Fighting Income Inequality with International Trade

Victor Hernandez Martinez†

Nicholas Kozeniauskas ‡

Roman Merga ⋆

June 2025

Preliminary and Incomplete

Abstract

This paper examines the impact of international trade on the wage distribution in Spain using employer-employee data from 1987 to 2004. We employ a novel instrumental variable approach to isolate the effects of trade openness on wage inequality. Our findings show that increased local trade exposure leads to a reduction in wage inequality. This effect is driven by within-industry and within-firm reallocation. At the industry level, trade openness leads to a shift in worker allocation towards small firms and low-skilled jobs. Furthermore, at the firm level, small firms expand their workforce (increased labor intensity) in response to trade openness, whereas larger firms experience a decrease in labor intensity. These findings suggest that trade openness generates a relative increase in demand for low-wage and low-skilled labor, contributing to a more equal distribution of wages in Spain.

We are greatly indebted to George Alessandria, Yan Bai, Ronni Pavan, and Lisa Kahn for their continuous advice and encouragement. We would like to thank Mark Bils, Gaston Chaumont, Hamid Firooz, John Singleton. In addition, we thank the participants at different seminars and conferences for their helpful comments. All errors are our own. The views in this paper are those of the authors and do not necessarily reflect the views of the Federal Reserve System, the Federal Reserve Bank of Cleveland, the Board of Governors, Banco de Portugal, the Eurosystem, or the International Monetary Fund, its Executive Board, or its management.

† Federal Reserve Bank of Cleveland, email: victor.hernandezmartinez@clev.frb.org.

‡ Banco de Portugal and Universidade Católica Portuguesa, Católica-Lisbon School of Business and Economics, email: nicjkoz@gmail.com.

⋆ International Monetary Fund, email: merga.roman@gmail.com

1 Introduction

A feature of many economies around the world during the last 40 years has been a sharp increase in income inequality. This phenomenon has generated considerable interest among policymakers, researchers in social science, and the general population due to its welfare and social implications. At the same time, during this period the worldwide economy has experienced dramatic technological changes and a sharp increase in international trade flows around the globe. As a consequence, these changes have been usually pointed to as the responsible forces for the changes in inequality in both developed and developing nations.

This paper studies the impact of global integration through international trade on income inequality. Using employer-employee data and equipped with recent advances in econometric methods, we develop a new instrumental variable approach to disentangle the effect that international trade had on wage and income inequality in Spain between 1993 and 2004. We focus on Spain during this period because Spain entered the European Single Market (ESM henceforth) in 1993.

Spain's entry into the ESM was coupled with two clear features directly observed in the aggregate data. Spain experienced a sharp increase in a) trade openness and b) wage inequality. In this paper, we estimate the impact that this trade liberalization episode had on the Spanish wage distribution. We show that, surprisingly and contrary to common belief, the increase in international trade reduced income inequality.

Our results suggest small benefits from increased trade for workers at the bottom of the income/wage distribution and larger losses for those in the top parts of the income/wage distribution. We show that this decrease in inequality is rooted in the firm and job level changes triggered by the increase in international trade. In particular, when looking across firms, increases in international trade shifted workers from medium and large firms towards small firms, while at the same time pushing workers from middle and high-skill jobs towards low-skill positions. Additionally, increases in trade openness exposure generated changes in the way firms organized their production. Small firms increased their use of labor, while larger firms reduced it. These within and across firm changes increased the relative demand for low-wage and low-skill workers, pushing their relative wages up and reducing income inequality.

These changes in wages were partially driven by two channels operating at different levels, one at the within-industry level, and the other at the within-firm level. Since smaller firms are more intensive in low-skill workers and tend to pay lower wages, the reallocation of workers towards these types of firms decreased the relative demand for medium and high-skill workers, reducing their premium. Second, as these smaller firms became more intensive in labor, and more low-skill intensive, they increased the relative demand for workers at the bottom of the wage distribution.

Our identification strategy for evaluating the effects of Spain's entry into the ESM relies on exploiting the sequential entry of countries into the ESM. Our main assumption is that as countries entered the ESM, the industry-level trade responses had two components: one that was country and time specific, and another that was common across countries. This allows us to exploit the evolution of trade openness in countries that entered the ESM after Spain as an instrument for the evolution trade openness in Spain when it entered the ESM. The underlying idea is that we are able to capture the common component that affects the countries that enter the ESM, strip out the effect of contemporaneous changes to the Spanish economy that could also have affected trade and labor market outcomes during the same period. For instance, there could have been changes in labor market regulations that affected demand for some types of labor or the production and export decisions of firms. Another possibility is that there were sectoral productivity shocks that affected international trade and labor demand that would generate bias in the estimated effect of international trade on the labor market in the absence of an instrument providing exogenous variation.

We embed this instrumental variable approach in an otherwise standard shift-share approach, as suggested by Borusyak et al. (2022), to estimate the effects of changes in region-level trade exposure on earnings and wage distributions. We use region-level analysis since we view regions as an appropriate level at which to define labor markets, since in the data we observe on a small amount of migration across regions.

These findings speak directly to a growing literature that argues that the standard Stolper-Samuelson predictions have not been observed in some trade liberalization episodes (see Goldberg and Pavcnik (2007) and Goldberg and Pavcnik (2005)) and that intra-industry adjustment are important for understanding the effects of trade on the wage distribution (Egger and Kreickemeier (2009), Helpman and Itskhoki (2010), Egger and Kreickemeier (2012) Amiti and D. R. Davis (2012), Ekholm and Midelfart (2005) Grossman et al. (2017)). We contribute to this literature in three ways. First, we estimate the causal effect of a trade liberalization on the full wage distribution. Second, we show that the labor market responses to the changes in international trade are not only driven by changes at the intra-industry level but also at the intra-firm level, something that is absent in most of the previous studies in the literature. Third, we develop a new instrumental variable that can be used in other settings with sequential entry into a system, as well as new econometric techniques to estimate the effects that trade has over the full distribution of wages.

We also rule some common mechanisms from existing research as drivers of our results. We show that, at least for the Spanish case, the increase in trade did not have an impact on entry or exit from unemployment, and a very small negative impact on labor market churning (i.e workers tend to change slightly less firm or industry). This speaks to the literature that relates trade and labor churning, as discussed by Davidson et al. (1999).

2 Data & Historical Context

2.1 Spain's entry into the EU and the ESM

The ESM is the result of a set of measures proposed in a 1985 white paper by the European Commission,[1] with the goal of finalizing the creation of an economic area in which no internal frontiers remain for the movement of goods, people, services, and capital. The 300 measures proposed in the 1985 white paper were signed into legislation at the European level in the Single European Act in 1986, with a proposed finalization date before the end of 1992.[2] By January 1st, 1993, approximately 90 percent of the proposed measures had been implemented, with the share varying from 80 to 95 percent across countries.[3]

It is important to note that the ESM did not create drastic changes in the tariffs that countries within the EU would implement toward other members' goods. The removal of these tariffs and quantitative restrictions, as well as the imposition of a common external tariff, was the result of the implementation of the Treaty of Rome in 1968, and this benefit was enjoyed immediately by any country entering the EU (which was 1986 for Spain). The creation of the ESM fulfilled a different purpose, that of removing any frictions impeding the economic integration of the EU members. As noted by Head and Mayer (2021), by the early 1980s it had become evident that the removal of formal trade impediments (i.e., tariffs and quantity restrictions) had not created the expected integration of the European market. The main reason was the existence of physical borders between countries (customs) and differing national regulations, which acted as a barrier to imports.[4]

[1] Completing the Internal Market: White Paper from the Commission to the European Council (1985).

