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Lark, Olga; Videnord, Josefin

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LUND UNIVERSITY

PO Box 117
221 00 Lund
+46 46-222 00 00

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Department of Economics
School of Economics and Management

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Olga Lark
Josefin Videnord

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Lund University

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Olga Lark[*] Josefin Videnord[†]

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Abstract

We examine whether exposure to gender inequality at export destinations affects the gender wage gap in exporting firms. We motivate the analysis through a stylized model where wages depend on worker productivity, and men have a comparative advantage when trading with gender-unequal countries due to customer discrimination. Empirically, we use high-quality matched employer–employee data from Sweden and calculate how exposed firms are to country-level gender inequality through their export destinations. Although increased export intensity on average leads to a wider within-firm gender wage gap, the effect is entirely driven by trade with gender-unequal countries; we find no impact on the gender wage gap when firms increase their exports to countries with gender-equality levels close to that of Sweden. Female managers, who are most likely to interact with foreign customers, experience the most pronounced negative relative wage effects.

Keywords: Export · International trade · Gender wage gap · Gender inequality · Customer discrimination · Gender inequality index

JEL: J16, J31, F14, F16, F66

[*]Lund University, the Research Institute of Industrial Economics, the Institute of Retail Economics

[†]Uppsala University. Corresponding author. Postal address: Department of Economics, Box 513, 751 20 Uppsala, Sweden. Email address: josefin.videnord@nek.uu.se

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1 Introduction

Despite a steady decline in recent decades, the gender wage gap remains resilient with significant variations across countries. Globalization, at the same time, increasingly exposes workers in exporting firms to markets with varying levels of gender equality. Although earlier studies have analyzed the connection between gender equality and globalization from several angles, the estimates and explanations regarding the impact of exports on the gender wage gap are still inconclusive, calling further attention to the mechanisms underlying this relationship (Bøler *et al.*, 2018; Bonfiglioli and De Pace, 2021; Halvarsson *et al.*, 2022b). We contribute to the literature by proposing that the impact of increased exports on the within-firm gender wage gap depends on the gender equality of the destination country.

Our study is motivated by the notion that firms' participation in international trade increases the number of direct interactions between domestic workers and foreign buyers.[1] Female workers may have restricted opportunities to generate revenues for their firms when interacting with customers who are primarily used to doing business with male counterparts. Hence, gender-biased customer discrimination, in the style of Becker ([1957] 1971), may create a comparative advantage for male workers in the exposed firms, leading to an elevated gender wage gap therein. Such effects could also arise in firms that operate in an otherwise gender-equal environment and remunerate workers in proportion to their performance. That is, the potential unequal treatment of female workers would stem from the trade partner side rather than the employer side. In this paper, we formalize the above idea in a stylized model and empirically investigate whether export linkages cause female wage penalties in firms selling to gender-unequal markets.

In the empirical analysis, we use matched employer–employee data from Sweden from 1997 to 2015. Sweden offers an interesting setting to study the nexus between gender inequality and trade since it is one of the most gender-equal countries, besides being a small, trade-dependent economy with an export value of around 50 percent of its GDP.[2] We also have access to wage data for a substantial part of the Swedish private sector workforce, which enables us to look at actual wages instead of income. As a broad metric of gender inequality in the destination countries, we use the well-established Gender Inequality Index (GII) developed by the United Nations.[3] We generate firm–year specific destination weights based on the share of the firm's annual sales directed to each destination, including nation-

[1]See the literature on the importance of business travels and in-person meetings, e.g., Bernard *et al.* (2019), Battiston *et al.* (2020), Söderlund (2020), and Startz (2021).

[2]The World Economic Forum, Global Gender Gap Report, 2022; The World Bank, 2022 [https://wits.worldbank.org/CountryProfile/en/SWE]

[3]As a sensitivity check, we also use the Gender Gap Index (GGI) from the World Economic Forum, as well as the subindices of the GII.

ally within Sweden. We then calculate firms' export-weighted exposure to gender inequality by multiplying the country weights with each destination's gender inequality index. Finally, we estimate the gender-specific wage impact of the obtained weighted exposure score.

To identify the effect of interest, we estimate a wage regression with fixed effects for each employer–employee match and for each firm–year observation. By doing so, we exploit within-firm variation in wages and export patterns while holding selection on the worker, firm, and match level constant. Additionally, the main empirical model is extended by including even more granular fixed effects. First, we add firm–year–occupation fixed effects, and later, firm–worker–occupation fixed effects.[4] These occupation-specific fixed effects further control for variation in wages from shocks to particular occupations within the firm and from workers switching occupations while employed at the firm.

Our main finding is that exports to gender-unequal destinations increase the within-firm gender wage gap. Disregarding the destination dimension, we find that increased exports negatively affect female relative wages on average. We show, however, that this negative effect is entirely driven by firms exporting to customers in gender-unequal countries. Increased exports to other countries, which are about as gender-equal as Sweden, have no impact on the gender wage gap. The estimated magnitudes are of clear economic importance: if a firm shifts all of its sales from the most gender-equal destination (Denmark) to one of the most gender-unequal destinations (Saudi Arabia), female relative wages will fall by approximately 14 percent. Notably, the gender wage gap at mean wages in our sample is 10 percent, which further underlines the nontrivial magnitude of the observed effects.

We find the most pronounced negative wage impacts for female managers, who appear to be particularly exposed to the gender inequality of export partners. The result for managers aligns with the insights from our stylized model, where the female workers most involved in trade with gender-unequal partners will face the most considerable wage penalties due to customer discrimination. Although we also find adverse effects for other female white-collar workers, these are only about a third as large as the estimated effects on female managers' wages. For blue-collar workers, we do not detect any significant change in the gender wage differential when exports to gender-unequal destinations surge.

The main result is robust to various sample cuts and specification tests. It is proven to hold when removing small firms, non-manufacturing firms, and workers with short tenure from the sample. In addition to being robust to different sample restrictions, the estimated effect stays intact when including controls for firm-level profitability and imports, and when

[4]Since switching an occupation might be an outcome of its own, restricting workers to stay in the same occupation might potentially introduce bias to our estimates. Hence, the models with additional occupational fixed effects serve as auxiliary rather than main specifications.

controlling for the overall income levels of trade partners. Additionally, we also account for several mechanisms discussed in the previous literature on exporting and the gender wage gap and show that the effect of destination gender inequality on female relative wages stays intact.

Earlier studies have analyzed the effects of trade liberalization and tariff cuts on gender-specific labor market outcomes, see e.g., Juhn *et al.* (2014) and Sauré and Zoabi (2014). Our findings add to the study of globalization and gender and, foremost, to the literature on exports and the remuneration of men and women. Specifically, we contribute by examining how the gender inequality of trade partners might translate to the gender wage gap in exporting firms. Using matched employer–employee data from Norway, Bøler *et al.* (2018) show that college-educated females experience a wage penalty relative to their male colleagues in exporting firms. They attribute the finding to lower temporal flexibility and commitment among women, which are important for firms operating across different time zones. We establish that accounting for such time-zone effects does not affect our results.

A related strand of the literature has emphasized the role of gender-specific skills in understanding the interaction between exports and the gender wage gap. Bonfiglioli and De Pace (2021) point to female comparative advantages in social skills as an explanation for their finding that exports reduce the gender wage gap for German white-collar workers while increasing the gap for blue-collar workers. Relatedly, Halvarsson *et al.* (2022b) find, using the same Swedish data as in our paper, that export of goods that require tight buyer–seller interaction (higher degree of contract intensity) widens the within-firm gender wage gap. We reaffirm that the contract intensity of traded goods is relevant for the wage gap, but accounting for this channel does not influence our main findings.

We also connect to the literature studying how foreign ownership—another important aspect of globalization—allows for transferring of cultural and gender norms across international borders and thereby leads to non-neutral effects on gender-specific labor market outcomes (Kodama *et al.*, 2018; Tang and Zhang, 2021; Halvarsson *et al.*, 2022a). The overall message of this literature is that foreign investors transplant their corporate culture and gender norms to foreign affiliates, affecting wages and labor market participation of women in the host countries.[5] In contrast to the foreign ownership literature, we focus on the role

[5] In a broader sense, our paper also connects to the literature on gender inequality in the labor market (Altonji and Blank, 1999; Blau and Kahn, 2000; Goldin, 2014), the literature on differences in psychological attributes and bargaining power across genders (Olivetti and Petrongolo, 2016; Card *et al.*, 2016; Blau and Kahn, 2017), the literature on globalization and wages (Helpman *et al.*, 2010; Akerman *et al.*, 2013; Autor *et al.*, 2013; Helpman *et al.*, 2017), as well as to the literature on exporting and wages (Bernard and Jensen, 1995; Bernard and Bradford Jensen, 1999; Schank *et al.*, 2007; Munch and Skaksen, 2008; Irarrazabal *et al.*, 2013; Krishna *et al.*, 2014; Macis and Schivardi, 2016; Barth *et al.*, 2016; Helpman *et al.*, 2017; Bødker *et al.*, 2018; Frías *et al.*, 2022).

of firm export activity in shaping its gender wage gap. To clean our estimates from the potential effects of foreign ownership, we exclude foreign-owned firms from the main analysis and consider only domestically owned exporters.[6]

Overall, the empirical evidence on internationalization and gender inequality has been diverging with findings of positive, negative, or no effects of exporting on the gender wage gap. We contribute to the literature by documenting that increased exports to gender-biased destinations widen the within-firm gender wage gap among exporters when destination and source countries differ in their equality levels. We show that international trade may generate negative externalities across countries by transferring gender inequality and affecting workers in the most gender-equal countries. Together with previous findings of the opposite effects in an unequal low-income country setting[7], the evidence suggests that the processes of globalization and gender equality convergence are deeply intertwined. This paper elicits the idea that, in an increasingly globalized world, a universal shift in attitudes toward higher gender equality is crucial to achieving the full potential of gender parity in society and the labor market.

The rest of the paper proceeds as follows. Section 2 presents a stylized theoretical framework that illustrates how gender inequality at export destinations may enter a worker's wage through customer discrimination. Next, in section 3, we connect the theoretical framework to the empirical strategy and demonstrate the wage equations we estimate. The data is described in section 4, while our results and robustness checks are presented in section 5. Finally, section 6 concludes.

