workers	7		
Vulnerable/Weak	34	Informal/Weak/Non-binding	0		
Vulnerable/None	34	Informal/Weak/No data (binding)	3		
Neither predicts stronger negative effects		Informal/Weak/All Workers	21		
Non-binding/Informal	1	Informal/None/Non-binding	1		
Non-binding/Both	6	Informal/None/No data (binding)	3		
Non-binding/Weak	0	Informal/None/All Workers	13		
Non-binding/None	8	Informal/Non-binding/All Workers	1		
Non-binding/All Workers	9	Informal/No data (binding)/All Workers	5		
Informal/Weak	28	Both/Weak/Non-binding	0		
Informal/None	19	Both/Weak/No data (binding)	4		
Informal/All Workers	6	Both/Weak/All Workers	12		
Informal/No data (binding)	35	Both/None/Non-binding	6		
Weak/All Workers	70				
Weak/No data (binding)	9				
Weak/Both	12				
All Workers/Both	22				
All Workers/None	74				
All Workers/No data (binding)	0				

None/No data (binding)	17				
None/Both	4				
Both/No data (binding)	35				

Note: As explained in the text, the classifications here pertain to the listed features of estimates. Thus, for example, under "two estimate features, both predict stronger negative effects," the two listed features more strongly predict negative effects and the other features are unspecified, so in actual fact in some cases three or four features of estimates may more strongly predict negative effects.

Appendix Table A7. Meta-Analysis Regressions, Based on Counts of Features of Estimates More Strongly Predicting Negative Employment Effects (Excluding Brazil)

	(1)	(2)	(3)
Variables: number of features of estimates that more strongly predict negative employment effects	Negative estimate (LPM)	Negative and significant estimate (LPM)	Estimated elasticity
No estimate features	0.667***	0.333**	-0.163
	(0.234)	(0.142)	(0.120)
One estimate feature	0.686***	0.371***	-0.146**
	(0.090)	(0.094)	(0.072)
One = No (p-value)	0.939	0.813	0.910
Two estimate features	0.708***	0.500***	-0.173***
	(0.064)	(0.083)	(0.052)
Two = One (p-value)	0.854	0.320	0.761
Two = No (p-value)	0.865	0.321	0.939
Three study features	0.789***	0.368***	-0.060
	(0.099)	(0.112)	(0.131)
Three = Two (p-value)	0.416	0.288	0.463
Three = One (p-value)	0.345	0.982	0.633
Three = No (p-value)	0.613	0.84	0.500
Four estimate features	1.000	0.571***	-0.192
	(0.000)	(0.039)	(0.127)
Four = Three (p-value)	0.039	0.120	0.459
Four = Two (p-value)	0	0.430	0.892
Four = One (p-value)	0.001	0.055	0.755
Four = No (p-value)	0.161	0.113	0.869
Joint test: Four = Three = Two = One (p-value)	0	0.205	0.912
Observations	168	168	168

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Note: LPM = linear probability model. The variables are defined to be mutually exclusive. There are 168 observations and 46 clusters. For the LPMs, standard errors are clustered by study. Note that for the estimates in column (1), there is no variation in the dependent variable for the "Four estimate features" variables, which is why there is no variation in the estimated coefficient. The only difference relative to Table 3 is the exclusion of studies for Brazil.

Table A8. Statistics of the HHI and Low Mobility by Method of Estimation

	Mean	Median	Min	Max	Sd
HHI	0.595	0.575	0.077	1.000	0.070
Low Mobility	0.608	0.608	0.000	1.000	0.071
NAICS	0.587	0.570	0.169	1.000	0.069
Flexible	0.578	0.560	0.125	1.000	0.069
Top Pairs	0.612	0.598	0.331	1.000	0.060

Note: The HHI is estimated by averaging industries, counties, and time (weighted by population). Low mobility is calculated as

$$Labor\ Mobility_{i,a,t} = \frac{remained_{i,a,t}}{remained_{i,a,t}+moved_{i,a,t}}$$

(see section 3) and then by averaging industries, counties, and time (weighted by population).

Table A9. Effects of the Log of the Minimum Wage on HHI and Low Mobility

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: HHI or Low Mobility	Hybrid	Low Mobility	NAICS	Flexible	Top Pairs
Ln (MW)	-0.00382	-0.00325	0.00301	-0.00729	-0.00347
	(0.00514)	(0.0165)	(0.00510)	(0.00636)	(0.00307)
Constant	1.083***	1.174***	1.123***	1.090***	1.082***
	(0.0914)	(0.419)	(0.0666)	(0.125)	(0.0681)
Observations	199,421	18,126	199,421	199,421	199,421
R-squared	0.909	0.365	0.974	0.856	0.981

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, log of teenage population, and log of total private-sector employment. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more relative flows of workers (top three links with highest flows with more than 90th percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. Column (3) defines the labor market by 3-digit NAICS code. In column (4), the cluster is defined by all the links; for instance, if industry A is connected to industry B, and industry B is connected to industry C, then A and C are connected. Column (5) only considers as a cluster the pair of industries with more relative flows between each other. See Section 4 for more details.