[2] The finalization date was the date by which the participating countries should have approved national legislation to comply with the proposed measures.

[3] Percentages derived from Ragonnaud (2023) briefing.

[4] For instance, Italy required all pasta to contain 100 percent durum semolina, while Germany only allowed four ingredients in beer, based on the 1815 Bavarian Purity Law.

Head and Mayer (2021) show that the introduction of the ESM spurred a reduction in the cost of trading goods between EU countries. Relative to the mid-1960s, by the early 1990s the cost of trading goods had declined by approximately 15 to 18 percent, but the progress on economic integration had started to stagnate.[5] The introduction of the ESM provided a new impetus for the decline in trade costs. Relative to the mid-1960s, costs had fallen by 25 percent by 2004. Perhaps more importantly, the ESM also had the seemingly unintended consequence of generating a decline in trade costs between Europe and the rest of the world. In fact, this decline had a similar magnitude to the fall in trade costs within Europe. Head and Mayer (2021), show that the trade costs of goods between EU members and the rest of the world declined by around 17 percent from the mid-1960s to 1980. There was then a period of stagnation with these costs remaining stable until the implementation of the ESM. Then trade costs declined again, going from a 17 percent reduction in 1993 to a 25 percent reduction in the mid-2000s. In retrospect, it seems natural that the homogenization of national regulations across EU countries had an impact on trade costs for countries outside the EU. After the ESM implementation, a foreign country's production no longer had to be tailored to the requirements of a single country, but simply had to comply with the common standards of the union.

In addition to improvements in the movement of goods and services (a significant part of the regulations were directed to achieving free movement of services), the ESM also aimed to finally achieve free movement of capital among EU members. While the legislative basis for the free movement of capital was set by the Treaty of Rome, in practical terms the EU saw extremely limited progress in this area until the creation of the ESM. The main reason was the loophole in the original regulation allowing countries to impose protective measures on capital flows, which was used extensively during the 1970s and 1980s.

[5] Head and Mayer (2021) estimate that in 1980 goods' trade costs had already declined by around 12 percent, while in 1993 the decline was only five or six additional percent points larger.

Additionally, in the early 1990s the EU revamped its funding program with the creation of the European Cohesion Fund (CF) to work alongside the Regional Development Fund (ERDF), which already existed. While the goals of each fund were slightly different,[6] Spain was a primary beneficiary of both programs. This was especially the case during our period of interest between 1993 to the mid-2000s, as documented in Fuente and Boscá (2010) and Fuente and Boscà (2013).

2.2 Data

Our main data source is the Spanish Social Security Registry (MCVL). The MCVL contains the labor stories of a random 4% sample of the universe of Spanish employees, self-employed, unemployed individuals, and retirees for the years 2006 to 2018. One of the main advantages of the MCVL is that the inclusion of an individual in the sample for one year implies the inclusion of her complete social security history from when she first entered the labor market. This allows us to create a panel of workers from 1980 to 2004. A similar approach to this data has also been used by Arellano-Bover (2020).

The data contains daily information on employment, including job (type of contract, skill group, hours, location, etc.), individual (age, gender, education, etc.), and firm characteristics (firm ID, industry, age of the firm, etc.). For each employment, unemployment, and retirement record, wages or benefits are observed at a monthly frequency and top coded. We do not observe wages for self-employment records.

We combine all information by creating a yearly panel that contains information for only the main (most hours worked) job in that year. We calculate total income and hourly wages using all jobs for the period and discard all observations whose

[6] The ERDF's main goal is to transfer resources from the most prosperous regions in the EU to the least prosperous ones, while the goal of the CF is to provide support to countries with a GNI per capita below 90 percent of the EU average

main yearly activity is self-employment, agriculture, or in a public adminstration position with a highly structured career and wage path.[7] We also discard all observations where no location or industry code is available. We keep observations where the main activity for the year is unemployment but do not include unemployment benefits in total income or wages. We convert all income and wages to 2006 EUR using the CPI from the Spanish National Institute of Statistics (INE).

Our trade openness data comes from combining two different sources. The OECD's input-output tables provide information on yearly imports, exports, and output for over 30 industry groups in Spain for 1995–2016. We supplement this with data for 1987–1994 from INE. Industry codes, classification systems, and currency can vary across these two datasets, so we manually match each industry to its post-1995 group using the naming conventions to construct a continuous series from 1987 to 2004.

As mentioned above, our goal is to assess the relevance of changes in trade openness for the income and wages of workers. However, as trade openness increased in Spain, EU grants and assisted expenditures did too. This could pose a threat to our estimates if these grants impacted earnings or wages and if the growth in both trade openness and assisted expenditures/grants had a similar geographic distribution. To control for variation in assisted expenditures and grants provided by the EU through the CF and the ERDF, we rely on the data from Fuente and Boscá (2010) and Fuente and Boscà (2013). It contains information at the annual level on total assisted expenditures (expenditures primarily financed through EU funds) and total grants received by each autonomous community between 1993 and 2004. This data not only provides total assisted expenditures and grants, but also disaggregates but

[7]We exclude agriculture (including fishing) because of institutional structures in Spain that mean that we do not fully observe wages in this sector. We remove people in structured public adminstration careers (*funcionarios*) because their wages evolve in an rigid manner that is usually pre-determined, and because their jobs are (in most cases) fully guaranteed and thus are not exposed to unemployment risk. These individuals are mostly, but not exclusively, in the public administration sector. People working in the public adminstration sector without this type of contract are included in the data.

also disaggregated the expenditure by the activities that they were used for.[8]

One concern with using the MCVL from 2006 onwards to construct a sample of workers for 1987 to 2004 is sample selection. The main issue is that if someone who worked during the period died before 2006 then we are not able to observe their employment history.[9] We address this concern in two ways. First, we restrict our sample to people aged from 20 to 55, so that no individual would be older than 74 by 2006. This reduces the extent of selectin due to mortality. Second, we check the sample's representativeness by constructing moments for the Spanish labor market and comparing these with moments of the aggregate data from the INE. Table 1 presents the results of this analysis.[10]

Most of the moments from the MCVL sample and aggregate data do not have large differences in either their level, or the way that they evolve over time. As expected, the sample-to-population ratio decreases as we go further back in time. For 2004 we find a 4% sample-to-population ratio, in line with the size of the random sample selected, while for 1987 the ratio is 2.9%. Fortunately, this sample attrition does not appear to be associated with sample selection.

Four main divergences between our sample and the population are worth noting. First, the relative employment of Madrid and Barcelona during the late 1980s and early 1990s is up to 3.5pp (or 10%) larger than in the population. This is potentially due to the informal sector having relatively less importance in these large cities, but its relevance decreasing over time. Second, we find a 1.8 pp (10%) divergence in the share of employment in manufacturing for the year 2004. We attribute this divergence to our assignment of 2 digit industries into manufacturing and construction,

[8]Specifically, we observe whether the funds were used for infrastructure projects, other public investment projects, aid to private companies, human resources, R&D, job creation, or for other purposes.