2 Theoretical Framework

This section outlines a stylized theoretical framework that captures the idea that the gender wage gap in exporting firms may depend on gender equality in destination countries. Notably, the effects we model may arise even if an exporting firm is profit-maximizing and pays its workers in proportion to their productivity. The proposed partial equilibrium model helps us to visualize the mechanism we have in mind of how gender inequality at export destinations may spill over to the wage of an individual worker. The model also motivates the functional form and the sets of control variables (fixed effects) used in the empirical analysis.

We propose an augmented version of a standard bargaining wage-setting framework (see, e.g., Card *et al.* (2016)) where the worker's wage at a given firm is equal to a weighted

[6]The foreign-owned firms are included in the sample as a robustness check and are proven not to influence any of the main findings significantly.

[7]See, e.g., Khoban (2021).

average of the revenue productivity (the inside value of the employer–employee match), and the market value (the outside option). The log wage (w_{ijt}) of individual i at firm j at time t can therefore be expressed as follows:

$$w_{ijt} = \Omega_i + \theta\big(r_{ijt} - \Omega_i\big) \tag{1}$$

where Ω_i is the worker i's outside option, θ is worker bargaining power (between 0 and 1), and r_{ijt} is revenue productivity from a given employer–employee match at time t. Productivity is match-specific, and all surplus is shared in a predetermined manner between the worker and the firm, as in, e.g., Fredriksson *et al.* (2018) and Jäger *et al.* (2020).

To introduce differences in productivity across genders, we assume that female workers produce less revenue than their male colleagues when exporting to gender-unequal destinations due to customer discrimination in the style of Becker ([1957] 1971). Thus, the revenue productivity (r^d_{ijt}) by worker i for firm j in destination d and year t can be written as:

$$r^d_{ijt} = a_{ij} + b^d_{jt} + \psi \times Female_i \times GI^d_t \tag{2}$$

where a_{ij} is worker match-specific productivity, b^d_{jt} is firm efficiency in destination d, $Female_i$ is a dummy variable for being a female, GI^d_t is the gender inequality in destination d, and finally, ψ is the weight on customer discrimination, similar to the customer discrimination coefficient in Becker ([1957] 1971)). We model the gender-specific impact of customer discrimination (ψ) as constant. Still, it is natural to assume that its relevance will differ depending on worker involvement in communication with the export partner. We will explore how the customer discrimination coefficient varies across occupations in the empirical analysis.

From the destination-specific revenue productivity expression, the log wage (rent sharing) of individual i working at a firm j at time t can be rewritten as:

$$w_{ijt} = \Omega_i + \theta\left(\sum_{d=1}^{N} \omega^d_{jt} r^d_{ijt} - \Omega_i\right) \tag{3}$$

where ω^d_{jt} is the share of sales to destination d. The destination-specific revenue productivity, r^d_{ijt}, is weighted by ω^d_{jt} and summed across all destinations, $d = 1, .., N$. Equation (3) can now be expressed as the following empirical wage equation:

$$w_{ijt} = \mu_{ij} + \eta_{jt} + \beta \times Female_i \times \sum_{d=1}^{N} \left(\omega^d_{jt} \times GI^d_t\right) \tag{4}$$

where μ_{ij} equals to $(1-\theta)\Omega_i + \theta a_{ij}$ and collects the employer–employee match-specific terms. The firm–year-specific terms are, in turn, collected in $\eta_{jt} = \theta \sum_{d=1}^{N} \omega^d_{jt} b^d_{jt}$. In the regression model outlined in section 3, both μ_{ij} and η_{jt} are estimated by employer–employee match and firm–year fixed effects, respectively. The estimated coefficient β captures the gender-specific wage impact of customer discrimination in the destination countries. It arises as a combination of θ, the bargaining power, and ψ, the effect of customer discrimination at the export destination. Our framework does not allow separating the two effects, but since θ is between 0 and 1, a negative β should necessarily imply a negative ψ. Moreover, we assume θ to be similar across genders, and thus θ will only impose a scaling effect on β.[8]

The model allows for the possibility that firms with higher productivity in a particular market self-select into that market. To rule out that all firms would perfectly sort themselves into the most profitable destination, we implicitly assume matching frictions on the product–destination market as in the international trade model by Eaton *et al.* (2022).[9] Any firm-specific productivity effect will also be captured by the firm-year (η_{jt}) fixed effects.

In a similar vein, the model allows for assortative matching of workers across firms. It implies that workers with certain characteristics (gender, ability, or skills) are more likely to be employed by firms operating in a particular market. As before, the identifying variation is guaranteed by implicitly assuming frictions that prevent workers from perfectly sorting across firms when they change their customer mix. Imperfect labor adjustments due to search and matching frictions are standard in the literature, see in particular Black (1995) and Rosén (2003) on the models with search frictions, where discrimination in some firms but not others generates adverse wage effects rather than perfect segregation of workers. In addition, the impact of worker–firm match-specific attributes is captured by the μ_{ij} fixed effects included in the model.

The proposed stylized model imposes a structure on the relationship between wages, foreign sales, destination-specific firm revenues, and customer discrimination. The model also serves as a basis for the empirical specification described in detail in the next section. Importantly, the inclusion of both employer–employee match (μ_{ij}) and firm–year (η_{jt}) fixed effects in the empirical specification allows us to address the potential endogeneity and selection concerns when estimating the effects of interest.

[8] In the empirical framework, we control for the possibility that men and women have different bargaining power by adding an interaction term, Female$\times$ln(Sales), reflecting if rents, in general, are shared differently across genders.

[9] Eaton *et al.* (2022) document that product market frictions are as important as "iceberg" trade costs in hampering trade flows.

3 Empirical Framework

3.1 The Measure of Export Exposure to Gender Inequality

To estimate the effect of increased exports to gender-unequal countries on female relative wages (the coefficient β in equation (4)), we need a firm-specific and time-varying proxy for gender inequality at export destinations. We construct such measure, the export-weighted gender index of firm j at time t, as follows:

$$GI_{jt} = \sum_{d=1}^{N-1} [\omega_{jt}^{d} \times GI_{t}^{d}] \tag{5}$$

where we weigh the destination-specific gender index, GI_t^d, by the firm j export share to destination d and then sum over all destinations, $d = 1, .., (N-1)$. The weights are denoted by ω_{jt}^d and constructed as the ratio of exports to a destination d in year t over the total export value of the firm j in the same year. Formally,

$$\omega_{jt}^{d} = \frac{Export_{jt}^{d}}{TotalExport_{jt}} \tag{6}$$

In equation (4) of the stylized model, domestic sales are implicitly included in the destination-specific gender index while summing across all destinations $d = 1, .., N$. Specifically, the domestic market is treated as one of the firm's destinations. On the other hand, the empirical gender index from above, GI_{jt} (equation (5)), does not account for domestic sales. To correct for that, we obtain a measure of Gender-Inequality-Weighted Sales (GIWS) in the next step as follows:

$$GIWS_{jt} = GI_{jt} \times EI_{jt} + GI_{t}^{SWE} \times (1 - EI_{jt}) \tag{7}$$

where firm export intensity, EI, is defined as the total export value over total sales in a particular year. The $GIWS_{jt}$ measure represents a sum of the two terms: the export-value weighted gender index, GI_{jt}, interacted with the firm export intensity EI_{jt}, and a domestic gender index, i.e., the gender index for Sweden GI_t^{SWE}, interacted with the share of domestic sales, $(1 - EI_{jt})$. Conceptually, the $GIWS$ reflects the firm overall exposure to gender inequality and discrimination through its sales, be it domestic or foreign, and constitutes an essential building block of the empirical analysis.[10] A higher value of $GIWS$ implies a larger share of the total firm sales going to gender-unequal destinations, whereas we capture sales to all markets, including the Swedish domestic market.

[10]In what follows, we omit subscript jt on the $GIWS_{jt}$ and GI_{jt} for brevity.

3.2 Empirical Model

The empirical model builds on the empirical wage equation (4) extended with the $GIWS$ measure from equation (7). Using the full set of matched employer–employee data, we estimate the following model:

$$\begin{aligned} ln(wage)_{ijt} =& \beta[Female_i \times GIWS_{jt}] \\ &+ \mu_{ij} + \eta_{jt} \\ &+ \mathbf{X'}_{it}\gamma + \mathbf{F'}_{jt}\phi \\ &+ \varepsilon_{ijt} \end{aligned} \tag{8}$$

where β is the main coefficient of interest which shows the effect of the $GIWS$ on female relative wages. As emphasized in section 2, the key elements of our identification strategy are the employer–employee match fixed effects, denoted by μ_{ij}, and firm–year fixed effects, denoted by η_{jt}. The employer–employee match fixed effects care for workers' sorting into firms. Otherwise, a potential sorting of workers with certain abilities or characteristics to firms exporting to certain destinations could bias our estimates of the effect on female relative wages. The worker and firm fixed effects, embedded in the match fixed effects, also deal with possible biases due to time-constant worker and firm characteristics, for example, individual worker ability or firm wage-setting practices. Additionally, the firm–year fixed effects deal with firm-specific shocks, for example, import supply shocks or financial shocks in a given year which, if not accounted for, would confound the estimate of β. Overall, our identification strategy relies on the assumption that after controlling for match and firm–year fixed effects and observable worker characteristics, other shocks that could impact workers' wages are orthogonal to a firm's choice of export destinations and export behavior in these destinations. Hence, the key identifying assumption we make is that firms' export patterns (as captured by $GIWS$) are unrelated to other time-varying gender-specific shocks to wages.

The fact that we exploit the variation in wages from within employer–employee match keeps the within-firm gender composition constant; it allows us to study what happens to the within-firm gender wage gap when exports to gender-unequal partners intensify. Concerns about reversed causality and endogeneity of exports are mitigated in our setting since firm export decisions are unlikely to be influenced by a single worker. To estimate the empirical regression models with high-dimensional fixed effects, we use an algorithm developed by Correia (2016), which takes care of the dimensionality problem induced by the multiple levels of fixed effects.

In addition to the match and firm–year fixed effects, we include control variables at the individual level, denoted by $\mathbf{X}_{it}$, that vary over time. The vector includes the potential labor market experience (*Experience*) and its square (*Experience*^{2}), a dummy variable for having children in the household between 0 and 18 years old (*Children*), and a dummy variable for working in a white-collar occupation (*White collar*). Control variables at the firm level, included in the vector $\mathbf{F}_{jt}$, are subsumed by the firm–year fixed effects unless interacted with the female dummy variable. Our baseline specification includes two such interactions: an interaction of the female dummy and firm export intensity, and an interaction of the female dummy and the log of firm sales. ε_{ijt} denotes the error term. The standard errors are clustered at the firm level.

