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Export Scarring After a Trade Ban: A Quasi-natural Experiment from the Algerian Embargo*

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Abstract

Between June 2022 and October 2024, Algeria imposed a politically motivated embargo on imports from Spain. Because neither the imposition of the embargo nor its lifting was anticipated by Spanish firms, this episode provides a quasi-natural experiment to estimate the causal effect of a sudden market shutdown on subsequent export behavior. We interpret this causal effect as evidence on export hysteresis, i.e., the degree to which exporting fails to resume because of market-specific frictions. We show that the embargo reduced the post-embargo probability of exporting to Algeria by 34%. However, this negative effect is smaller than the impact of an observationally equivalent situation in which a firm simply does not export to a market for two and a half years in the absence of an embargo. This finding suggests that studies relying on non-experimental variation may overstate the effect of export hysteresis on the probability of exporting. We also show that the embargo's negative effects continued even one year after it was lifted. Finally, we find that the largest decrease in the probability of exporting occurred in firms with a large pre-embargo market share and long export experience in Algeria.

JEL Codes: F10, F14
Keywords: export hysteresis, embargo, sunk entry costs in exporting, customer accumulation frictions, market experience, Spain, Algeria.

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1 Introduction

Whether a firm exported last year is one of the strongest predictors of exporting this year (Roberts and Tybout, 1997; Bernard and Jensen, 2004; Alessandria et al., 2021). There are two main explanations for export persistence. First, firm characteristics such as productivity, size, and workforce quality are positively correlated with the probability of exporting (Bernard et al., 2007, 2012). Because these attributes tend to persist in the short run, firms whose characteristics enabled them to export last year are likely to be able to export again in the current year. Second, firms' dynamics in foreign markets are shaped by various frictions (Tybout, 2024). Among them are the so-called beachhead sunk costs that firms must incur when entering a market for the first time, such as identifying the health or technical requirements their products must meet. There are also frictions involved in the accumulation of customers. Firms must invest in advertising, identify and visit potential clients, and build brand visibility to encourage customers to switch their purchases to firm products (Arkolakis, 2010; Piveteau, 2021; Fitzgerald et al., 2024). Finally, firms must learn about the appeal of their products abroad (Berman et al., 2019; Eaton et al., 2025) or the reliability of foreign customers (Aeberhardt et al., 2014). If a firm stops exporting, it will likely have to incur sunk costs again to re-enter the market. It also loses inertia in sales to foreign customers, while its market visibility and market knowledge gradually erode. To avoid these negative effects, a firm may continue exporting even when profits in the foreign market fall below the level that originally triggered entry, a phenomenon known as export hysteresis (Baldwin, 1988; Dixit, 1989).

To assess how firm characteristics and frictions contribute to export persistence, empirical studies typically estimate discrete-choice models of firms' export decisions. In these models, the probability of exporting is related to firm attributes and past export experience, with the latter proxying for the presence and magnitude of export frictions. Empirical studies find that past export experience has a large positive effect on the probability of exporting. For example, Roberts and Tybout (1997) and Bernard and Jensen (2004) found that after controlling for firm characteristics, exporting last year increased the probability of exporting between 39 and 60 percentage points.

The main caveat of these analyses is the existence of unobserved firm attributes that are correlated with current and past export decisions. If these unobserved attributes are not controlled for, estimates will tend to overstate the positive impact of past export experience on the probability of exporting and, therefore, the magnitude of export frictions. Although the literature uses different econometric strategies to deal with this limitation, none can fully ensure orthogonality between firm characteristics and previous export experience. To

address this concern, we exploit a quasi-natural experiment to estimate the causal impact of an unexpected temporary market shutdown on the probability of exporting in the future: the Algerian embargo on Spanish imports.

In March 2022, the Spanish government shifted its position on Western Sahara, aligning with Morocco, which claims sovereignty over the region, and antagonizing Algeria, which is in favor of a self-determination referendum. In retaliation, Algeria imposed an embargo on imports from Spain that lasted from June 2022 to October 2024.

This episode provides a suitable environment to estimate the causal impact of a temporary market shutdown on the probability of exporting to that market in the future. First, Spanish firms could not anticipate either the imposition or the lifting of the embargo. Second, conditional on exporting to Algeria, exposure to the embargo was not targeted to specific firms and is plausibly orthogonal to firm characteristics.