[9]A secondary concern is if the degree of informality changed over time, then it would affect the representativeness of the sample

[10]The aggregate data is restricted to people aged 20–54 who are not employed in the agricultural sector to make the moments comparable to those from our sample

Table 1: Summary Statistics

	1987		1993		1998		2004	
	Data	Pop	Data	Pop	Data	Pop	Data	Pop
N (000's)	246	8,408	343	9,542	441	11,311	603	15,088
Pop in Data	0.029		0.036		0.039		0.040	
Female	0.295	0.306	0.348	0.346	0.377	0.368	0.420	0.413
20-29	0.406	0.351	0.376	0.323	0.369	0.315	0.329	0.294
30-39	0.359	0.367	0.363	0.381	0.364	0.382	0.377	0.390
40-49	0.232	0.283	0.257	0.295	0.263	0.303	0.290	0.316
Manufacturing	0.290	0.286	0.233	0.237	0.206	0.222	0.168	0.186
Construction	0.108	0.102	0.114	0.105	0.116	0.108	0.137	0.131
Service	0.604	0.612	0.653	0.658	0.678	0.670	0.695	0.683
Large City	0.325	0.299	0.321	0.282	0.308	0.284	0.300	0.289
Medium City	0.273	0.296	0.269	0.295	0.275	0.291	0.286	0.294
Small City	0.402	0.405	0.409	0.423	0.417	0.425	0.414	0.417
Average Earnings	15,346	16,045	17,957	18,068	16,820	18,260	17,082	17,043
Median Earnings	14,252		16,494		15,219		15,454	
Average H Wages	8.04		9.45		9.21		9.30	

as well as to the conversion of industry information in different time periods into one single industry classification system that does not overlap perfectly. Third, as expected, our sample skews younger in all periods of time, but this effect is much more pronounced in the early years of our sample. This is the effect of mortality on the sample. Finally, we find average total earnings to be very similar in the MCVL and INE's data for all years except 1998, which presents an 8% lower value in the former compared to the latter.

Overall our sample appears to provide a good representation of the Spanish labor market for the period that we study. This is particularly the case for 1993 onwards, which is the period that most of the analysis will focus on.

3 Empirical strategy

To measure exposure to trade, we measure *trade openness* as the ratio of exports plus imports to output. Our basic equation of interest for understanding the effects of trade openness (denoted as TO) on labor market outcomes is:

$$Y_{i,p,k,t} = +\beta_1 \ln TO_{p,t} + \beta X_{i,p,k,t} + \alpha_i + \epsilon_{i,p,k,t}, \tag{1}$$

where an observation is for an individual i, located in province p, working in industry k at time t. $Y_{i,p,k,t}$ is a labor market outcome, which we will be more explicit about soon, $TO_{p,t}$ is trade openness at the province level, $X_{i,p,k,t}$ is a vector of control variables that we specify in detail below, α_i contains individual fixed effects and ϵ is the residual.

The main labor market outcome of interest is wages/earngings, so we will primarily discuss the empirical strategy with respect to this. Recovering the causal effect of trade openness on wages directly from equation (1) is infeasible for two reasons. First, there may be unobserved variables affecting both the evolution of trade openness and earnings/wages in a given province simultaneously. For instance, if areas with larger labor supply growth tended to increase trade, and labor supply growth affects wages, we would face an omitted variable bias problem. Similarly, if areas where trade openness increased more also saw larger increases in assisted expenditures/grants that affected earnings or wages, our estimates would also be biased. While we can account for variation in some of those unobserved covariates (for instance, we have data on assisted expenditures/grants), it is impossible to account for all of the potential omitted variables. Second, there could be reverse causality. Provinces with higher or lower wage growth might be more likely to engage in more international trade. To overcome these potential problems and estimate the impact of international trade on our outcomes of interest we use a shift-share instrumental variable approach that is carefully described in Borusyak et al. (2022). It is also similar to the approach of Autor et al. (2016),

3.1 Shift-share instrumental variable

Our data on trade in Spain contains information on the value of imports, exports, and output, by industry, for each year from 1987 to 2004. Our interest is to build a measure of exposure to international trade for each location (i.e., province) and time period (i.e., year). While an industry measure of exposure to trade is more readily available, we believe that a location-based measure of trade openness is a better unit of observatyion for capturing workers' exposure to international trade. Workers change industries relatively often (in our sample, on average 15 percent of workers change their 2-digit industry code each year), while moves between provinces are far less common (on average 3 percent of our sample move between provinces each year).[11].

We provinces provinces as the unit for location for several reasons. First, continental Spain is composed of 50 provinces which allows us to have a significant amount of variation in the cross-section. Second, provinces are the closest unit that we can observe in our data that closely resembles a local labor market. Most provinces in Spain contain one or two major cities and a surrounding area whose labor market activity is heavily determined by the major cities in the province. According to the Spanish Survey of the Labor Force (EPA), the share of workers living in a different province from the one they work in is less than 3 percent throughout our sample period. This is not the case for smaller units within provinces (e.g. cities) that see a significant number of workers commute to different cities within the province (especially from suburban cities to big population centers). Third, as discussed above, migration across provinces is rare. This is not true for smaller location units within

[11]The annual cross-province migration rate ranges from 3 to 4 percent during our sample period. This rate is consistent with estimates from the Spanish Survey of the Labor Force. As further support for our approach, Autor et al. (2014) and Autor et al. (2016) show that import shocks have a relatively small impact on workers' migration probability in the USA.

provinces that see a significantly larger migration share each year, primarily driven by housing choices. Fourth, provinces are the smallest location units that we can reliably and consistently measure in the data. Smaller location units (e.g. zip codes or cities) are only available for a subset of workers located in larger cities, which would leave over 30 percent of workers unassigned to a location.

For our measure of exposure to trade, we need to measure trade openness at the province level. Since province-level data for international trade is not available, we build a measure based on industry-level trade. For province p at time t we take the weighted sum of trade openness in each industry k at time t, where the weights are industry employment shares for the relevant province. That is,

$$TO_{p,t} = \sum_k \frac{N_{k,p,t-3}}{N_{p,t-3}} TO_{k,t}, \tag{2}$$

where $N_{k,p,t-3}$ denotes the number of employees in industry k, province p, at time $t-3$, $N_{p,t-3}$ is the number of employees in province p at time $t-3$,[12] and $TO_{k,t}$ is the trade openness of industry k at time t. At the industry level trade openness is the ratio of imports plus exports to industry-level production.[13] Since the evolution of the local industry employment shares may be endogenous to the evolution of trade, the use of contemporaneous employment shares could generate a bias in our estimates. To overcome this problem, we therefore lag the province level industry employment shares by three years. Even if changes in trade have some persistence for a location, the three-year lag allows us to reduce this concern.

Even after the introduction of lagged industry employment shares, there is still the concern that today's industry trade openness $TO_{k,t}$ is not exogenous. It could be correlated with other characteristics that also affect earnings or wages. Since our

[12] Province-level employment satisfies $N_{p,t} = \sum_k N_{k,p,t}$.