In section 5, we display alternative specifications with even more granular fixed effects, which include occupations at the three-digit level. Specifically, we add firm–year–occupation fixed effects to adjust for heterogeneous effects across occupations within a firm. These fixed effects are intended to control for occupation-specific productivity shocks affecting a firm in a given year. Secondly, we include match–occupation fixed effects to care for workers switching occupations within the firm in response to a shock. However, the models with occupation-specific fixed effects are not considered our main specification since that might introduce bias to the estimates. Labor force adjustments within the firm, for example, workers switching occupations, serve as a potential mechanism through which changes in trade partners affect the gender wage gap. We return to this issue in section 5.

4 Data and Descriptive Statistics

4.1 Main Data Sources

In analyzing gender inequality at export destinations and exporters' gender wage gap, we use high-quality matched employer–employee data from Statistics Sweden, covering the years 1997–2015. The wage data we use stem from the Wage Structure Statistics (WSS), an annual labor force survey carried out by the Swedish National Mediation Office. From the survey, we get information on workers' full-time equivalent monthly wages in the survey month, as well as the contracted working hours. The monthly wage includes the agreed-upon wage plus amenities, bonuses, and variable incomes, but not over-time payments. The WSS survey covers all workers in private sector firms with 500 employees or more and more than 50 percent of the remaining workforce is surveyed every year. We also observe detailed occupational codes for the workers on a three-digit level.

We then match WSS survey information to the Structural Business Statistics (FEK), which yields a sample of approximately two million workers per year or half of all private sector employment. Due to the sampling of smaller firms in the wage survey, there are gaps in the data. Since an employee may be employed by another firm during the missing years, we do not impute values. From the FEK, we also collect information for all private non-financial companies on sales, profits, value-added, and industry affiliation.

Information on all workers and their socio-demographic characteristics, such as gender, age, education, and the number of children 0-18 years old, are gathered from the longitudinal integrated database for health insurance and labor market studies (LISA). The actual labor market experience is not available in LISA. Still, we use the available information to construct potential labor market experience as the difference between an individual's age and i) the year since attaining the highest level of education or ii) the total years of education (based on the variable for highest attained education). In cases where neither of the education variables is available, we subtract 16 from an individual's age to measure the potential labor market experience.

Some data are also gathered on the plant level from the labor statistics based on administrative sources (RAMS), namely the location and the number of employees. The plant-level data are aggregated to the firm level. Information on firm-level export of goods comes from the Swedish Foreign Trade Statistics and is broken down by country of destination and the type of goods classification on an eight-digit level. The data cover all compulsory transactions registered by Swedish Customs: all export transactions of goods with countries outside the EU (Extrastat). Furthermore, data on trade with the EU countries are collected via a comprehensive population survey subject to a threshold. The threshold implies that the smallest firms are not included in the data collection procedure. Statistics Sweden complements the trade survey data with information from VAT declarations to the Swedish Tax Agency.

4.2 Sample Restrictions

To arrive at our baseline sample, we introduce some additional restrictions to the data. All workers between 18 and 67 years old are connected to the firm where they earned their highest yearly income using a unique identifier. Part-time workers may introduce biases to the estimate of the gender wage gap and are excluded from the analysis (Manning and Petrongolo, 2008; Albrecht *et al.*, 2018).[11] To make a clear distinction between the effect of exporting and the effect of foreign ownership on female relative wages, we exclude foreign-owned firms from the baseline sample but include them in the analysis as a robustness test further on. After the sample restrictions mentioned above, we proceed by adding the measures of gender inequality of export destinations at the firm level.

[11] Information on contracted hours is obtained from the WSS.

4.3 The Gender Inequality Index

We measure gender inequality across countries with the well-established Gender Inequality Index (GII) constructed by the United Nations. The GII is available for most countries for the study period (1997–2015). There exists some variation in the availability of the index across years, but, as a general rule, we use the latest available data for each country by imputing values for the missing years. We believe that imputation is justified since we do not expect significant, sudden changes in the country's GII from one year to the next.

Table 1. Descriptive Statistics: Raw GII

Most Gender-Equal Countries			*Least Gender-Equal Countries*		
	Rank	Mean		Rank	Mean
Denmark	1	0.066	Brazil	41	0.481
Netherlands	2	0.079	Turkey	42	0.490
Switzerland	3	0.080	Algeria	43	0.525
Finland	4	0.082	Indonesia	44	0.542
Norway	5	0.088	Iran, Islamic Rep.	45	0.556
Belgium	6	0.107	Morocco	46	0.599
Germany	7	0.115	Egypt, Arab Rep.	47	0.607
Spain	8	0.117	Saudi Arabia	48	0.615
Austria	9	0.131	India	49	0.624
France	10	0.137	Pakistan	50	0.644
Sweden		0.054			
OECD		0.259			
World		0.495			

Note: The ranking is based on the 50 largest export destination countries for Swedish exporting firms over the 1997–2015 period. The reader can find the complete list of Sweden's 50 largest export destinations and their corresponding gender indices in Table A1 in the Appendix.

The GII embraces three distinct dimensions of gender inequality: health, empowerment, and labor market participation across genders. For each of these overarching areas, different indicators are used as building blocks to arrive at the broad and representative metric of gender inequality across countries. The health indicators include maternity mortality ratio and adolescence birth rate; the empowerment indicators include the female and male populations with at least secondary education and female and male share of parliamentary seats; and, for the labor market participation, the measures of female and male labor force participation rates are used. From the indicators mentioned above, a separate gender index is built for males and females, a combination of which yields the GII. A higher value of the GII implies that a higher gender inequality characterizes a country.

To illustrate the extent of gender inequality that Swedish exporters in our sample are exposed to, Table 1 ranks Sweden's 50 largest export destinations according to their raw GII value. As expected, other Nordic and Western European countries remain among the ten most gender-equal Swedish export destinations. On the other hand, countries in Asia, Africa, and South America are among Sweden's least gender-equal trade partners. Importantly, we use all Swedish export destination countries when constructing our empirical gender indices. In contrast, countries in Table 1 are only meant to illustrate the spectrum of gender inequality that Swedish exporters are exposed to through their international operations.[12]

4.4 Descriptive Statistics

Table 2 shows the descriptive statistics for the firm-level variables in Panel A, and the worker-level variables in Panel B. Firm size is defined as the total number of employees attached to the firm in our sample, and the mean firm size is 196 employees. To obtain the export intensity variable, we divide total goods exports by total sales. Panel A demonstrates that the mean export intensity among the analyzed firms is 21 percent, while the median export intensity is 6 percent. The female share of the labor force is the number of female workers through the total number of workers in the firm. It is 26 percent at the mean and 20 percent at the median, suggesting that most firms we study are male-intensive. We observe 5,171 unique firms in the sample, yielding 24,954 firm–year observations.

Summary statistics of the gender indices are also presented in Panel A in Table 2. The mean (median) export-weighted gender index GI is 0.15 (0.12), while the mean (median) gender-inequality-weighted sales $GIWS$ is 0.08 (0.06). Figure 1(a) displays the distribution of the GI in our sample, which appears right-skewed, with the bulk of observations below 0.4. Figure 1(b) instead shows the distribution of $GIWS$, which is even more right-skewed with a thin tail of large $GIWS$ values. Most firms in our sample exhibit a value of $GIWS$ below 0.3.

Panel B in Table 2 displays the average sample values of the worker-level variables. The average monthly wage is €3,299, and the gender wage gap is about 10 percent. On average, women earn €3,056, whereas men earn €3,373. Female workers are slightly younger and less experienced than their male colleagues. The share of workers with children below 18 years old is 0.43, with no significant difference across genders. Furthermore, 17 percent of the workers in the sample have attained a college degree. This number is 21 percent among women, and 15 percent among men, indicating that women, on average, are more

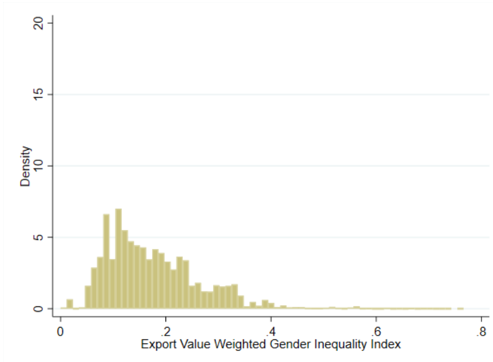
[12] Table A1 in the Appendix lists all 50 destination countries receiving the largest share of Swedish exports, ranked based on the raw GII value.

educated than their male colleagues. Female workers tend to be much more represented in white-collar occupations, with 64 percent, as opposed to their male counterparts, with 46 percent. For the analysis, the total number of workers we observe is 830,031, which yields 4,895,953 worker–year observations. As indicated above, the sample consists of more male workers (610,271) than female workers (219,760), reflected in the worker–year observations being around 3.8 million for men and 1.1 million for women.

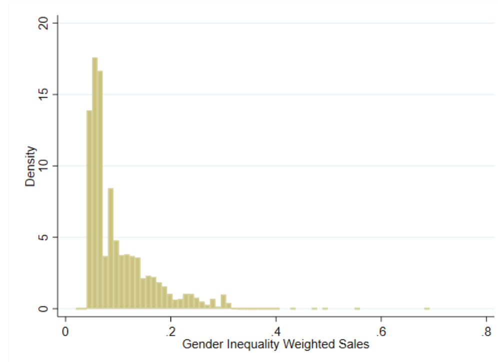
Table 2. Descriptive Statistics

Panel A. Firm-level Statistics	Mean	Median	SD
Firm size (number of employees)	196	763	899
Sales (mln €)	7,629	969	35,909
Export/Sales	0.21	0.06	0.27
GI	0.15	0.12	0.09
GIWS	0.08	0.06	0.04
Female share of labor force	0.26	0.20	0.19
Panel B. Individual-level Statistics	All	Female	Male
Monthly Wage (€)	3,299	3,056	3,373
Monthly Wage (log)	8.04	7.97	8.06
Experience	21.02	19.73	21.41
Age	42.09	41.52	42.26
Share with children	0.43	0.42	0.43
Share with college education	0.17	0.21	0.15
Share of white-collar workers	0.51	0.64	0.46
Share of blue-collar workers	0.49	0.36	0.54
Number of individuals	830,031	219,760	610,271
Number of individual−year obs	4,895,953	1,139,217	3,756,736

Notes: On the firm level, all numbers are based on the panel of firm-level data in our sample of domestic exporting firms for 1997–2015. On the individual level, all numbers refer to average values of the indicated variables for the panel of worker-level data for 1997–2015.