We estimate the effect of the embargo on the post-embargo probability of exporting to Algeria using a synthetic difference-in-differences (SDiD) procedure (Arkhangelsky et al., 2021). Specifically, we compare the probability of exporting to Algeria and a control country before and after the embargo, using a synthetic matching procedure to ensure that firms exporting to Algeria and those exporting to the control country followed similar export trends before the embargo. Unlike previous studies, which examine how past export experience raises the probability of exporting, we analyze how an unexpected market shutdown depresses the probability of exporting in the future.

We show that, relative to the control country, the probability of exporting to Algeria in the post-embargo period was 6.6 percentage points lower than in the pre-embargo period, corresponding to a 34% decline in the pre-embargo export probability. This estimate is robust to using alternative control destinations, time windows, frequency of exports, and to controlling for firms' re-routing of exports to circumvent the Algerian embargo. We also show that the embargo's negative effects persisted one year after its removal.

We compare the impact of the Algerian embargo on the post-embargo probability of exporting with the impact of an observationally equivalent situation in which a firm simply does not export to a market for two and a half years, the length of the embargo. We find that the negative effect of a two-and-a-half-year non-export spell is much smaller when identified in our quasi-experimental setting—the Algerian embargo—than when estimated in a standard non-experimental setting. This finding suggests that studies relying on non-experimental variation tend to overstate the effect of past export experience on the probability of exporting, because unobserved firm characteristics remain correlated with both past and current export decisions.

Next, we examine whether re-entry sunk costs, erosion of the customer base, or the deterioration of market-specific knowledge are the main channels through which the embargo affected the post-embargo probability of exporting. Our strategy is to exploit heterogeneity in the embargo's impact across firms and link it to these frictions. To guide the empirical analysis, we develop in Appendix B a simple discrete-choice model in which firms decide whether to re-enter the Algerian market once the embargo is lifted. Consistent with our baseline result, the model predicts that a firm's probability of exporting to Algeria is lower after the embargo than before it. It also predicts that the probability of exporting to Algeria decreases more in firms that enjoyed greater market visibility and a larger customer base, and had longer pre-embargo experience in the Algerian market.

The empirical evidence is in line with the model's predictions. We find that the probability of exporting to Algeria fell more among firms with long pre-embargo export experience and those with a large pre-embargo market share in Algeria, with the decline being more pronounced for firms with longer experience than for those with larger market shares. By contrast, we do not find evidence that re-entry sunk costs played a major role in driving the negative impact of the embargo. Overall, our results suggest that preserving market visibility and experience, and maintaining inertia in sales to foreign customers are key motives for firms to persist in exporting.

Our paper is related to the large literature on exporter dynamics (Alessandria et al., 2021). Specifically, our paper is related to the empirical literature that has analyzed the contribution of firm characteristics and previous export experience to the probability of exporting (Roberts and Tybout, 1997; Bernard and Jensen, 2004; Moxnes, 2010; Timoshenko, 2015; Máñez et al., 2020). Our contribution is to use a quasi-natural experiment, the Algerian embargo, to estimate the causal effect of an unexpected market shutdown on the probability of exporting to that market in the future. We show that the embargo lowered the post-embargo probability of exporting, but that this reduction is smaller than the impact inferred from an observationally equivalent two-and-a-half-year non-export spell in a standard non-experimental setting. This comparison suggests that studies relying on non-experimental variation tend to overstate the effect of past export experience on the probability of exporting. We also present evidence consistent with the view that firms' lower post-embargo probability of exporting to Algeria was driven by a loss of market visibility and market-specific knowledge. In this way, our results contribute to the literature on the sources of export persistence (Timoshenko, 2015; Piveteau, 2021; Eaton et al., 2025).

Our paper also contributes to the literature on how firms respond to trade shocks (Fitzgerald and Haller, 2018; Albornoz et al., 2021; Fernandes and Winters, 2021) and to the litera-

ture on the effects of trade sanctions and boycotts on trade flows (Felbermayr et al., 2021; Korovkin and Makarin, 2023; Syropoulos et al., 2024; de Lucio et al., 2025). We show that an embargo depresses exports to the sanctioned market not only while it is in force, but also after it has been lifted. In the Algerian case, the negative effect of the embargo had not fully disappeared almost one year after the embargo was lifted. This post-embargo effect arises because a market shutdown erodes the forces that sustain export persistence, such as the inertia in foreign buyers' purchasing decisions.

The remainder of the paper is organized as follows. The next section explains the motivation for the Algerian embargo on Spanish products and its effects on the value of exports and the number of firms exporting to Algeria. Section 3 explains the identification mechanism, presents the econometric results on the impact of the embargo on the post-embargo probability of exporting to Algeria, compares it with that estimated in a non-experimental setting, and performs some robustness analyses. Section 4 explores the reasons for the negative impact of the Algerian embargo on the post-embargo probability of exporting to Algeria. The last section concludes.