[13] Industry level imports in the data are measured by assigning imported products to the industry that would produce them. So, for example, a wooden table would be assigned as an import in the furniture production industry.

values of local trade openness exposure are built from industry measures of trade openness, this could generate a bias in our estimated results. Additionally, workers could anticipate changes in trade openness and try to change industries, and this could be correlated with future changes in international trade.

To address these issues, we instrument the evolution of trade openness for each industry in Spain with the evolution of trade openness in the same industry in the Czech Republic, Hungary, and Poland after they joined the ESM. We choose these countries because they joined the ESM in 2004, 11 years after Spain. Furthermore, they did not adopt the Euro currency at the time of joining or soon after, making their cases more comparable to Spain's, as it maintained the Peseta as its currency until 2002. Regarding timing, we match our panel such that trade openness in Spain in industry k for 1993 (Spain's first year in the ESM) coincides with the average trade openness in the Czech Republic, Hungary, and Poland in industry k in 2004 (their first year in the ESM).

We instrument $TO_{p,t}$ using $TO^{IV}_{p,t}$, which is constructed as:

$$TO^{IV}_{p,t} = \sum_k \frac{N_{k,p,t-3}}{N_{p,t-3}} \left(\frac{TO^{CZ}_{k,t+11} + TO^{HU}_{k,t+11} + TO^{PL}_{k,t+11}}{3} \right) \tag{3}$$

where $TO^{j}_{k,t+11}$ is the trade openness of industry k in the country j at the time when that country had been in the ESM for the same number of years as Spain had in year t (i.e. $t + 11$ since the Czech Republic (CZ), Hungary (HU), and Poland (PL) entered the ESM 11 years after Spain).

The assumption underlying this instrument is that the Czech Republic's, Hungary's and Poland's exposures to trade liberalization through joining the ESM generated changes in trade openness that were similar to the changes in Spain. In other words, joining the same markets, even at different times, generated trade dynamics that share a common component. Taking the average over the countries allows us to reduce the relevance of idiosyncratic characteristics of each country, making the instrument a better measure of the common component of trade openness present in all countries. This assumption has an easily testable implication. Our first stage should be robust after the countries entered the ESM and, more importantly, it should be irrelevant prior to these countries joining the ESM. As we show below, that is precisely the case in the data. The instrument has a strong first stage in the year after a country joins the ESM and is very close to zero, and insignificant, prior to the countries entering the ESM.

Beyond assuming the existence of a common component in the evolution of trade openness, our identification relies on there being no other changes jointly affecting the evolution of trade openness and wages at the times when the countries joined the ESM, after controlling for relevant observable characteristics. A situation in which countries are at similar points of economic development when they join the ESM, leading to a rapid increase in trade, would generate a bias in our results. As we discuss later, this seems unlikely, given the differential stages of development that we observe for the Czech Republic, Hungary and Poland at the time they joined the ESM. Similarly, if joining the ESM results in other types of economic changes (subsidies, assisted expenditures, etc.) that affect both trade decision and wages, this could bias our results. To address this we will document that forces such as subsidies and assisted expenditures coming from the EU appear irrelevant for our main results. Finally, it would be an issue if there were common changes in trade for reasons other than joining the ESM, that affected both trade openness and wages, and were present in Spain in 1993–2004 and in the other countries in 2004–2015. While theoretically possible, this seems unlikely given the difference in the time of ESM entry for Spain and the robustness of our results to different specifications that consider this possibility.

3.2 Measurement of the distributional impact

Looking beyond the effects of trade openness on mean wages and earnings, we want to understand its impact at different points of the distributions of wages and total income. To be able to estimate the distributional impact, we use the unconditional quantile regression method developed by Firpo et al. (2009). This method builds on the recentered influence function (RIF) and is well suited to our purpose.[14]

The idea behind this method is to transform the problem by computing the covariate's influence on income shares rather than quantiles. For instance, let us assume that we want to estimate the impact that trade has on wages at different points of the wage distribution. By estimating how a covariate (i.e. trade openness) affects the share of the population below different wage thresholds, the semi-elasticity shows the effects of an increase in the trade openness on the cumulative distribution functions (CDF) of the wages. Then, we invert the impact of trade openness on the CDF of income to estimate the impact on an income quantile, which we achieve by using the re-centered influence function (RIF) regression approach proposed by Firpo et al. (2009).

An advantage of using an unconditional quantile regression, as we are doing, compared to a conditional approach is that the latter only measures the within-group dispersion, while the former measures the impact on overall wage dispersion. It does this because as well as measuring the impact within the group (captured by the standard conditional quantile regression), it also includes effects on between-group inequality. In our context this essentially means that it also captures how trade affects the conditional mean of wages.[15]

There are several methods for estimating the unconditional quantile partial effects (UAPE, henceforth). We proceed using the RIF-OLS method as described by

[14] For a further discussion about this method we refer the reader to Firpo et al. (2009), Currie et al. (2020) and Fortin et al. (2011).

[15] See Firpo et al. (2009) for details

Firpo et al. (2009) [16], as follows:

1. We compute the RIF for people in the τ^{th} quantile, q_τ, for the variable of interest Y:

$$RIF(y; q_\tau) = q_\tau + \frac{\tau - \mathbf{1}\{y \leq q_\tau\}}{f_Y(q_\tau)}, \tag{4}$$

   where $f_Y(q_\tau)$ is the density of Y evaluated at q_τ, which is estimated using a non-parametric kernel estimation and Epanechnikov kernel function. We denote the estimated RIF by $\widehat{RIF}(y; q_\tau)$

2. We proceed to estimate the following equation for each τ:

$$\widehat{RIF}(y; q_\tau) = \beta_1^\tau \ln TO_{p,t} + \beta X_{i,p,k,t} + \alpha_i + \epsilon_{i,p,k,t}, \tag{5}$$

   where the i, p, k and t subscripts have the same interpretation as previously, and τ denotes the quantile of the distribution of y and individual i belongs to at time t. Our main interest continues to be estimating β_1^τ, which measures the effects of international trade on income over the income distribution.

For our identification strategy, we use the same procedure as described above. We instrument trade openness using the trade openness of our three donor countries during the period following their entry into the ESM, $\ln TO_{p,t}^{IV}$. It is important to note that Firpo et al. (2009) does not consider the use of an instrumental variable approach in unconditional quantile regressions. While a complete econometric analysis of the validity of an IV approach in the RIF framework is outside of the scope of this paper, in the Appendix we provide simulations showing that, in the presence of omitted variable bias, our instrumental variable strategy within the RIF-OLS framework recovers identical point estimates than those derived from the RIF-OLS in the absence of omitted variable bias.

[16] For this we need to assume that $Pr(Y > q_\tau | X = x)$ is linear in x. For our interest, we assume that the probability a worker's income is above the quantile τ is linear in the trade openness.