(a) Distribution of GI



(b) Distribution of GIWS

Figure 1. Distributions of GI and GIWS

5 Results

5.1 GIWS and the Gender Wage Gap

To establish whether gender inequality can spread through international trade and impact the gender wage gap in exporting firms, we start with the model outlined in equation (8), where we regress log wages on the $GIWS$ measure. Table 3 displays the main results, and in column (1), we document an estimated coefficient of Female×GIWS of -0.195. Since $GIWS$ captures firm exposure to gender inequality through its total sales, we augment the model with an interaction of export intensity and the female dummy. In column (2), Female×Export is intended to control for the direct effect of exports on female relative wages, along with the $GIWS$.[13] When directly controlling for export intensity, we find a slightly more negative estimate of Female×GIWS, -0.25. The negative estimate implies that the gender wage gap increases when firms export a larger proportion of their total sales to gender-unequal destinations. To make sense of the magnitude, a 10 percentage point increase in $GIWS$ (an increase in $GIWS$ of 0.1) would yield an estimated 2.9 percent decrease in female relative wages. Putting it in context, if a firm that used to export all of its sales to Denmark (ranked number one in Table 1) would now export all of its sales to Saudi Arabia (ranked 48 in Table 1), the average female relative wage would decrease by approximately 14 percent.

[13] We prefer the model specification in column (2) of Table 3 to its alternatives and will refer to it as our main, or baseline, specification throughout the rest of the paper.

Table 3. Main Results: GIWS

Dep. var: ln(Wage)	(1)	(2)	(3)	(4)
Female×GIWS	-0.195***	-0.250***	-0.146***	-0.117***
	(0.025)	(0.036)	(0.023)	(0.021)
Female×Export		0.019***	0.0001	0.003
		(0.009)	(0.011)	(0.009)
Match FE	yes	yes	yes	no
Firm×Year FE	yes	yes	no	no
Firm×Year×Occup. FE	no	no	yes	yes
Match×Occup. FE	no	no	no	yes
Adj. R^2	0.93	0.94	0.93	0.95
Observations	4,895,953	4,895,953	4,812,942	4,433,872

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. When constructing occupational fixed effects, missing occupations are grouped into one category. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Robust standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

To control for occupation-specific shocks within the firm and to remove any changes in the gender wage gap mediated through this channel, we add firm–year–occupation fixed effects in column (3). The estimated impact of $GIWS$ is reduced to -0.146, indicating that some of the overall negative impact of $GIWS$ stems from shocks to particular occupations, making men and women differently exposed. Referring to the previous example, a swap in exclusive exporting from Denmark to Saudi Arabia would decrease female relative wages by around 8 percent, according to the estimated effect in column (3). The fact that the average effect on female relative wages might hide substantial heterogeneity across occupations urges us to investigate this channel in greater detail in section 5.2, where we pay particular attention to female managers.

As outlined in section 3.2, a final extension of the main model is the inclusion of match–occupation fixed effects in column (4) of Table 3. The model with match–occupation fixed effects and firm–year–occupation fixed effects represents our most stringent specification. In this specification, we only compare the wages of female and male colleagues working at the same firm and holding the same occupation over time, in addition to controlling for occupation-specific shocks affecting the firm. The model with an extensive number of fixed effects is very restrictive. However, it still underlines our previous finding: the estimated coefficient on female relative wages, -0.117, is similar to the estimate in column (3).[14] One

[14]The sample size decreases somewhat between columns (2) to (3) of Table 8, and even more so between columns (3) to (4). The reason behind the reduction in sample size is that some occupations may not be present in a firm for more than one year or that workers are only observed in the same occupation in a given firm for only one year.

should remember that the specifications with occupational fixed effects are very demanding on the data and leave us with a thin level of variation in wages. Additionally, since switching an occupation might be an outcome of its own, restricting workers to stay in the same occupation might potentially introduce bias to the estimated effects and should therefore be interpreted with caution. For these reasons, we keep the specification in column (2) as our main specification throughout the paper.[15]

An important control variable included in all specifications is the interaction term between the female dummy and the firm sales in log form. The variable aims to capture if men and women have different bargaining power (θ) and, thus, if rents are shared differently across genders. As shown in Table A2 in the Appendix, the interaction term lacks statistical significance in all specifications. This result confirms that the theoretical assumption of similar bargaining power of men and women holds, at least in our sample.

In Table 3, all the estimated coefficients of Female×GIWS are statistically significant at the one percent significance level. The estimated effects also display non-trivial magnitudes, with large economically meaningful effects for workers most exposed to gender inequality on behalf of trading partners.[16]

5.2 Female Managers

The negative average impact of $GIWS$ on female relative wages identified in section 5.1 may potentially hide substantial heterogeneity across occupational groups. We explore this in Table 4, where workers are divided into three occupational categories: managers, other white-collar workers, and blue-collar workers. As seen in column (1), the negative effect on female managers' wages is substantial and more pronounced than the effect on other white-collar workers in column (2), also when considering the precision of the estimated effects. We explain the observed strong response in manager wages by managers being more involved in exporting activities and communication with foreign customers, and hence more exposed to potential gender inequality and customer discrimination. For other female white-collar workers, the negative wage effect is about a third as large as the estimated effect on female managers' wages. We detect no significant impact of increased exports on the relative wages of female blue-collar workers, as indicated in column (3).

[15]Table A2 in the Appendix displays additional fixed-effects specifications and estimates for all control variables included in the model.

[16]The insensitivity of our findings to different levels of clustering of the standard errors is shown in Table A3 in the Appendix. A higher level of clustering of the standard errors at the 2-digit industry level still yields statistical significance at the 1 percent level.

Table 4. Occupations

Dep. var: ln(Wage)	Managers (1)	Other White Collar (2)	Blue Collar (3)
Female×GIWS	-0.341***	-0.119***	-0.048
	(0.122)	(0.029)	(0.042)
Female×Export	0.024	-0.013	0.005
	(0.034)	(0.015)	(0.007)
Match FE	yes	yes	yes
Firm×Year FE	yes	yes	yes
Adj. R^2	0.96	0.94	0.81
Observations	280,241	2,131,867	2,405,140

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. The sample is split into three groups based on the worker's occupation: managers (1), other white-collar workers (2), and blue-collar workers (3). Additional control variables included in all specifications are Experience, Experience^2/100, Children, and Female×ln(Sales). Robust standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

The evidence in Table 4 is conclusive: female managers tend to be most affected when the firm exports increasingly to gender-unequal destinations. The effect we find for the other white-collar category indicates that workers in other occupations, possibly the ones interacting with customers, also tend to be influenced by gender-biased export partners, forcing their wages to decline with more exposure. Our findings are reminiscent of the emerging literature on glass ceiling effects for women and gender gaps in promotions due to internationalization.[17]

5.3 Export Partner Gender Inequality and the Gender Wage Gap: An Alternative Empirical Model

The $GIWS$ measure, used in the empirical model in section 3.2, assumes a certain structure of how gender (in)equality of foreign partners and gender equality of domestic partners translates to female relative wages. In this section, we reformulate the empirical model slightly to obtain a more direct effect of exports on wages, depending on the gender inequality of the destination country. The model is otherwise similar to equation (8), but the double interaction Female×GIWS is now replaced with a triple interaction of the female dummy, firm export intensity, and the export-weighted gender index, GI. The log wage of individual i working at firm j at time t is now given by:

[17] See, e.g, Heyman *et al.* (2018)

$$
\begin{aligned}
w_{ijt} = &\beta_1[Female_i \times EI_{jt} \times GI_{jt}] \\
&+ \beta_2[Female_i \times EI_{jt}] \\
&+ \beta_3[Female_i \times GI_{jt}] \\
&+ \mu_{ij} + \eta_{jt} \\
&+ \mathbf{X'}_{it}\gamma + \mathbf{F'}_{jt}\phi \\
&+ \varepsilon_{ijt}
\end{aligned}
\tag{9}
$$

where the main coefficient of interest is the slope coefficient of the three-way interaction, β_1, indicating a combined effect of export intensity and gender inequality on female relative wages. Moreover, β_2 measures the impact of increased export intensity on female relative wages, evaluated at the mean GI, and β_3 is the coefficient of the interaction between the female dummy and the export-weighted gender index. The wage equation in (9) is otherwise specified exactly as in equation (8) with employer–employee match fixed effects, μ_{ij}, and firm–year fixed effects, η_{jt}. Advantageously, equation (9) allows us to visualize the marginal effects in a figure, illustrating the estimated effects of increased export intensity on wages for various levels of the GI.

The estimated effects from equation (9) are shown in Table 5. In column (1), we first display the average effect on female relative wages from increased export intensity, which is not interacted with the export-weighted gender index. The estimated coefficient (-0.029) aligns with the effects found earlier in the literature.[18] Specifically, if a firm shifts all of its sales from the domestic market to the international market, female relative wages would, on average, decrease by approximately 3 percent. This finding suggests that increased export intensity, on average, leads to a wider gender wage gap.

In column (2) in Table 5, we add the triple interaction with the GI to establish whether the average export effect differs across the level of gender equality of destination countries. The coefficient of the three-way interaction is -0.235, which corresponds to a 13 percent decrease in female relative wages if a firm exporting all of its sales shifts its destination from Denmark to Saudi Arabia. Column (2) also displays the estimates of the double interactions Female×Export and Female×GI, but they are small in magnitude and not statistically significant. All specifications in Table 5 include the standard employer–employee match and firm–year fixed effects. As before, specifications in columns (3) and (4) are augmented with firm–year–occupation and match–occupation fixed effects, respectively. Once occupational fixed effects are added, the corresponding three-way interaction estimates are -0.138 and

[18]See Bøler *et al.* (2018) for results for Norway, and Halvarsson *et al.* (2022b) for Sweden.