2 The Algerian embargo

In March 2022, the Spanish Prime Minister sent a letter to the King of Morocco announcing that Spain considered the Moroccan government's proposal to grant autonomy to Western Sahara as the best option to resolve the conflict in this region.[5] This letter marked a change in the previous neutral position of Spain with respect to its former colony.[6] Once the letter was made public, the Algerian government, which supports a referendum on self-determination in Western Sahara, expressed its discomfort with the Spanish decision and recalled its ambassador from Madrid.

Algeria retaliated after the Spanish government confirmed its change of position with respect to Western Sahara in the Spanish Congress on 8 June 2022.[7] First, it suspended a 20-year friendship treaty with Spain. Second, the Algerian Association of Banks and Financial Entities sent instructions to freeze all direct debits for import operations with Spain. As explained by Feás (2022), an Algerian firm that wants to import a product worth more than around 600 euros must send the import invoice to a bank 30 days before the

[5] See Miguel González: "La carta de Pedro Sánchez a Mohamed VI", El País, March 23, 2022.

[6] The change in the Spanish position was explained by the need to reach an agreement with Morocco with respect to the control of immigration flows into Spain. See Francisco Peregil and Miguel González: "España toma partido por Marruecos en el conflicto del Sáhara", El País, March 18, 2022.

[7] See Diario de Sesiones del Congreso de los Diputados, June 8, 2022.

shipment, deposit 120% of the value of the invoice, and receive a clearance document from the Algerian Export Promotion Agency. By forbidding direct debits for import operations, the Algerian government established a de facto embargo.[8] The embargo was not a minor economic measure for Algeria, given that Spain was its third-largest supplier of goods in 2021, accounting for 6.4% of total imports.[9]

The embargo on Spanish imports lasted until 7 November 2024, when the Central Bank of Algeria instructed banks to resume trade-related financial operations with Spain. For our empirical analysis, we treat July 2022–October 2024 as the embargo period, since July was the first month the embargo was in force during the entire month and November 2024 was the first month in which trade gradually resumed.

It is very unlikely that Spanish firms could have anticipated the change of position of the Spanish government with respect to Western Sahara and subsequent Algerian retaliation. Spain had maintained a neutral position with respect to the Western Sahara conflict for 47 years. Furthermore, in 2022, 30% of Spanish gas imports came from Algeria. In March 2022, the month in which Spain announced its change in position with respect to Western Sahara, gas prices were particularly high due to the post-Covid-19 economic recovery and the Russian invasion of Ukraine. It was surprising that the Spanish government chose to antagonize a critical energy supplier at that moment.

It is also unlikely that firms anticipated the end of the embargo. Algeria lifted its embargo on Spanish products on 7 November 2024, at a time when some media outlets reported that Algeria had imposed an embargo on France.[10] In July 2024, the French Prime Minister stated that the Moroccan proposal was the only option to resolve the Western Sahara conflict, and he reiterated this position in an address to the Moroccan Parliament on 29 October 2024. In response to this stance, and as mentioned above, the Algerian Association of Banks and Financial Entities allegedly ordered banks to suspend all import and export transactions with France.[11] It appears that Algeria sought to mitigate the negative economic impact of a potential embargo on French products, the second main source of imports for Algeria, by lifting the embargo on Spain. The lifting of the embargo may also have been facilitated by

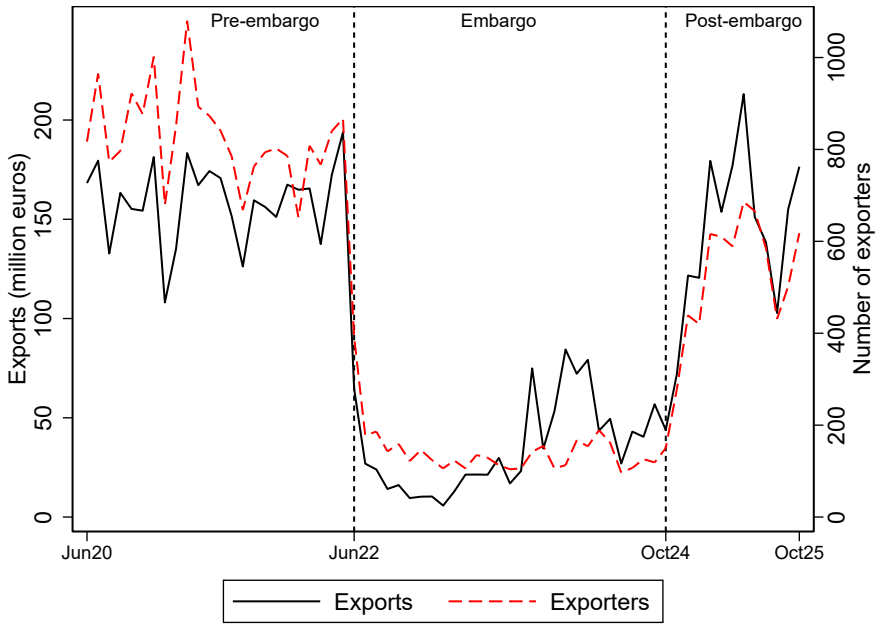
[8]The Algerian government did not want to officially declare an embargo on Spanish products, because this would breach its association agreement with the European Union (EU). In fact, in August 2022 the Algerian government declared that it had not instructed banks to freeze import operations from Spain.