Table A10. Effects of the Log of the MW Interacted with the HHI and Low Mobility on the Log of Teenage Employment (HHI Calculated Only for Teenage Workers)

Dependent Variable: Ln (Teen Emp)	(1) Hybrid	(2) Low Mobility	(3) NAICS	(4) All Nodes	(5) Top Pairs
Ln (MW)	-0.223**	-0.167	-0.188*	-0.165*	-0.543***
	(0.0938)	(0.138)	(0.0944)	(0.0928)	(0.124)
Monopsony Variable (HHI or LM)	-0.381***	-0.859***	-0.241**	-0.349	-1.173***
	(0.138)	(0.242)	(0.106)	(0.228)	(0.342)
Monopsony x Ln (MW)	0.223***	0.447***	0.126**	0.196	0.631***
	(0.0757)	(0.127)	(0.0558)	(0.122)	(0.184)
Constant	-0.494	-1.806	-0.416	-0.577	-0.0139
	(0.723)	(1.539)	(0.750)	(0.740)	(0.727)
Observations	195,205	18,121	195,205	195,205	199,123
R-squared	0.988	0.989	0.988	0.988	0.988

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1
Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, log of teenage population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 implies full concentration. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more relative flows of workers (top three links with highest flows with more than 90th percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. Column (3) defines the labor market by 3-digit NAICS code. In column (4), the cluster is defined by all the links; for instance, if industry A is connected to industry B, and industry B is connected to industry C, then A and C are connected. Column (5) only considers as a cluster the pair of industries with more relative flows between each other. See Section 4 for more details.

Table A11. Effects of the Log of the MW Interacted with the Low Mobility on the Log of Teenage Employment (Low Mobility using Workers that Moved to Other County)

	(1)
Dependent Variable: Ln (Teen Emp)	Low Mobility
Monopsony Variable (HHI or LM)	-0.968***
	(0.275)
Monopsony x Ln (MW)	0.503***
	(0.145)
Elasticity of the MW depending on Monopsony	
Monopsony = 0	-0.184
	(0.158)
Monopsony = 0.2	-0.0843
	(0.161)
Monopsony = 0.4	0.0158
	(0.169)
Monopsony = 0.6	0.116
	(0.181)
Monopsony = 0.8	0.216
	(0.197)
Monopsony = 1	0.316
	(0.215)
Constant	-1.691
	(1.761)
Observations	18,127
R-squared	0.988

Robust clustered standard errors in parentheses by states.
*** p<0.01, ** p<0.05, * p<0.1
Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, log of teenage population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 means full concentration. Low mobility, which measures the percentage of workers who, when they change jobs, do not change industries, it includes workers that moved to other counties.

Table A12. Effects of the Log of the M.W Interacted with the HHI and Low Mobility on the Log of Prime-Age

Dependent Variable: Ln (Prime Age Emp)	(1) HHI	(2) Low Mobility	(3) NAICS	(4) Flexible	(5) Top Pairs
Ln (MW)	-0.0272	0.139	0.0345	-0.00485	0.0400
	(0.0866)	(0.113)	(0.0797)	(0.0830)	(0.0992)
HHI	0.149		0.317	0.199	0.389
	(0.296)		(0.286)	(0.290)	(0.322)
HHI x Ln (MW)	0.0514		-0.0572	0.0136	-0.0601
	(0.151)		(0.141)	(0.148)	(0.160)
Low Mobility		0.452			
		(0.368)			
Low Mobility x Ln (MW)		-0.220			
		(0.186)			
Constant	0.960	2.732***	0.897	0.930	0.828
	(0.672)	(0.602)	(0.678)	(0.683)	(0.665)
Observations	204,984	18,130	204,984	204,984	204,984
R-squared	0.999	0.999	0.999	0.999	0.999

Robust clustered standard errors in parentheses by states.

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include two-way fixed effects (county and time). Control variables are the log of the total population, log of prime-age population, and log of total private-sector employment. HHI measures concentration: HHI=0 implies perfect competition, and HHI=1 implies full concentration. Column (1) defines the labor market as clusters of industries, which consists of keeping only connections or links between industries with more relative flows of workers (top three links with highest flows with more than 90^{th} percentile of relative flows between industries). Column (2) uses low mobility, which measures the percentage of workers who, when they change jobs, do not change industries. Column (3) defines the labor market by 3-digit NAICS code. In column (4), the cluster is defined by all the links; for instance, if industry A is connected to industry B, and industry B is connected to industry C, then A and C are connected. Column (5) only considers as a cluster the pair of industries with more relative flows between each other. See Section 4 for more details.