-0.100 in columns (3) and (4), respectively.[19]

Table 5. Alternative Model: GI

Dep. var: ln(Wage)	(1)	(2)	(3)	(4)
Female×Export×GI		-0.235***	-0.138***	-0.100***
		(0.037)	(0.022)	(0.020)
Female×Export	-0.029**	-0.011	-0.014	-0.012
	(0.014)	(0.007)	(0.009)	(0.008)
Female×GI		-0.007	0.001	0.005
		(0.007)	(0.006)	(0.006)
Match FE	yes	yes	yes	no
Firm×Year FE	yes	yes	no	no
Firm×Year×Occupation	no	no	yes	yes
Match×Occupation	no	no	no	yes
Adj. R^2	0.93	0.93	0.94	0.95
Observations	4,895,953	4,895,953	4,812,942	4,433,872

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. When constructing occupational fixed effects, missing occupations are grouped into one category. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Robust standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

Figure 2 illustrates the negative effect on female relative wages from intensified exports to gender-unequal destinations—the main result from column (2) in Table 5. In addition to the point estimates and the 95 percent confidence bounds, Figure 2 also plots the density of the export-weighted gender index. We observe a few spikes among the more gender-equal parts of the distribution (to the left) and a slight decrease of the mass towards the most gender-unequal parts of the *GI* distribution (to the right). In Figure 2, we hold the *GI* constant and only allow the export intensity to vary. Hence, an increase in export intensity generates a negative but not statistically significant point estimate of around -0.01 when measured at the mean level of the gender index (0.15).[20]

Figure 2 clearly illustrates that there are no wage effects from increased exports to gender-equal countries (the left tail of the *GI* distribution). However, when exports to gender-unequal countries surge (the right tail of the *GI* distribution), female relative wages appear to fall. The strong response from exports to gender-unequal destinations explains the overall negative effect we have previously established on female wages. Taken together, the results show that intensified exports to gender-biased countries generate statistically significant wage

[19] Table A4 in the Appendix displays our findings for the *GI* model with the three-way interaction across different levels of fixed effects, as well as estimates of all the control variables included in the models.

[20] The estimate corresponds to the coefficient for Female×Export in column (2) of Table 5, where the *GI* is held constant at its mean sample value.

penalties for female workers, which sheds further light on the mechanisms at play.[21]

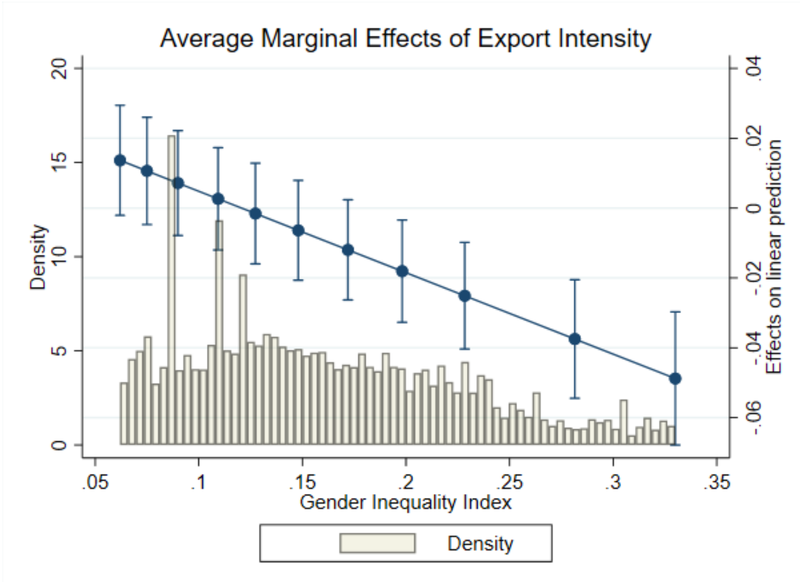


Figure 2. Export-Weighted Gender Index

5.4 Robustness

To investigate our findings' robustness, we apply various restrictions to the baseline sample. In column (1) of Table 6, we estimate the benchmark model on a sample of large firms, that is, firms with at least 50 employees. Notably, only about 250,000 observations in the sample of 4.9 million observations belong to firms with less than 50 employees in our sample. As expected, excluding small firms leaves the main estimated effect essentially unchanged. We move on by excluding workers with less than five years of tenure at a firm to test whether the adverse wage effect we find could be driven by newly hired workers or workers with short tenure. The estimate in column (2) rules out this possibility. If anything, the results are stronger for workers staying longer at the firm. This finding relates to the literature on managers' earlier experiences and firm export patterns.[22] If the identified effect only stemmed from a new manager entering the firm and changing both firm export decisions and wages, it is unlikely that we would find an even stronger effect when excluding newly hired employees.[23]

[21]Figures A1 and A2 in the Appendix show marginal effects from non-linear, quadratic, and cubic specifications of equation (9). We also present results from using a standardized version of the *GI* in Table A5 and Figure A3 in the Appendix. The findings do not appear to be sensitive to the functional form of the gender index.

[22]See, e.g., Mion *et al.* (forthcoming) and Meinen *et al.* (2022).

[23]Table A6 in the Appendix provides more results across employment tenure groups.

In column (3), we consider only manufacturing firms, for which we find that the effect of increased exposure to gender inequality is approximately the same magnitude as before. In column (4), we almost double the estimation sample by including foreign-owned firms.[24] The impact on female relative wages becomes slightly less pronounced, with a coefficient of -0.22 compared to -0.25 in our main specification in column (2) of Table 3. Despite the slightly attenuating effect when including foreign-owned firms, we deem our main finding robust to firm ownership status. In column (4), we exclude firms with less than 10 percent of female workers to establish whether firms with low female worker shares drive our findings. In a similar vein, we exclude firms with less than 10 percent of women among managers in column (5). As apparent from Table 6, our findings are insensitive to the exclusion of firms with a low share of female workers or a low share of female managers.

Table 6. Robustness I: Sample Restrictions

Dep. var: ln(Wage)	Empl.>= 50 (1)	Tenure>= 5 (2)	Manufacturing (3)	Fng incl. (4)	Fem. Share> 0.1 (5)	Fem. Manager> 0.1 (6)
Female×GIWS	-0.262***	-0.293***	-0.260***	-0.220***	-0.219***	-0.223***
	(0.037)	(0.046)	(0.035)	(0.029)	(0.034)	(0.033)
Female×Export	0.021**	0.031**	0.017*	0.019***	0.012	0.009
	(0.010)	(0.015)	(0.009)	(0.006)	(0.009)	(0.015)
Match FE	yes	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes	yes
Adj. R^2	0.93	0.95	0.94	0.93	0.94	0.95
Observations	4,635,036	1,888,065	2,585,525	9,115,603	4,235,337	2,869,597

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. The following sample restrictions are applied, as indicated by the column headings: firms with more than 50 employees (1); employees with five or more years of tenure at the firm (2); manufacturing firms (3); both domestic and foreign-owned exporting firms (4); firms with more than 10% of female workers (5); firms with more than 10% of female managers (6). Additional control variables included in all specifications are Experience, Experience^2/100, Children, College, White collar, and Female×ln(Sales). Standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

In sum, Table 6 provides conclusive evidence that the estimated effects are stable under different sample restrictions. The solid pattern across specifications in Table 6 makes us confident in our main conclusion: when firms intensify their sales to gender-unequal destinations, the female relative wages decrease, yielding an increase in the within-firm gender wage gap.

5.4.1 Irregular Export Behavior

A potential concern is that firms' irregular export behavior might generate attenuation bias to our findings. It is well recognized that many exporting firms sell abroad at low intensity,

[24]See Appendix Table A7 for results when the sample is split by firm ownership status.

with export values close to zero.[25] As illustrated by Figure 3, we observe a similar pattern in our sample, where a handful of firms exhibit moderate to small export intensity. To account for that, in Table 7, we focus on firms following more stable exporting patterns and exclude firms hovering just around the zero export value.

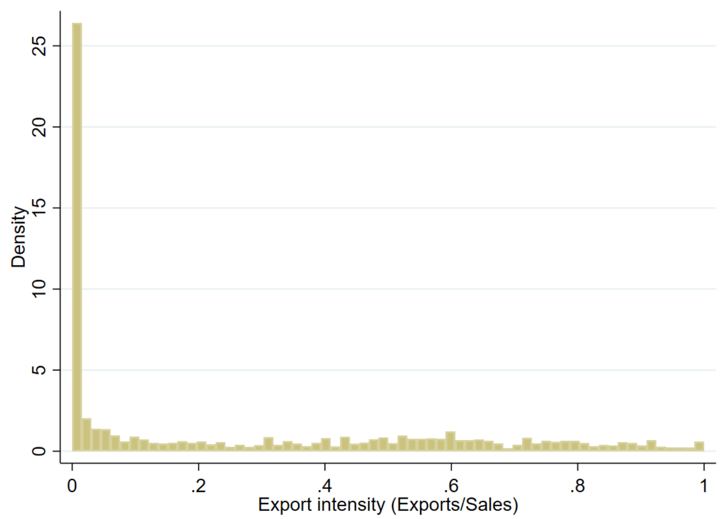


Figure 3. Density of Export Intensity

In Table 7, each column subsequently excludes firms with the lowest degree of export intensity. Leaving out firms with export intensity below one percent cuts the sample by approximately two million observations and removes the spike just above zero in Figure 3. In columns (2) and (3), firms below two and three percent export intensity are excluded, respectively, and in column (4), we remove firms with less than five percent of their total sales abroad. Finally, column (5) omits firms with an export intensity below ten percent. The impact of intensified trade with gender-unequal partners remains largely unaffected. The coefficient of Female×GIWS is -0.235 in column (1) and stays around this magnitude throughout Table 7. Similarly, the precision of the estimated effect also remains intact throughout this robustness exercise. Noteworthy, we observe a large drop in observations as we move from the baseline sample in Table 3 to the restricted sample in column (1) in Table 7, but further restrictions of the sample do not alter the number of observation in any significant way. For example, going from a one percent cutoff in column (1) to a ten percent cutoff in column (5) only generates a loss of approximately 560,000 worker–year observations. In general, the sensitivity analysis of firm export behavior largely confirms our previous findings.[26]

[25] As before, we measure export intensity as a fraction of exports to total sales.