[9]The two largest suppliers were China and France, accounting for 18.4% and 12.7% of total imports, respectively. Data are obtained from the BACI database (Gaulier and Zignago, 2010).

[10]See https://www.middleeastmonitor.com/20241108-algeria-halts-trade-with-france-over-western-sahara-stance/.

[11]The Algerian government finally decided not to introduce general trade sanctions against France, See, for example, https://www.africaintelligence.com/north-africa/2024/11/11/agri-food-giants-breathe-sigh-of-relief-after-algiers--trade-sanctions-withdrawn%2C110338631-art.

Figure 1: Spanish monthly exports and exporters to Algeria, June 2020-October 2025



Source: authors' elaboration based on AEAT-Customs data.

the Spanish position of condemning some actions of Israel in Gaza.[12] It is very unlikely that Spanish firms would have anticipated the change in position of the French government with respect to Western Sahara, the response of the Algerian government, and the subsequent decision to lift the embargo on Spanish imports.

Figure 1 shows the evolution of Spanish monthly exports and exporters to Algeria before, during, and after the embargo. These data were obtained from the Customs and Excise Department of the Spanish Tax Agency (AEAT-Customs). Before the embargo (June 2020–May 2022), Spain exported on average 159 million euros per month to Algeria.[13] There is a marked reduction in exports once the embargo was imposed. In the first month of the embargo, June 2022, exports decreased to 65 million euros. Exports continued to decrease until February 2023, when the lowest monthly amount of exports occurred: 5 million euros. On average, the value of monthly exports during the embargo, 35 million euros, was 78% lower than before the embargo.

The lifting of the embargo led to a sharp increase in Spanish exports to Algeria. In the first month after the embargo, November 2024, exports increased by 64% relative to the

[12]See, for example, https://www.exteriores.gob.es/es/Comunicacion/Comunicados/Paginas/2024_COMUNICADOS/20240628_COMU034.aspx.

[13]Algeria occupied the 30th position in the ranking of Spanish export destinations in 2021, accounting for 0.6% of total Spanish exports. Data obtained from the BACI database (Gaulier and Zignago, 2010).

last embargo month, October 2024. Four months after the lifting of the embargo (February 2025), Spanish monthly exports to Algeria had already reached the pre-embargo levels. Total exports accumulated during the first year after the embargo was lifted (November 2024–October 2025) amounted to 94% of the export value recorded over the corresponding pre-embargo period (November 2020–October 2021).

The evolution of the number of Spanish exporters to Algeria mirrors that of Spanish export values to Algeria: there is a sharp decline in the number of exporters once the embargo is imposed, followed by a strong rebound after it is lifted. On average, the number of monthly exporters during the embargo (143) was 83% lower than before the embargo (829). The number of firms that exported after the embargo (November 2024-October 2025), 2,382, amounted to 79% of the number of exporters recorded over the corresponding pre-embargo period (November 2020–October 2021).

Figure 1 shows that Spain still exported to Algeria during the embargo; that is, the embargo was not complete.[14] One reason for this is that Spanish exporters had to honor contracts that were signed before the embargo. In other cases, exports responded to the particular needs of the Algerian government. For example, at the beginning of 2024 Algeria authorized the import of chickens and red meat from Spain to curb an increase in the prices of these products.[15] In other cases, Algeria authorized the import of inputs and equipment that were needed for the operation of key infrastructures, such as desalination plants, and of transport equipment for airports. Finally, imports from Spain were allowed due to the particular needs of the Algerian army.[16]

Table 1 shows the product composition of Spanish exports to Algeria before and during the embargo. Before the embargo (panel A), chemicals, machinery, paper, metals, and fats and oil were the most exported products, representing 62% of Spanish exports to Algeria. The export pattern during the embargo (panel B) was markedly different. Transport equipment, which was not a major export prior to the embargo, represented 50% of Spanish exports during the embargo. Animal products and machinery were the second and third most exported products, representing, respectively, 17% and 13% of total exports. This change in the composition of exports highlights that the trade pattern during the embargo responded to the specific needs of the Algerian economy.