4 The labor market consequences of international trade

First Stage Results

After following the procedure above to construct our instrument, our first stage specification is:

$$\ln TO_{p,t} = \gamma_1 \ln TO_{p,t}^{IV} + \gamma X_{i,p,k,t} + \theta_i + e_{i,p,k,t} \tag{6}$$

the second stage is:

$$Y_{i,p,k,t} = +\beta_1 \ln \hat{TO}_{p,t} + \beta X_{i,p,k,t} + \alpha_i + \epsilon_{i,p,k,t} \tag{7}$$

Our main specification includes industry and province dummies to account for persistent differences in trade openness levels across industries and provinces.[17] Similarly, it includes year fixed effects to extract the common evolution of trade openness in Spain over our years of interest. Since all our specifications are individual-level, we also control for persistent worker characteristics that are not affected by the evolution of internal trade. Specifically, we include age fixed effects and allow them to vary by gender, as well as individual fixed effects.[18] Finally, since our variation in trade openness is at the province-year level and not at the individual level, we cluster the standard errors at the province level. Individual fixed effects are in θ_i and α_i, and all other controls are in $X_{i,p,k,y}$.

The first stage estimates are presented in Table 2. In column (1), where we consider the period after the implementation of the ESM, the results indicate that a 1 percent increase in our instrumented trade openness results in a 0.66 percent increase in trade openness in Spain. This result is highly significant, with an F-

[17]For workers whose main/only activity in a year is non-employment, we create a separate industry dummy.

[18]For age, we use one fixed effect for each year of age.

statistic of 34.

Table 2: First Stage Results

	(Post ESM Entry) (1993-2004) $\ln TO_{l,t}$	(Pre ESM Entry) (1987-1992) $\ln TO_{l,t}$
$\ln TO_{l,t}^{IV}$	0.655***	0.008
	[0.112]	[0.211]
Controls	All	All
F-stat	34.11	0.001
Observations	4,421,499	750,468
R^2	0.978	0.996

Note: This table displays the first stage of the IV estimation. Column (1) shows the estimates for the period post-entry in the ESM. Column (2) shows the estimates for the period pre-entry in the ESM. Controls "All": ID, year, province, industry, and age by gender fixed effects. Province-level clustered standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Our data and empirical strategy allow us to test the main identification assumption of the instrumental variable approach: that entry into the ESM generated a common dynamic in trade openness. If this was the case, we should not observe any relationship between the potential endogenous variable and the instrument in the years prior to the entry into the ESM. Therefore, to test the validity of our IV approach, we reestimate our first stage but focus on the period before entry into the ESM. Specifically, we rerun our first stage results for the years 1987 to 1992 in Spain, matched to the corresponding period for our three countries that joined the ESM later. The results are included in column (2) of Table 2. In line with our expectations, we find that the instrument does not satisfy the relevance condition in the years before entry into the ESM, with a coefficient very close to zero and insignificant.

Second Stage Results

Table 3 presents the OLS and IV estimates of the effect of trade openness on wages, total earnings, and total hours worked. Starting with total earnings our OLS results show a small negative association between local trade openness and total earnings. Our IV estimates amplify the results from the OLS specification. Specifically, we find that a 1 percent increase in trade openness decreases total earnings at the individual level, on average, by approximately 0.1 percent, an effect that is six times larger than the one estimated via OLS.

Table 3: Average effects

		(OLS)			(IV)	
	Earnings	Wages	Hours	Earnings	Wages	Hours
$\ln TO_{l,t}$	-0.014	-0.020	0.006	-0.096	-0.118***	0.029
	[0.030]	[0.014]	[0.031]	[0.073]	[0.035]	[0.077]
Controls	All	All	All	All	All	All
Observations	4,421,499	4,372,739	4,372,739	4,421,499	4,372,739	4,372,739

Note: This table displays the second stage of the IV estimation. Controls "All": ID, year, province, industry, and age by gender fixed effects. Province-level clustered standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

When we replace our dependent variable with hourly wages, the conclusion is similar. We find no effect of trade openness on wages when considering our OLS specification. On the other hand, the IV results show that for a 1 percent increase in trade openness, wages decrease, on average, by a significant 0.12 percent. Finally, looking at total hours, neither the OLS nor the IV estimates find a significant impact of trade openness on hours worked at the individual level.

An important concern with these results is that the implementation of the ESM coincided with a period when the assisted expenditures and grants received from the EU increased significantly in Spain. If the increase in subsidies was responsible for the observed changes in wages and total earnings but, at the same time, were correlated with the evolution of trade openness across provinces in Spain, our main estimates could be biased. To assess the relevance of this, we use data from Fuente and Boscá (2010) and Fuente and Boscà (2013) that provides the amounts that of Spain's 17 autonomous regionsreceived (and used) from these sources during 1993–2006. We also make use of their estimates of assisted expenditures and grants for detailed expenditure subcategories. These include public infrastructure, other public spending, aid to private companies, employment creation, human resources, R&D, and others.

To control for the effects of these subsidies, we convert their nominal data to real Euros using appropriate GDP deflators and apply a log transformation to the results. We do this for both total assisted expenditures and grants, and their subcategories. Using estimates of total population by year and autonomous community from INE we also compute per capita values for these variables. We add these covariates (one by one and in groups) to our estimation. If our main results were driven by the correlation between these changes in subsidies and the changes in trade openness across provinces, our point estimates of interest would change significantly and the effect of the different subsidy covariates on the outcome would be economically meaningful and statistically significant.

The results are shown in the Appendix in Table A.1 for assisted expenditure and Table A.2 for grants. We find that controlling for these subsidies has no impact on our estimates of interest and, if anything, makes the negative aggregate effect on total earnings and hourly wages slightly larger. For instance, after controlling for all disaggregated categories of subsides, we find that a one percent increase in trade openness decreases mean total earnings by a significant 0.11 to 0.12 percent instead of the 0.1 percent in our main results above. Similarly, we find that including these subsidies does not have any meaningful impact on our first-stage results, with the F statistics and point estimates almost identical to those in the main text. In general, the effect of these subsidies on our outcomes of interest tends to be either insignificant or small, suggesting that not only were they uncorrelated with the evolution of trade openness but that they were not very effective at improving within worker earnings or wages. Two notable exceptions are investments in productive infrastructure (primarily transport, water, and other urban infrastructures) and residential development. The former tends to have a positive and significant association with total earnings and hourly wages, while the opposite is true for the latter.

It is worth mentioning again at this point that, as shown in Head and Mayer (2021), the creation of (or access to) the ESM not only impacted trade costs with EU members but also significantly reduced trade costs with the rest of the world. Spain saw an increase in trade deficits within and outside the EU during this period, making these results consistent with the ones found by Autor et al. (2016) for the USA when looking at import shocks from China.

Previous literature, as in Egger and Kreickemeier (2009) and D. Davis and Harrigan (2011), show quantitatively that increased exposure to trade can increase churning in the labor market. And this increase in churning can lead to wage/income inequality increases. To test whether this is the case for Spain's entry into the ESM we analyze the effects of trade openness on the propensity to be unemployed, as a measure of labor market rotation.

To perform this analysis we construct two variables to use as our outcomes. First, we construct a dummy variable taking a value of one if a worker's main activity during the relevant year is unemployment and zero otherwise. Second, we construct a variable that counts the number of months the worker spent unemployed during the year, regardless of whether unemployment was the main activity of the worker or not. It is important to note that our unemployment information comes from records from the social security registry of whether workers collect unemployment insurance (UI). This implies some limitations. Workers with working histories shorter than one year do not qualify for UI benefits. Workers may exhaust their UI benefits and therefore not appear in the data despite being unemployed. Finally, workers may qualify for UI benefits but not claim them.