[26] For the sensitivity analysis of the $GIWS$ measure, see Table A8 in the Appendix, where we winsorize

Table 7. Robustness II: Export Intensity

	Exclude if Export Intensity Below				
	[0.01]	[0.02]	[0.03]	[0.05]	[0.1]
Dep. var: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Female×GIWS	-0.235***	-0.234***	-0.233***	-0.235***	-0.237***
	(0.033)	(0.033)	(0.033)	(0.033)	(0.034)
Female×Export	0.015*	0.014*	0.014	0.014	0.014
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes
Adj. R^2	0.94	0.94	0.94	0.94	0.94
Observations	2,967,699	2,838,820	2,755,461	2,618,544	2,402,701

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

5.4.2 Alternative Gender Indices

In this section, we investigate whether our main findings are sensitive to the choice of the gender index. We start by replacing the GII with the GGI—a composite measure of gender equality from the World Economic Forum. The GGI is available from 2006 onward and is intended to measure the extent of gender equality at the country level. The index covers four main themes: economic participation and opportunity, educational attainment, health and survival, and political empowerment. An essential difference between the GII and the GGI is that the latter does not account for women being better off than men in any area.[27] To make the two indices comparable, we reverse the values of the GGI such that larger values of the index reflect a higher level of gender inequality. Similar to the GII, we impute values for missing years to utilize the full range of employer–employee observations in our sample.

Column (1) in Table 8 corresponds to column (2) in Table 3 in section 5.1, but now the $GIWS$ is constructed with the GGI instead of the GII. The estimated coefficient of Female×GIWS is -0.393 in column (1).

and trim the variable.

[27] The construction method of the GGI implies that areas, where women are better off compared to men, will not discount the areas where women are worse off compared to men. For example, women having a higher share of seats in parliament does not compensate for the skewed educational attainment in favor of men. The conceptual differences in the construction of the indices influence their distributions such that countries look more gender-unequal as measured with the GGI compared to the GII. Figure A4 in the Appendix shows the distribution of the (reversed) GGI. Furthermore, the correlation between the two export-weighted gender indices is 0.75 in our sample.

Table 8. Robustness III: Alternative Gender Indices

	GGI	GII Subindices			
		LFP	Empowerment	Seats in Parliament	Secondary Educ.
Dep. var: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Female×GIWS	-0.393***	-0.420***	-0.132***	-0.132***	-0.099***
	(0.096)	(0.101)	(0.027)	(0.036)	(0.017)
Female×Export	0.012	0.008	0.008	0.006	-0.013
	(0.009)	(0.009)	(0.013)	(0.015)	(0.011)
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes
Adj. R2	0.93	0.93	0.93	0.93	0.93
Observations	4,895,899	4,895,953	4,895,953	4,895,953	4,895,953

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Subindices of the GII, pertaining to women and indicated in the column headings, are used to construct the GIWS measure. The values of the GGI and the GII subindices are reversed to match the interpretation of the baseline measure. Additional control variables included in all specifications are Experience, Experience^2/100, Children, College, White collar, and Female×ln(Sales). Standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

In addition to the GGI, we also control whether our results hold under alternative measures of gender inequality. Specifically, in columns (2)-(5) in Table 8 we present the results using four sub-indices of the GII. Labor force participation (LFP) in column (2) is the reversed female labor force participation rate (1 - Female LFP) to keep the same interpretation as for the overall GII and GGI, where higher values imply less equality. Likewise, Empowerment in column (3) is the reversed measure of female empowerment, which combines the other two indicators - Seats in parliament and Secondary education. The reversed measures of female share of seats in parliament and female secondary education are included separately in columns (4) and (5) in Table 8.

The main takeaway from Table 8 is that it does not seem to matter for the main conclusion exactly which measure of gender inequality one uses. Hence, using an alternative gender index in the construction of the exposure measure provides qualitatively similar estimates and thus further confirms our findings.

5.5 Relation to Mechanisms in the Previous Literature

The previous literature on exports and the gender wage differential has highlighted several mechanisms that may explain why exporters would favor male worker wages. To account for this evidence, we in Tables 9, 10, and 11 sequentially control for channels underlined in the related research to establish whether the mechanism proposed in this paper—gender inequality at export destinations—still matters.

We start by examining the mechanism suggested by Bøler *et al.* (2018), in which female workers are penalized by exporters due to the lack of flexibility in working hours and commitment. First, we exclude workers who may experience more time constraints than others.

The categories of excluded workers in Table 9 are i) workers with children aged 0–6 (column (1)), ii) workers with children aged 0–18 (column (2)), and iii) workers under the age of 45 (column (3)). In essence, we leave out workers with children and young workers who are more likely to plan for children and may therefore opt for jobs offering flexible working arrangements. Throughout columns (1)–(3), we observe that workers with children, or young workers below the age of 45, are not driving our findings.

Table 9. Worker Temporal Flexibility

Dep. var: ln(Wage)	No Child 0-6 (1)	No Child 0-18 (2)	Age>44 (3)	BHO (4)	Time Zone FE (5)
Female×GIWS	-0.296***	-0.279***	-0.299***	-0.249***	-0.251***
	(0.032)	(0.031)	(0.027)	(0.036)	(0.036)
Female×Export	0.029***	0.031***	0.029***	0.018**	0.019**
	(0.009)	(0.007)	(0.010)	(0.009)	(0.009)
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes
Time Zone FE	no	no	no	no	yes
Adj. R2	0.94	0.93	0.96	0.93	0.95
Observations	3,884,669	2,752,158	2,059,343	4,895,953	4,895,953

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Column headings in (1)-(3) indicate the group of workers included in the analysis. The business hours overlap index as constructed by Bøler *et al.* (2018) is included as a control variable in column (4), and a set of time zone fixed effects interacted with the female dummy variable are included in column (5). Additional control variables included in all specifications are: Female×ln(Sales), Experience, Experience^2/100, and White Collar. Robust standard errors clustered at firm-level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We continue controlling for the flexibility hypothesis in column (4) of Table 9. Specifically, we add the business hours overlap (BHO) index following Bøler *et al.* (2018), which is intended to account for the temporal flexibility required by exporting firms when operating across different time zones. Similar to their measure, the BHO index is constructed at the firm level as a trade-weighted average of the BHO, where the weights are the number of products exported to each time zone. Essential for our conclusions, the effect of trading with gender-biased customers stays intact when controlling for the BHO index.

In column (5), we add time zone fixed effects to address the same concern as with the BHO index. Adding fixed effects for the time zones a firm trades with times a female dummy does not change the effect of Female×GIWS. The findings in Table 9 indicate that although we control for female temporal flexibility in different ways, there is still a significant effect from trading with gender-biased customers.

We continue to test for other mechanisms that could be at play in Table 10. Column (1) displays the baseline estimate of Female×GIWS found earlier in Table 3. In column (2), we augment the model with the contract intensity index, which is shown to affect the gender wage gap in globalized firms (Halvarsson *et al.*, 2022b). The contract intensity index after Nunn (2007) reflects the share of differentiated, as opposed to homogeneous, goods exported by a firm and intends to capture the extent of interpersonal contact needed in international transactions. The contract intensity index is a time-fixed index at the industry level, and therefore similar to all firms in the same industry. Goods contract intensity appears to exert a negative and statistically significant effect on female relative wages but yields no changes to the estimate of Female×GIWS, compared to the baseline estimate in column (1).

Table 10. Other Mechanisms

Dep. var: ln(Wage)	Baseline (1)	CI Index (2)	Profitability (3)	GDP (4)	Imports (5)
Female×GIWS	-0.250***	-0.229***	-0.252***	-0.246***	-0.252***
	(0.036)	(0.042)	(0.036)	(0.035)	(0.036)
Female×Export	0.019*	0.022***	0.020**	0.018*	0.019**
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
Female×Export×CI		-0.062**			
		(0.030)			
Female×ln(Profitability)			-0.001*		
			(0.000)		
Female×GDP				0.000***	
				(0.000)	
Female×ln(import)					0.000
					(0.001)
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes
Adj. R^2	0.93	0.93	0.93	0.93	0.93
Observations	4,895,953	4,895,953	4,895,953	4,895,953	4,656,233

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Additional control variables are added to the model as indicated by the column headings: (1) Baseline, (2) the Contract Intensity Index, (3) ln(Profitability), (4) GDP of export destinations, and (5) ln(Imports). Additional control variables included in all specifications are Experience, Experience^2/100, Children, College, White collar, and Female×ln(Sales). Standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

A firm-level measure of profitability is added in column (3) to address the issue that men tend to claim higher rents than women as a firm becomes more profitable (see, e.g., Card *et al.* (2016)). Controlling for firm-level profitability does not alter our main result in any significant way. Noteworthy is that we already include a control variable for unequal rent sharing between men and women, Female×ln(sales), in all specifications. The interaction variable should already capture if men and women gain differently from increases in firm sales. As shown in column (3), adding an additional gender-specific control for profitability on top of this does not alter the estimated effect of Female×GIWS compared to the baseline.

Another concern is that GIWS might reflect the general development of the economy at export destinations rather than its gender norms and equality. To test that, we augment the model with an export-weighted GDP of the firm's export partners. As apparent from column (4), we do not find any evidence that our exposure measure, $GIWS$, captures the overall economic development rather than the level of gender inequality of the trade partners. Finally, in column (5), we add the log of imports to account for the potential role it might have in shaping the gender wage gap.[28] This robustness check does not disturb our earlier findings.

Table 11. Trade Within and Outside of the European Union and Norway

Dep. var: ln(Wage)	GII (1)	GGI (2)
Female×GI(EU)×Export(EU)	-0.287**	-0.456***
	(0.129)	(0.156)
Female×GI(nonEU)×Export(nonEU)	-0.254***	-0.332**
	(0.064)	(0.151)
Female×Export(EU)	-0.014*	-0.003
	(0.008)	(0.009)
Female×Export(nonEU)	0.001	-0.001
	(0.015)	(0.015)
Female×GI(EU)	-0.065**	-0.154***
	(0.031)	(0.037)
Female×GI(nonEU)	-0.007	-0.002
	(0.005)	(0.007)
Match FE	yes	yes
Firm×Year FE	yes	yes
Adj. R^2	0.93	0.93
Observations	3,947,762	3,947,762

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Additional control variables included in all specifications are Experience, Experience^2/100, Children, College, White collar, and Female×ln(Sales). Robust standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

Another potential concern is that the geographical distance between Swedish firms and their export destinations drives our findings. We examine this possibility by dividing total exports into exports to the European Union and Norway and exports to the rest of the world. Table 11 shows results from this exercise. Suppose it is indeed gender inequality, rather than distance, standing behind our findings. In that case, we expect to obtain negative and statistically significant effects of exports to the EU countries on female relative wages. The coefficients are -0.287 and -0.254 for EU and non-EU exports. The non-EU estimate is

[28]See Khoban (2021) on the impact of firm imports on female labor market outcomes in Indian firms.

estimated with better precision and is statistically significant at the 1 percent level, while the EU coefficient is slightly noisier, although still statistically significant at the 5 percent level. The results in Table 11 confirm our hypothesis and show that both exports to gender-unequal countries within and outside the EU yield a wider gender wage gap.