[14] During the embargo period, 1,170 firms exported to Algeria, of which 539 had also exported during the pre-embargo period and 631 had not. There were 3,413 firms that had exported during the pre-embargo period that did not export during the embargo.

[15] Ricard González: "Argelia retira parcialmente la prohibición de importar productos españoles", El País, January 16, 2024.

[16] Marta Dorgambide: "Surgen contradicciones en el embargo comercial argelino a mercancías españolas", Atalayar.com, June 26, 2023.

Table 1: Export pattern to Algeria before and during the embargo (% of total Spanish exports to Algeria)

Ranking	Before the embargo		During the embargo	
1	Chemicals	18	Transport equipment	50
2	Machinery	13	Animal products	17
3	Paper	11	Machinery	13
4	Metals	11	Chemicals	4
5	Fats and oil	9	Metals	4
6	Rubber and plastic	7	Textiles	3
7	Animal products	5	Foodstuff	2
8	Foodstuff	5	Paper	1
9	Non-metallic minerals	3	Miscellaneous	1
10	Textiles	3	Non-metallic minerals	1

Source: AEAT-Customs.

To confirm that firms that exported during the embargo did not have stronger export capabilities than those whose exports to Algeria were interrupted, Table 2 reports summary statistics for both groups.[17] During the pre-embargo period (June 2020–May 2022), firms affected by the embargo had a higher average monthly probability of exporting than firms that exported during the embargo, both overall (0.71 vs. 0.44) and to Algeria (0.19 vs. 0.14). In terms of export values over the pre-embargo period, total exports and exports to Algeria were very similar across the two groups (43,709 vs. 44,161 and 834 vs. 832).[18] In sum, firms affected by the embargo had a higher monthly probability of exporting than firms that exported during the embargo, while exhibiting similar export values. This evidence indicates that, during the embargo, the Algerian government's approval of import permits was not systematically related to a firm's general probability of exporting or its overall export volume. In other words, exporting to Algeria during the embargo did not depend on a firm's general export capacity or its specific capacity to serve the Algerian market; rather, permit approval was primarily driven by whether the firm produced goods that met the Algerian economy's specific needs during the embargo.

Summing up, the Algerian embargo was politically motivated, largely unanticipated by Spanish firms, and had severe negative effects on Spanish export flows to Algeria. In the next section, we exploit this unexpected disruption as a quasi-natural experiment to estimate its

[17] We define firms affected by the embargo as those that did not export to Algeria between July 2022 and October 2024.

[18] For total exports and exports to Algeria, a t-test cannot reject the null hypothesis that both groups have the same mean.

Table 2: Embargoed vs. non-embargoed firms. Average values during the pre-embargo period (June 2020-May 2022)

	Global		Algeria	
	Embargoed	Non-embargoed	Embargoed	Non-embargoed
Monthly prob. of exporting	0.71	0.44	0.19	0.14
Value of exports (thousand €)	43,709	44,161	834	832

Note: Embargoed firms are firms that did not export to Algeria between July 2022 and October 2024. Non-embargoed firms exported at least one month to Algeria between July 2022 and October 2024. Source: AEAT-Customs.

causal effect on firms' post-embargo probability of exporting to Algeria.

3 Impact of the Algerian embargo on the probability of exporting

3.1 Identification strategy

We use a difference-in-differences design to identify the effect of the embargo on the post-embargo probability of exporting to Algeria. First, we identify all firms that (i) exported to Algeria at least one month during a two-year pre-embargo period: June 2020-May 2022; and (ii) did not export to Algeria during the embargo. The embargo began on 8 June 2022 and lifted on 7 Nov 2024; in the baseline we exclude June 2022 (partial month) and treat July 2022–Oct 2024 as the embargo window. There are 3,413 firms that meet these conditions.

Second, we implement the synthetic difference-in-differences (SDiD) method developed by Arkhangelsky et al. (2021). This procedure combines the features of the synthetic control method (Abadie, 2021) and the difference-in-differences design. In particular, as in the former, it generates optimal control units to ensure that the parallel trends assumption is met; as in the latter, it only demands treated and control units to follow parallel trends during the pre-treatment period. On top of generating control units, the SDiD procedure optimally weights pre-treatment periods to minimize the differences between the control unit levels before and after the treatment.