To circumvent these issues, we consider an additional outcome to determine the effect of trade openness on unemployment. We collect data on the unemployment rate at the province-by-year level and replicate our main analysis using province-by-year data instead of individual data.

The results are shown in panel (A) of Table 4. The first two columns explore whether an increase in trade openness in a year affects either the probability of collecting UI benefits during that year (column 1) or the total number of months the worker collects UI benefits in that year (column 2). We find no evidence for either of these relationships. This evidence is consistent with the fact that we find no change in hours worked, on average, in Table 3.

The results using the unemployment rate at the province-year level are shown in columns (3) and (4). Column (3) uses the average unemployment rate during the year, while column (4) uses the fourth quarter unemployment rate. Both results indicate that provinces where trade openness increased more did not see a significant difference in their unemployment rate.

Panel (B) repeats the exercise in panel (A), but lage the unemployment variables by one year. The results are identical to those in panel (A). We do not find evidence that trade openness affects a worker's unemployment probability one year in the future or the future province-level unemployment rate. As was the case in our results on wages and total earnings, controlling for the amounts received in subsidies from the EU has no impact on any of the results in panels (A) or (B).

Methodologically, some limitations to our empirical strategy remain. First, we do not use different price deflators by location due to the lack of data at this geographic level. If locations with higher exposure to international trade experienced different price changes, our results for real wages may be biased. While this is clearly

Table 4: Trade Openness and Unemployment

	(IV)	(IV)	(IV)	(IV)
	Panel A			
	Unemployment in t	Months in Unemployment in t	Province Level UR in t	Province Level UR in Qoft
$\ln TO_{p,t}$	-0.004	-0.163	0.020	0.002
	[0.021]	[0.127]	[0.055]	[0.054]
Controls	All	All	All	All
Obs	4,421,499	4,421,499	600	600
	Panel B			
	Unemployment in $t+1$	Months in Unemployment in $t+1$	Province Level UR in $t+1$	Province Level UR in Q4 of $t+1$
$\ln TO_{p,t}$	-0.050	-0.414*	-0.045	-0.064
	[0.033]	[0.219]	[0.053]	[0.053]
Controls	All	All	All	All
Obs	3,690,841	3,690,841	550	550

Note: The table displays the effect of trade openness on different measures of unemployment. Panel (a) considers the effect of trade openness on contemporaneous unemployment, while Panel (b) does so for future unemployment. Controls "All": ID, year, province, industry, and age by gender fixed effects. Province-level clustered standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

important for accounting for the average effect on real wages, it is less likely to generate a bias in the estimated changes in the real wage distribution within particular provinces.

Second, the data we use to control for the evolution of subsidies during this time period is at the autonomous community level instead of at the province level. Autonomous communities are larger than provinces (using a US analogy, think of autonomous communities as states and provinces as commuting areas). Third, as described by Head and Mayer (2021), with the implementation of ESM the free flow of capital across EU members became a realization after years of countries using loopholes to avoid it. If capital inflows into Spain were correlated during this period with the evolution of trade openness, part of the effects we identify as arising from trade openness may result from changes in capital flows.

In summary, we estimate that in regions with a larger increase in trade openness, average earnings and wages declined, while average hours worked remained unchanged. These results are robust to introducing controls for EU assisted expenditures and grants at the regional level, which were increasing during our period of interest.

4.1 The distributional consequences of international trade

We now proceed to estimate the effects of trade openness on total earnings and wages throughout their respective distributions. The previous literature has documented the effects of international trade on wages over the wage distribution through the lens of models of international trade or by documenting stylized facts. For instance, Helpman et al. (2017) finds that international trade increased wage inequality for Brazil in 1994 because it had a more positive impact on the wages of workers in larger firms, and firms that exported, who already higher wages.[19] Similarly, Burstein and Vogel (2017) finds that trade liberalization can reallocate factors towards more skill-intensive sectors, and towards more productive and skill-intensive firms within sectors. This leads to higher skill premiums and higher wage and income inequality for most of the countries they analyze.

The results of our distributional analysis are shown in Figure 1. In the top two panels we present the results for total earnings, in the middle two panels the results for hourly wages, and in the bottom two panels the results for hours worked.

Since our data has a panel structure and the RIF requires us to determine the density of the outcome at each of the quantiles of interest, we propose two methods to do this. The panels on the left (panels (a), (c), and (e)) ignore the panel dimension of our data and calculate the density of the outcome at each quantile using the complete panel to determine the location of the quantiles. For the panels on the right (panels (b), (d), and (f)) we estimate the density at each quantile (and determine the location of those quantiles) by year, for each year between 1993 and 2004. In

[19] The model in Helpman et al. (2017) could also predict a reduction in wage inequality in case the labor force is mostly concentrated in firms that engage in export activities.

all figures, the solid line shows the estimate of the effect of trade openness on the outcome (total earnings or hourly wages) at that point of the outcome's distribution (the x-axis). The dotted lines in all panels are the 95 percent confidence intervals.

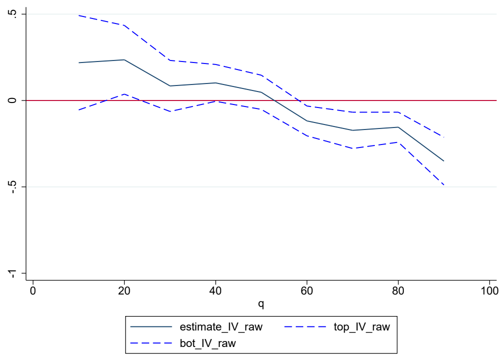
Starting with the results for earnings, both methods show similar conclusions. We do not find significant effects of trade openness on earnings for the bottom half of the earnings distribution. However, for the top half of the distribution, we observe that increases in trade openness had a negative impact on earnings, with the negative impact becoming larger the higher up the distribution you go. For instance, in panel (a) at the 60th and 80th percentile, a one percent increase in trade openness decreases total earnings by approximately 0.2 percent. Comparing panels (a) and (b), we find very similar patterns. We do not present the results after controlling by assisted expenditure or grants because we find them to be almost identical to the ones displayed here.[20]

Moving to the results for hourly wages in panels (c) and (d), the conclusion is similar. In general, the effect of trade openness on hourly wages becomes worse the further up the distribution you go. However, there exist two small differences compared to the results on total earnings. First, we observe some evidence that an increase in trade openness had a significant positive effect on hourly wages for workers in the bottom 30 percentiles of the hourly wage distribution. Second, we see more negative estimates at the top of the distribution (percentile 60th and above). At the 80th percentile, a one percent increase in trade openness decreases total earnings by approximately 0.5 percent.

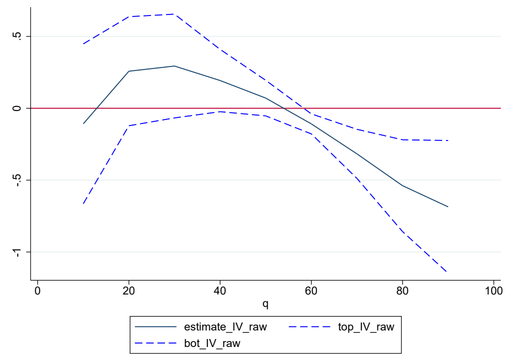
Finally, panels (e) and (f) show the effect of trade openness on hours worked across the total earnings distribution. Note that using the RIF here is impossible since we are not interested in the effect of trade openness on hours worked throughout the distribution of hours worked but throughout the distribution of total earnings. Therefore, instead of using unconditional quantile regressions, we rely

[20]These results are available upon request.