Appendix C: World Bank Worldwide Governance Indicators

The World Bank Worldwide Governance Indicators are a research dataset summarizing views on the quality of governance provided by a large number of enterprises, citizens, and experts responding to surveys in industrial and developing countries. These data are gathered from several survey institutes, think tanks, non-governmental organizations, international organizations, and private sector firms.

1. **Rule of Law**: Reflects perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence. The indicator is continuous and its normalized, which means that 0 is the mean of all the sample, negative values indicates that the quality is below the average and positive above the average; it varies in time.
2. **Government Effectiveness**: Reflects perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government's commitment to such policies The indicator is continuous and its normalized, which means that 0 is the mean of all the sample, negative values indicates that the quality is below the average and positive above the average; it varies in time.
3. **Regulatory Quality**: Reflects perceptions of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development. The indicator is continuous and its normalized, which means that 0 is the mean of all the sample, negative values indicates that the quality is below the average and positive above the average; it varies in time.
4. **Control of Corruption**: Reflects perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as "capture" of the state by elites and private interests. The indicator is continuous and its normalized, which means that 0 is the mean of all the sample, negative values indicates that the quality is below the average and positive above the average; it varies in time.

Appendix D: Clusters Classifications

I follow four criteria to calculate the industry clusters:

1. **HHI (Preferred Classification)**: The rule followed is that I only use the top three connections for each industry (i.e. one industry with another three) or any other industry with at least in the 90th of the relative flows to capture important connections. For instance, the industry 4239 (Miscellaneous Durable Goods Merchant Wholesalers) has more flows with 562 (Waste Management and Remediation Services), 2213 (Water, Sewage and Other Systems), and 2123 (Nonmetallic Mineral Mining and Quarrying). However, there are many flows with other industries as well, such as with the industry 4219 (Miscellaneous Durable Goods Wholesalers). Thus, in these cases, I added more industries to the cluster until the next candidate has less than the value 90th of relative flow. In Figure 1, the red link is the stronger connection, green links are medium, and the yellow ones are weak. One cluster is formed by all the industries connected by the sum of green and red links. The hybrid cluster includes 421 and the top three connections 4413, 4411, and 4213. In some cases, clusters in this classification have more than four industries (one has up to 148), and others have fewer (one has two industries). Using this classification results in nine clusters of industries. This is my preferred classification because it is a compromise between all four.
2. **NAICS**: I assume that a worker can only work in the same industry (defined by the NAICS code). This assumption is the most restrictive, because workers cannot move among different industries; it is included for robustness and to present the extreme case where workers are stuck in one industry.
3. **Flexible**: For the calculation of criteria 2, 3, and 4, I create a web of industries connected by links. I need to restrict the number of flows to define a link because if I use a small number of flows, all the industries became one whole cluster. I define a link as more than the mean of

flows of workers (75.25 flows)[63] between industries in the whole period and all counties. Once a link is defined, I allow that all the industries connected by a link become one cluster. In Figure 1, this classification includes all the industries that are connected in the figure (sum of yellow, green, and red links), even if the connection is not direct, such as 722 and 4211, which are connected through 3327; or consider 4213, which is connected to 6241 via 4221. Using this classification leads to only two clusters: one with 215 industries and the other with only two industries: Iron and Steel Mills and Ferroalloy Manufacturing, and Metal and Mineral (except Petroleum) Merchant Wholesalers.

4. **Top Pairs**: This method includes only the pair of industries with a stronger connection, that is, a greater number of flows between each other. Industry 4411 (Automobile Dealers) has a greater flow of workers with industry 4413 (Automotive Parts, Accessories, and Tire Stores) than with any other industry. Hence, 4411 and 4413 have a reliable connection, and they form a cluster. In Figure 1, it is represented with a red node. Using this classification results in 60 clusters of two industries each.

Note that not all the industries are in a cluster. If one industry has less than the mean of relative flows, it is not considered to be part of any cluster. A final remark is that CPS industry codes are different from NAICS. I use the official Census "Industry Code Crosswalk" to transform the codes from CPS to NAICS.

[63] I try different cutoffs for the number of flows. If I consider nodes with fewer than 50 flows, it results in one cluster of industries (all the industries are connected). Hence, using the mean of flows can be interpreted as the minimum number of flows needed to have at least two clusters of industries.