The SDiD procedure yields the average treatment effect on the treated (ATT). It is

obtained by minimizing the following function:

$$\left(\hat{\tau}^{\text{sdid}}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \arg\min_{\tau,\mu,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \right\}. \tag{1}$$

where $\hat{\tau}^{\text{sdid}}$ is the ATT. Y_{it} is the outcome variable of unit i at time t, and W_{it} is an indicator variable that turns one if unit i is treated at time t. $\hat{\omega}_i^{\text{sdid}}$ and $\hat{\lambda}_t^{\text{sdid}}$ are unit and time weights, respectively. The SDiD procedure optimally chooses $\hat{\omega}_i^{\text{sdid}}$ to ensure that treated and synthetic control units follow similar trends before the treatment. The procedure chooses optimally $\hat{\lambda}_t^{\text{sdid}}$ to reduce level differences between control units in post-treatment and pre-treatment periods.

It is important to stress that, in our setting, the outcome of interest—the post-embargo probability of exporting to Algeria—is only observable two and a half years after the treatment starts. That is, we have a delayed outcome (Bugni et al., 2025).

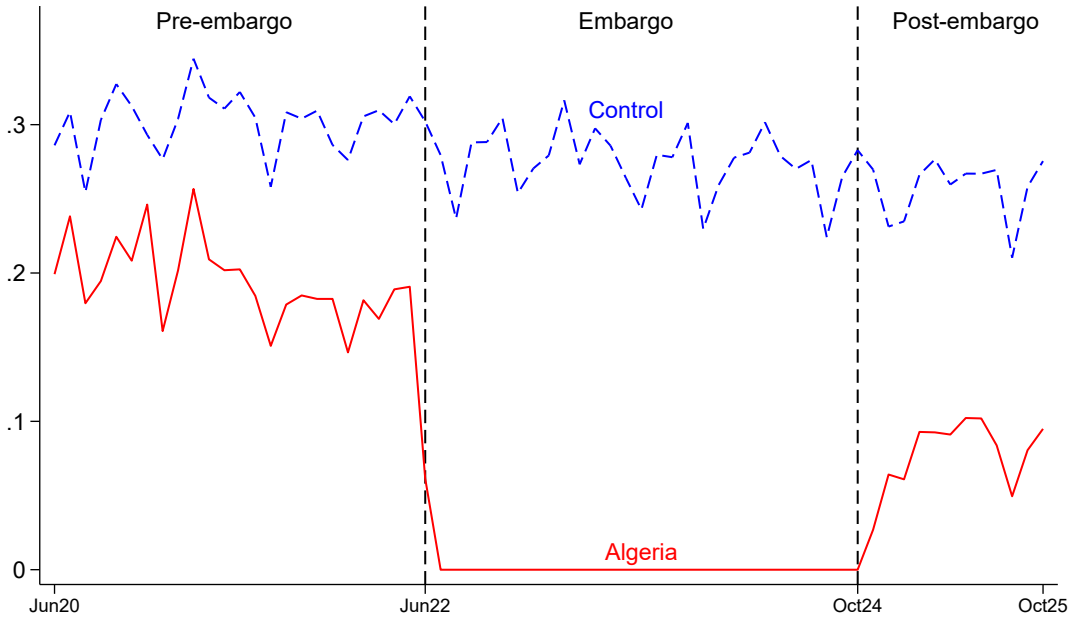
To select the control destination, we re-estimated the SDiD model 50 times, each time using a different country among the top 50 destinations of Spanish exports in the pre-embargo period (June 2020–May 2022) as the control. The average ATT across these 50 estimations was very similar to the ATT obtained when using Romania as the control country.[19] We therefore chose Romania as our baseline control destination. The number of control firms was 4,921.

Figure 2 shows the monthly probability of exporting to Algeria and the control destination over the pre-embargo, embargo, and post-embargo periods. For Algeria (red solid line), we plot the unconditional monthly export probability, while for the control destination (blue dashed line) we plot the monthly export probability weighted by the SDiD unit weights. During the pre-embargo period, there is substantial month-to-month variation in the probability of exporting to Algeria, and in most months the control series closely tracks this variation. This pattern supports the parallel trends assumption. In the remainder of the section, we provide additional evidence validating this assumption.

During the embargo period, excluding June 2022, the unconditional probability of exporting to Algeria is zero for our estimation sample because, by construction, we only retain firms that exported to Algeria before the embargo and did not export during it. For the control destination, the unconditional probability follows a slightly downward trend. After the embargo was lifted, the probability of exporting to Algeria increased, but it remained

[19] 82% of the coefficients estimated in the separate regressions are within one standard error of the average coefficient.