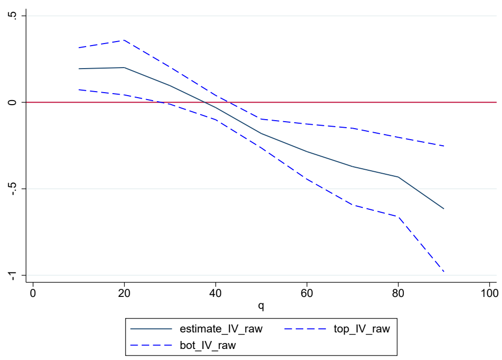
Figure 1: Distributional Effects



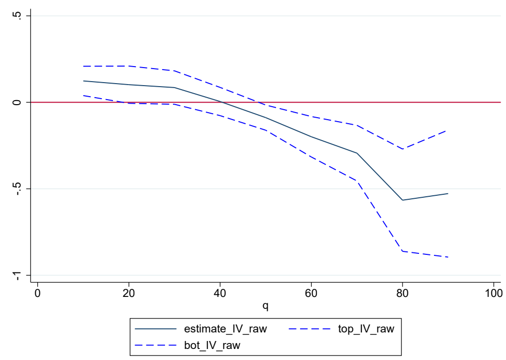
(a) Total Earnings



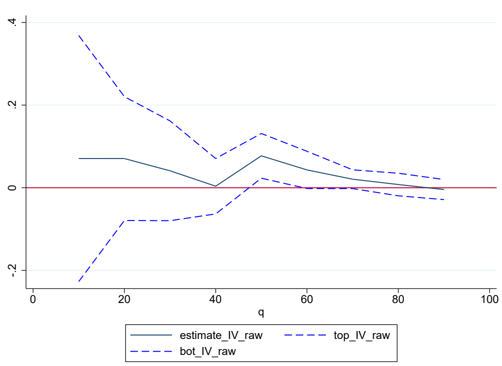
(b) Total Earnings, Yearly



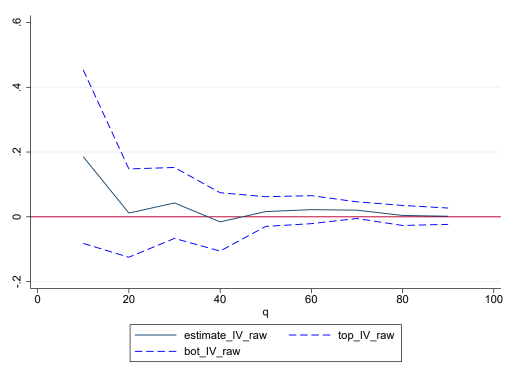
(c) Hourly Wages



(d) Hourly Wages, Yearly



(e) Hours Worked



(f) Hours Worked, Yearly

Notes: The results for total earnings and hourly wages are over the total earnings and hourly wage distributions, respectively. For hours worked, the results show changes across the distribution of total earnings. For the panels on the left (a, c and e), the quantiles of the distributions are constructed by pooling all observations in the sample. For the panels on the right (b, d and f) the quantiles are computed year by year.

on conditional quantile regressions for this exercise and estimate the effect of trade openness on hours worked for each one of the ten deciles of the earnings distribution. We do not find evidence that trade openness had a significant effect on hours worked.[21]

The results in Figure 1 show at least four striking features regarding the distributional impacts of international trade. First, they show that international trade changed labor demand and hence the income distribution towards low-income types of workers. This result stands in contrast with the common belief that trade increases inequality, and it is contrary to the quantitative predictions of a wide variety of models in international trade that predict increases in wage inequality (see Harrison et al. (2010) and Crozet and Orefice (2017) for more details).[22] Second, the results show that while wages appear to slightly increase for workers at the bottom of the wage distribution, this is not the case when we look at the total income. While it is possible that this was driven by a small income effect, the result presented does not imply this since people in each quantile of each distribution are not necessarily the same. Third, the effects of international trade on income or wages over the distribution of total income and wages (respectively) are highly non-linear. Fourth, despite the strong effects on income and wages, we do not find any evidence that hours worked within worker changed as a consequence of international trade exposure.

In summary, these results highlight the relevance of applying the proposed method of unconditional quantile regression with the instrumental variable approach to answer and understand the effects that international trade has on the distributions of wages and earnings. Otherwise, our results would have been biased by the inability to capture the sole effect of international trade over the distribution or by

[21]Looking at the effect of trade openness on hours worked throughout the distribution of hourly wages results in identical conclusions.

[22]This result opens the door to the possibility of a missing factor that can be affecting international trade and inequality at the same time generating the common increasing trend in inequality documented in **??** and in several works, see Crozet and Orefice (2017) for more details

the assumption of homogeneous effects over the distributions that are not supported by the data.

5 Conclusion

In this paper we develop a new type of instrumental variable approach to understanding the effects of increased exposure to international trade, specifically focusing on Spain's entry into the ESM. The period in which this occurred is characterized by a sharp increase in trade openness paired with an increase in the trade deficit in Spain. Our main results suggest that the increase in trade openness reduced income inequality.

Our empirical exercises allow us to characterize the effect that these changes in trade had on income and wages over the entire distribution. We observe that changes in international trade increased wages at the bottom of the wage distribution, and decreased them for workers in the middle and top parts of the wage distribution. We find a similar conclusion when considering the effects on total income. Our estimated effects are not only statistically significant but economically important. We observe an average increase in trade openness of approximately 5% annually in Spain from 1993 to 2004. This change implies an estimated increase in wages of approx. 1% annually for workers at the 20 percentile of the wage distribution. On the other hand, this change also implies an estimated annual decrease in wages of approx. 2% for workers at the 70-80 percentile of the income distribution.

These results are in contrast with the recent quantitative predictions of international trade models analyzing the effects of trade on wages over the wage distribution. We find two main mechanisms absent in these models that may explain these differences. First, increased international trade induced a reallocation of resources towards smaller firms. Second, increased international trade changed the way certain firms organized their production, especially small firms. We find that as these firms are more exposed to international trade, they tend to increase their employment, become more labor intensive, and more intensive in low-skill types of jobs.

We think this paper opens several opportunities for future research. First, understanding not only why firms change their labor intensity but also the quantitative impact of these changes to account for the changes in the wage distribution. Second, disentangling the effects that the increase in the trade deficit had on the labor market in this period and its relevance in accounting for the observed changes. Finally, another interesting avenue of research is to understand the quantitative relevance of these micro-level results in explaining the large drop in the total factor productivity experienced by Spain after joining the ESM.

References

Amiti, M., and Davis, D. R. (2012). Trade, firms, and wages: Theory and evidence. *The Review of Economic Studies*, *79*(1), 1–36.

Arellano-Bover, J. (2020). Career consequences of firm heterogeneity for young workers: First job and firm size.