Taken together, the results from Tables 9, 10, and 11 confirm that the mechanism we identify is robust to the alternative explanations, and that gender inequality transferred from export destinations appears to non-trivially contribute to the gender wage gap in globalized firms.

6 Conclusions

We evaluate the impact of gender inequality at export destinations on the gender wage gap in exporting firms. To construct the firm-level measure of exposure to gender inequality of trading partners, we utilize the well-established gender inequality index by the United Nations. To guide our analysis, we outline a stylized partial equilibrium model showing how customer discrimination on behalf of export partners may spill over to female wages in exporting firms, even if these firms are otherwise gender-equal.

In the empirical analysis, we document that increased export to gender-unequal destinations widens the gender wage gap in exporting firms. The finding is of clear economic importance: if a firm shifts all of its sales from the most gender-equal destination in our sample (Denmark) to one of the most gender-unequal destinations (Saudi Arabia), female relative wages decrease by approximately 14 percent. In addition, we document an average negative effect of increased exports on female relative wages. The average negative effect is, however, entirely driven by firms working with gender-biased partners; for firms exporting mainly to countries of similar equality levels, we detect no impact on the gender wage gap. The main finding is robust to different model specifications, sample restrictions, and alternative measures of gender inequality.

We show that the estimated negative effect on female relative wages is most pronounced for female managers. A possible explanation behind this finding is that managers are more exposed to gender inequality at export destinations through their communication with foreign partners and involvement in exporting activities. Although we also find adverse effects for other female white-collar workers, these are only about a third as large as the effects for managers. For female blue-collar workers, the effects are small and insignificant.

As a final note, the proposed stylized model represents partial equilibrium in a subset of firms meaning that the effects we identify for the exporting firms correspond to a lower bound of the general equilibrium estimates. In a general equilibrium setting, the decreasing female relative wages from exports to gender-unequal destinations would reduce demand for female labor and hence the overall level of female wages in the economy. Taking these adjustments into account, we would find an even larger detrimental effect on female relative wages in exporting firms.

Our paper contributes to the debate on how globalization and, in particular, exporting behavior of firms shapes their wage setting. We document that gender inequality of export partners matters for the gender wage gap among exporting firms. The finding elicits a channel through which gender inequality may spread through internationalization—a channel shown to significantly impact the gender wage gap in exporting firms.

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A Tables and Figures

Table A1. Descriptive Statistics: Raw GII

	Rank	Mean		Rank	Mean
Denmark	1	0.066	Lithuania	26	0.223
Netherlands	2	0.079	Estonia	27	0.250
Switzerland	3	0.080	Latvia	28	0.268
Finland	4	0.082	Hungary	29	0.275
Norway	5	0.088	United States	30	0.276
Belgium	6	0.107	Malaysia	31	0.298
Germany	7	0.115	Russian Federation	32	0.362
Spain	8	0.117	Ukraine	33	0.372
Austria	9	0.131	Chile	34	0.383
France	10	0.137	Argentina	35	0.387
Japan	11	0.138	Romania	36	0.392
Australia	12	0.145	Thailand	37	0.405
Korea, Rep.	13	0.147	Mexico	38	0.425
Canada	14	0.153	South Africa	39	0.457
Italy	15	0.157	United Arab Emirates	40	0.463
Portugal	16	0.168	Brazil	41	0.481
Singapore	17	0.172	Turkey	42	0.490
Israel	18	0.179	Algeria	43	0.525
Ireland	19	0.180	Indonesia	44	0.542
Poland	20	0.183	Iran, Islamic Rep.	45	0.556
Czech Republic	21	0.186	Morocco	46	0.599
Greece	22	0.192	Egypt, Arab Rep.	47	0.607
United Kingdom	23	0.193	Saudi Arabia	48	0.615
Slovak Republic	24	0.202	India	49	0.624
China	25	0.220	Pakistan	50	0.644
Sweden		0.054			
OECD		0.259			
World		0.495			

Notes: The ranking is based on the 50 largest export destination countries for Swedish exporting firms over the 1997–2015 period.

Table A2. Alternative Specifications: GIWS

Dep. var: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Female×GIWS	-0.288***	-0.319***	-0.323***	-0.262***	-0.250***
	(0.061)	(0.062)	(0.062)	(0.038)	(0.036)
Female×Export	0.080***	0.090***	0.090***	0.025**	0.019**
	(0.016)	(0.015)	(0.015)	(0.010)	(0.009)
Female	-0.194***	-0.192***	-0.195***		
	(0.040)	(0.042)	(0.043)		
GIWS	0.020	-0.036		-0.062	
	(0.088)	(0.066)		(0.064)	
Export	0.028*	0.001		0.011	
	(0.015)	(0.014)		(0.012)	
ln(Sales)	0.025***	0.013***		0.011***	
	(0.004)	(0.003)		(0.003)	
Female×ln(Sales)	0.003	0.003	0.003	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Experience	0.017***	0.017***	0.017***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Experience^2	-0.029***	-0.028***	-0.028***	-0.030***	-0.030***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Children	0.027***	0.027***	0.027***	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
College	0.254***	0.246***	0.245***		
	(0.006)	(0.006)	(0.006)		
White Collar	0.237***	0.236***	0.237***	0.034***	0.035***
	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)
ln(Firm Size)	-0.012***	-0.016**		0.013***	
	(0.004)	(0.006)		(0.004)	
Functional region FE	yes	yes	yes	yes	no
Industry×Year FE	yes	yes	yes	yes	no
Firm FE	no	yes	no	no	no
Firm×Year FE	no	no	yes	no	yes
Match FE	no	no	no	yes	yes
Adj R^2	0.57	0.59	0.59	0.94	0.94
Observations	4,895,953	4,895,953	4,895,953	4,895,953	4,895,953

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Robust standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

Table A3. Standard Errors Clustered at Different Levels

	Level of Clustering				
	Firm (Baseline)	Individual	Individual×Firm	Firm×Year	Industry (2-digit)
Dep. var: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Female×GIWS	-0.195***	-0.195***	-0.195***	-0.195***	-0.195***
	(0.025)	(0.010)	(0.010)	(0.019)	(0.029)
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes
Adj R^2	0.93	0.93	0.93	0.93	0.93
Observations	4,895,953	4,895,953	4,895,953	4,895,953	4,895,953

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Column (1) corresponds to column (1) in Table 3 with standard errors clustered at the firm level; robust standard errors are in parentheses. Columns (2)-(5) show estimated effects with different levels of clustering of the standard errors, as indicated by the column headings. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

Table A4. Alternative Specifications: GI

Dep. var: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Female×Export×GI	-0.106	-0.178*	-0.180*	-0.245***	-0.235***
	(0.104)	(0.099)	(0.102)	(0.043)	(0.037)
Female×Export	0.042***	0.050***	0.049***	-0.006	-0.011
	(0.013)	(0.012)	(0.012)	(0.008)	(0.007)
Female×GI	-0.038	-0.023	-0.023	0.003	-0.007
	(0.041)	(0.040)	(0.043)	(0.009)	(0.007)
Export×GI	-0.062	-0.007		-0.039	
	(0.094)	(0.071)		(0.067)	
Female	-0.198***	-0.200***	-0.202***		
	(0.043)	(0.045)	(0.045)		
Export	0.032***	-0.003		0.004	
	(0.010)	(0.010)		(0.008)	
GI	0.047*	-0.013		-0.007	
	(0.027)	(0.014)		(0.009)	
ln(Sales)	0.025***	0.013***		0.011***	
	(0.004)	(0.003)		(0.003)	
Female×ln(Sales)	0.002	0.002	0.002	-0.000	-0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Experience	0.017***	0.017***	0.017***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Experience^2	-0.029***	-0.028***	-0.028***	-0.030***	-0.030***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Children	0.027***	0.027***	0.027***	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
College	0.254***	0.246***	0.245***		
	(0.006)	(0.006)	(0.006)		
White Collar	0.237***	0.236***	0.237***	0.034***	0.035***
	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)
ln(Firm Size)	-0.012***	-0.016**		0.013***	
	(0.004)	(0.006)		(0.004)	
Functional region FE	yes	yes	yes	yes	no
Industry×Year FE	yes	yes	yes	yes	no
Firm FE	no	yes	no	no	no
Firm×Year FE	no	no	yes	no	yes
Match FE	no	no	no	yes	yes
Adj R^2	0.53	0.56	0.57	0.93	0.93
Observations	4,895,953	4,895,953	4,895,953	4,895,953	4,895,953

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Robust standard errors clustered at the firm level are in parentheses. Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