Figure 2: Monthly probability of exporting to Algeria and the control destination



Note: The figure plots the average unconditional probability of exporting to Algeria of Spanish firms that exported to Algeria at least one month between June 2020 and May 2022 and did not export to Algeria during the embargo (red solid line). The figure also plots the weighted average unconditional probability of exporting to the control country (Romania) for a sample of matched firms (blue dashed line). Source: authors' elaboration using data from AEAT-Customs and the SDiD estimation.

well below its pre-embargo level. Four months after the lifting, except for August 2025, the monthly export probability stabilized at roughly 8–10%. In other words, beyond the fourth month we observe no significant further improvement in the probability of exporting to Algeria.[20]

Finally, Figure 2 shows that the gap between the probability of exporting to Algeria and the control country is wider after the embargo than before the embargo, suggesting that the embargo had a negative effect on the post-embargo probability of exporting to Algeria. In the next subsection, we use regression analysis to confirm this conclusion.

3.2 Econometric results

Table 3 reports the results of the estimation. Column 1 shows that, compared to the control destination, the embargo reduced the probability of exporting to Algeria by 6.6 percentage

[20] Among the 3,413 firms that exported during the pre-embargo period, only 997 exported during the post-embargo period (29%).

Table 3: Impact of the embargo on the post-embargo probability of exporting to Algeria

	Experimental	Non-Experimental	
	(1)	(2)	(3)
	Algeria	Control country	Top 50 destinations
main			
Algeria$_j$ × Post-embargo$_t$	-0.066^a		
	(0.007)		
No exports in Jul22–Oct24$_{i,j}$		-3.807^a	-0.244^a
		(0.113)	(0.002)
Observations	300024	68220	3612564
P exporting pre-embargo	0.194		
P exporting with experience		0.393	0.386
Estimation procedure	SDiD	Logit	Linear probability

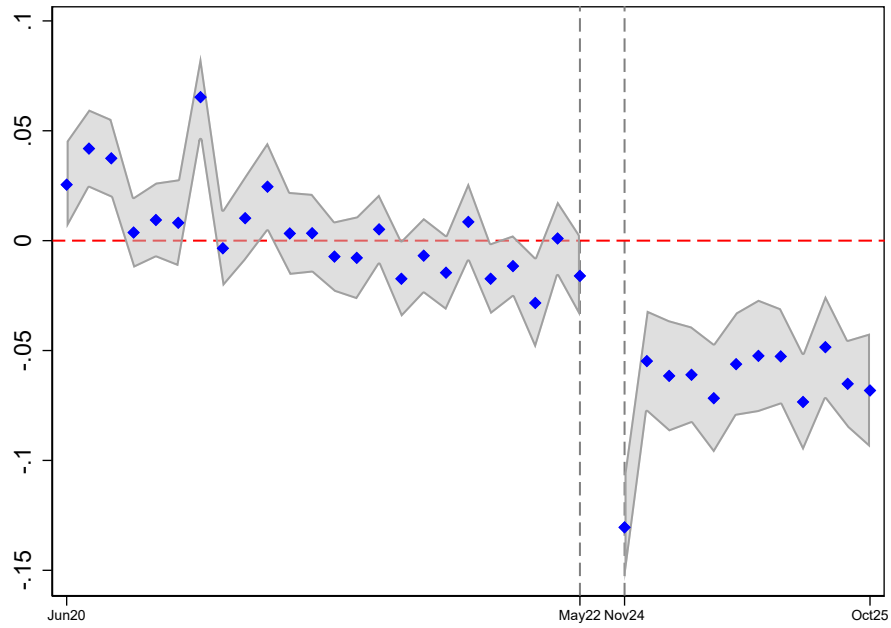
Note: The dependent variable turns one if firm i exported to country j at time t. Estimation in column 2 includes time fixed effects. Estimation in column 3 includes firm×time and destination×time fixed effects. Standard errors clustered at the firm level are in parentheses. a, b, and c: statistically significant at 1%, 5%, and 10%, respectively. In column 1, P exporting pre-embargo is the sample's unconditional monthly probability of exporting to Algeria during the pre-embargo period. In columns 2 and 3 P exporting with experience is the unconditional post-embargo probability of exporting to country j of firms that had export experience in country j between July 2022 and October 2024.

points. This effect is economically large. In our sample, the unconditional probability of exporting to Algeria before the embargo is 19.4%. Hence, the embargo reduced the post-embargo probability of exporting by 34% relative to the pre-embargo probability.