Autor, D., Dorn, D., and Hanson, G. (2016). The china shock: Learning from labor-market adjustment to large changes in trade. *Annual Review of Economics*, *8*(1), 205–240.

Autor, D., Dorn, D., Hanson, G., and Song, J. (2014). Trade adjustment: Worker-level evidence. *The Quarterly Journal of Economics*, *129*(4), 1799–1860.

Borusyak, K., Hull, P., and Jaravel, X. (2022). Quasi-experimental shift-share research designs. *The Review of Economic Studies*, *89*(1), 181–213.

Burstein, A., and Vogel, J. (2017). International trade, technology, and the skill premium. *Journal of Political Economy*, *125*(5), 1356–1412.

Crozet, M., and Orefice, G. (2017). *Trade and labor market: What do we know?* (CEPII Policy Brief No. 2017-15). CEPII.

Currie, J., Voorheis, J., and Walker, R. (2020). What caused racial disparities in particulate exposure to fall? new evidence from the clean air act and satellite-based measures of air quality. https://doi.org/10.3386/w26659

Davidson, C., Martin, L., and Matusz, S. (1999). Trade and search generated unemployment. *Journal of International Economics*, *48*(2), 271–299.

Davis, D., and Harrigan, J. (2011). Good jobs, bad jobs, and trade liberalization. *Journal of International Economics*, *84*(1), 26–36.

Egger, H., and Kreickemeier, U. (2009). FIRM HETEROGENEITY AND THE LABOR MARKET EFFECTS OF TRADE LIBERALIZATION. *International Economic Review*, *50*(1), 187–216.

Egger, H., and Kreickemeier, U. (2012). Fairness, trade, and inequality. *Journal of International Economics*, *86*(2), 184–196. https://doi.org/10.1016/j.jinteco.2011.10.002

Ekholm, K., and Midelfart, K. H. (2005). Relative wages and trade-induced changes in technology. *European Economic Review*, *49*(6), 1637–1663.

Firpo, S., Fortin, N., and Lemieux, T. (2009). Unconditional quantile regressions. *Econometrica*, *77*(3), 953–973. https://doi.org/10.3982/ECTA6822

Fortin, N., Lemieux, T., and Firpo, S. (2011). Decomposition methods in economics. In *Handbook of labor economics* (pp. 1–102, Vol. 4). Elsevier.

Fuente, A. d. l., and Boscá, J. E. (2010). EU cohesion aid to spain: A data set. part i: 2000-06 planning period [Number: 477 Publisher: Barcelona School of Economics]. *Working Papers*.

Fuente, A. d. l., and Boscà, J. E. (2013). EU cohesion aid to spain: A data set part II: 1994-99 planning period [Number: 678 Publisher: Barcelona School of Economics]. *Working Papers*.

Goldberg, P. K., and Pavcnik, N. (2005). Trade, wages, and the political economy of trade protection: Evidence from the colombian trade reforms. *Journal of International Economics*, *66*(1), 75–105.

Goldberg, P. K., and Pavcnik, N. (2007). Distributional effects of globalization in developing countries. *Journal of Economic Literature*, 73.

Grossman, G., Helpman, E., and Kircher, P. (2017). Matching, sorting, and the distributional effects of international trade. *Journal of Political Economy*, *125*(1), 224–264.

Harrison, A., McLaren, J., and McMillan, M. S. (2010). Recent findings on trade and inequality. https://doi.org/10.3386/w16425

Head, K., and Mayer, T. (2021). The united states of europe: A gravity model evaluation of the four freedoms. *Journal of Economic Perspectives*, *35*(2), 23–48.

Helpman, E., and Itskhoki, O. (2010). Labour market rigidities, trade and unemployment. *Review of Economic Studies*, *77*(3), 1100–1137.

Helpman, E., Itskhoki, O., Muendler, M.-A., and Redding, S. J. (2017). Trade and inequality: From theory to estimation. *The Review of Economic Studies*, *84*(1), 357–405.

Ragonnaud, G. (2023). *The single market at 30 (1993-2023)* (Briefing) [Briefing].

A Appendix

Additional Tables

Table A.1: Aggregate effects
Controls by Assisted Expenditures (AE) by Autonomous Community and Year

	Earnings		Wages		Hours	
$\ln TO_{p,t}$	-0.091	-0.116*	-0.114***	-0.098***	0.029	-0.011
	[0.071]	[0.067]	[0.034]	[0.026]	[0.076]	[0.067]
ln Total AE	0.001		0.001*		0.000	
	[0.001]		[0.001]		[0.001]	
ln AR Emp Creation		0.000		-0.000		0.001
		[0.001]		[0.000]		[0.001]
ln AE Human Res		-0.001		0.001***		-0.003***
		[0.001]		[0.000]		[0.001]
ln AE Other		0.002		0.001**		0.000
		[0.001]		[0.001]		[0.001]
ln AE Prod Infrac		0.003***		-0.000		0.003***
		[0.001]		[0.000]		[0.001]
ln AE Public Inv		0.001		0.001*		-0.000
		[0.001]		[0.000]		[0.001]
ln AE Resi Dev		-0.007***		-0.002***		-0.005**
		[0.002]		[0.001]		[0.002]
	First Stage Results					
$\ln TO^{C}_{p,t}$	0.660***	0.661***	0.660***	0.661***	0.660***	0.661***
	[0.113]	[0.114]	[0.113]	[0.114]	[0.113]	[0.114]
Observations	4,421,499	4,421,499	4,372,739	4,372,739	4,372,739	4,372,739

Note: This table displays the first and second stages of the IV estimation after we include controls for the quantity of the assisted expenditures (in log of thousands of euros) received from the EU by Autonomous Community and year. Province-level clustered standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Aggregate effects
Controls by Grants (G) by Autonomous Community and Year

	Earnings		Wages		Hours	
$\ln TO_{p,t}$	-0.090	-0.114*	-0.113***	-0.096***	0.029	-0.011
	[0.071]	[0.066]	[0.033]	[0.025]	[0.076]	[0.066]
ln Total Grants	0.001		0.001*		0.000	
	[0.001]		[0.001]		[0.001]	
ln G Emp Creation		-0.000		-0.001*		0.001
		[0.001]		[0.000]		[0.001]
ln G Human Res		-0.001		0.001***		-0.003***
		[0.001]		[0.000]		[0.001]
ln G Other		0.002		0.002***		0.000
		[0.001]		[0.001]		[0.001]
ln G Prod Infrac		0.003***		-0.000		0.003***
		[0.001]		[0.000]		[0.001]
ln G Public Inv		0.001		0.001		-0.000
		[0.001]		[0.000]		[0.001]
ln G Resi Dev		-0.007***		-0.002***		-0.005**
		[0.003]		[0.001]		[0.002]
			First Stage Results			
$\ln TO^{C}_{p,t}$	0.660***	0.661***	0.660***	0.661***	0.660***	0.661***
	[0.113]	[0.114]	[0.113]	[0.114]	[0.113]	[0.114]
Observations	4,421,499	4,421,499	4,372,739	4,372,739	4,372,739	4,372,739

Note: This table displays the first and second stages of the IV estimation after we include controls for the quantity of the grants (in log of thousands of euros received from the EU by Autonomous Community and year. Column (1) shows the estimates for the period post-entry in the European Single Market. Province-level clustered standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$