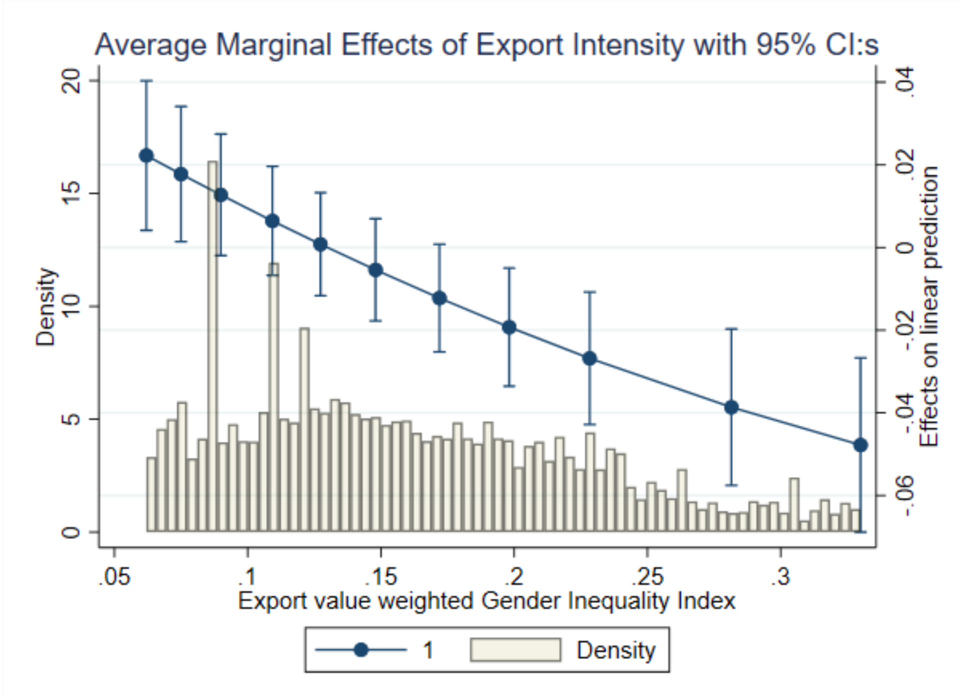

Figure A1. Quadratic GI

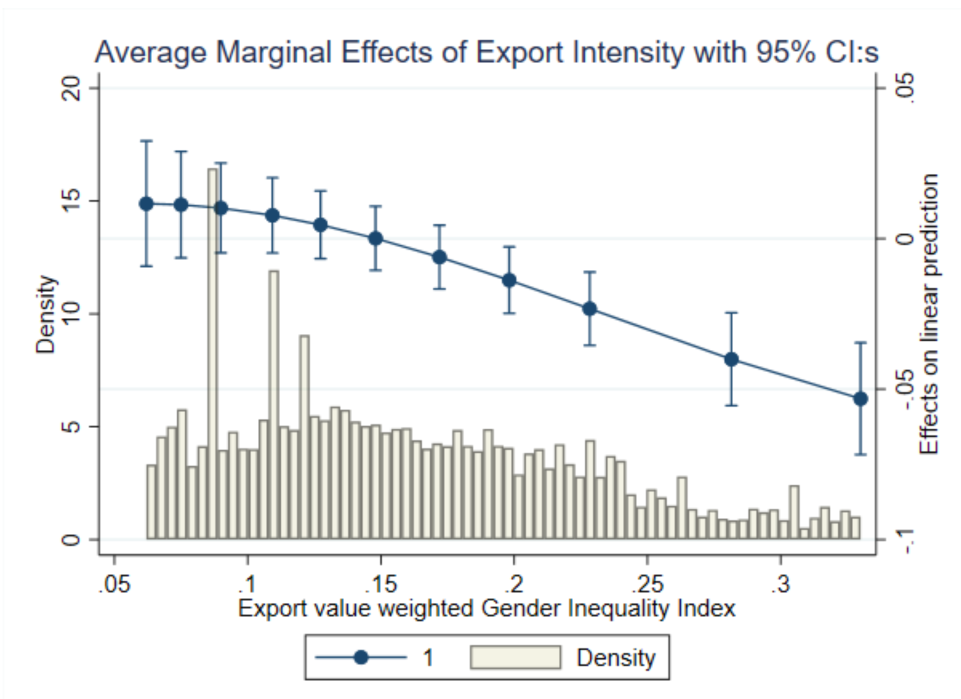

Figure A2. Cubic GI

Table A5. Standardized GI

Dep. var: ln(Wage)	(1)	(2)	(3)	(4)
Female×Export× GI (Std)		-0.021***	-0.012***	-0.009***
		(0.003)	(0.002)	(0.002)
Female×Export		-0.011	-0.014	-0.012
		(0.007)	(0.009)	(0.008)
Female×GI (Std)	-0.002***	-0.001	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Match FE	yes	yes	yes	
Firm×Year FE	yes	yes		
Firm×Year×Occup. FE			yes	yes
Match×Occup. FE				yes
Adj. R^2	0.93	0.94	0.93	0.95
Observations	4,895,953	4,895,953	4,812,942	4,433,872

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. The GI is standardized. When constructing occupational fixed effects, missing occupations are grouped into one category. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

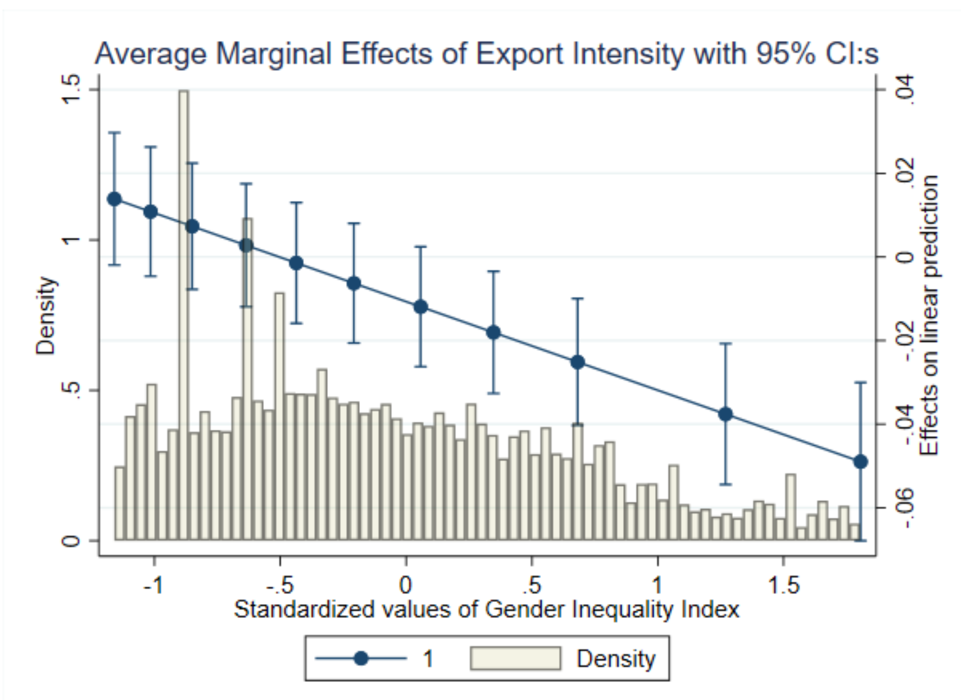

Figure A3. Standardized GI

Table A6. GIWS and Worker Tenure

Dep. var: ln(Wage)	Tenure>1 (1)	Tenure>2 (2)	Tenure>3 (3)	Tenure>4 (4)	Tenure>5 (5)
Female×GIWS	-0.254***	-0.264***	-0.294***	-0.293***	-0.300***
	(0.034)	(0.035)	(0.046)	(0.046)	(0.038)
Female×Export	0.020*	0.025*	0.031**	0.031**	0.037***
	(0.011)	(0.014)	(0.015)	(0.015)	(0.013)
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes
Adj. R^2	0.94	0.94	0.94	0.95	0.95
Observations	3,764,415	2,969,755	2,363,096	1,888,065	1,522,683

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. The sample is restricted to workers with a certain number of years of tenure, as indicated by the column headings. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

Table A7. GIWS and Firm Ownership

	MNEs			Local Firms
	All	Foreign	Domestic	
Dep. var: ln(Wage)	(1)	(2)	(3)	(4)
Female×GIWS	-0.226***	-0.157***	-0.265***	-0.206**
	(0.030)	(0.045)	(0.040)	(0.095)
Female×Export	0.021***	0.018***	0.022*	0.012
	(0.006)	(0.006)	(0.011)	(0.013)
Match FE	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes
Adj R^2	0.93	0.94	0.94	0.93
Observations	7,678,366	4,067,573	3,528,307	1,248,322

Notes: Estimates are based on the worker-level panel data over 1997–2015. The dependent variable is deflated monthly wage in log form. The sample of firms is restricted based on the firm ownership status: columns (1)-(3) display results for multinational exporters, while column (4) presents results for local exporters. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

Table A8. Winsorized and Trimmed GIWS

	Cutoffs				
	[1,99]	[5,95]	[10,90]	[15,85]	[20,80]
Dep. var: ln(Wage)	(1)	(2)	(3)	(4)	(5)
Panel A. Winsorized					
Female×GIWS	-0.258***	-0.221***	-0.220***	-0.255***	-0.253***
	(0.036)	(0.058)	(0.072)	(0.073)	(0.081)
Adj. R^2	0.93	0.93	0.93	0.93	0.93
Observations	4,895,953	4,895,953	4,895,953	4,895,953	4,895,953
Panel B. Trimmed					
Female×GIWS	-0.258***	-0.254***	-0.301***	-0.327***	-0.297***
	(0.037)	(0.057)	(0.068)	(0.075)	(0.084)
Adj. R^2	0.93	0.93	0.93	0.93	0.92
Observations	4,839,718	4,298,430	3,828,969	3,343,307	2,911,757
Match FE	yes	yes	yes	yes	yes
Firm×Year FE	yes	yes	yes	yes	yes

Notes: Estimates are based on the worker-level panel data over 1997–2015, whereas workers employed in domestic exporting firms are considered. The dependent variable is deflated monthly wage in log form. Additional control variables included in all specifications are Experience, Experience^2/100, White Collar, Children, and Female×ln(Sales). Significance levels: *** ($p < 0.01$), ** ($p < 0.05$), and * ($p < 0.1$).

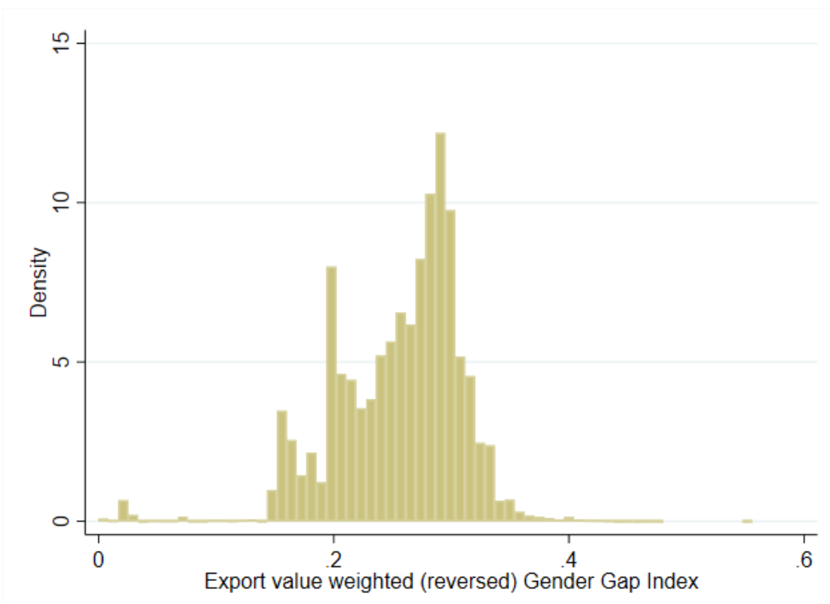

Figure A4. Distribution of the GGI