The coefficient reported in column 1 of Table 3 is the average effect of the embargo on the monthly probability of exporting to Algeria during the entire post-embargo period. Next, we investigate whether the negative effect of the embargo was more pronounced in the months immediately after it was lifted than in the following months. We use the event study extension of the SDiD procedure developed by Ciccia (2024). This extension estimates the difference between the probability of exporting to Algeria and the control destinations in a given month relative to the average difference between exporting to Algeria and the control destinations during the pre-embargo period.

Figure 3 plots the monthly coefficients. In addition to the point estimate, we draw the 95% confidence interval for each coefficient. Most of the monthly interaction coefficients in the pre-embargo period were not statistically different from the pre-treatment average difference between exporting to Algeria and the control country. This pattern is consistent

Figure 3: Monthly coefficients on the probability of exporting to Algeria relative to the control destination



Note: The figure reports the point estimate and the 95% confidence interval of the monthly coefficients estimated with the event study extension of the SDiD procedure (Ciccia, 2024).

with the parallel-trends assumption required by our difference-in-differences design.

The probability of exporting to Algeria reaches its lowest point in November 2024, the month in which the embargo was lifted. In that month, the probability of exporting to Algeria, relative to the control destination, was 13 percentage points below its pre-embargo average. Thereafter, the negative effect stabilizes at around 6 percentage points, with no sign of recovery. Taken together, these results indicate that the adverse impact of the embargo persisted a year after it was lifted.

As explained in the introductory section, a potential limitation of previous empirical studies is that they could not fully ensure orthogonality between a firm's attributes and previous export experience. This could lead to an overestimation of the impact of previous export experience on the probability of exporting. To assess this limitation, we compare the impact of the Algerian embargo with the impact of not exporting to a market between July 2022 and October 2024 on the probability of exporting to that market after that period.

To keep the sample design comparable to that used for the embargo episode, we begin by estimating the following regression on the control firms in our baseline sample:

$$Y_{i,t} = \alpha \, \text{No exports in Jul22–Oct24}_i + \gamma_t + \epsilon_{i,t}. \tag{2}$$

Here, $Y_{i,t}$ is an indicator equal to one if firm i exports to the control destination in post-embargo month×year t. No exports in Jul22–Oct24$_i$ is an indicator equal to one if firm i did not export to the control destination at any point between July 2022 and October 2024 (the embargo window defined in the baseline analysis). γ_t denotes month×year fixed effects.

The α coefficient in Equation (2) is the non-experimental equivalent of the τ coefficient in the baseline Equation (1). Both coefficients capture the effect of not having exported to a destination for two and a half years on the probability of exporting to that destination. However, while τ is estimated in an experimental setting, α is estimated in a non-experimental setting. If not exporting to a destination is correlated with an unobservable market-specific firm attribute, the α coefficient may overestimate the negative impact of not having exported to a destination on the probability of exporting to that destination.

We estimate Equation 2 using a logit model. Column 2 of Table 3 reports the log-odds of having no exports to the control destination during July 2022–October 2024. It indicates that if the firm had not exported during that period the odds of exporting were 2.2% of the odds if it had exported (exp -3.807). We also calculated the average marginal effect: firms that did not export to the control destination over the July 2022–October 2024 period are, on average, 37.9 percentage points less likely to export in the post-embargo months. The predicted post-embargo probability of exporting for firms that did export to the control destination during July 2022–October 2024 is 39.3%. Hence, the absence of exports during the embargo window implies a 96.7% lower probability of exporting in the post-embargo period relative to firms with export experience in that window (0.379/0.392).

This result suggests that, in a non-experimental setting—where the orthogonality between past export experience and firms' characteristics cannot be guaranteed—the model substantially overestimates the effect of lacking prior export experience on the probability of exporting. In particular, the implied reduction is almost three times larger than in the experimental setting (96.7% in the non-experimental setting versus 34.0% in the experimental setting).

In the previous logit specification, there is insufficient within-firm variation in export experience to include firm fixed effects. As shown by Bernard and Jensen (2004), failing to control for firm fixed effects tends to inflate the estimated (absolute) impact of past export experience on the probability of exporting. To address this concern, we focus on Spanish firms that exported at least once to any of the top 50 destinations for Spanish exports (excluding

Algeria) during the pre-embargo period, and we estimate the following regression:

$$Y_{i,j,t} = \alpha \text{ No exports in Jul22-Oct24}_{i,j} + \gamma_{i,t} + \gamma_{j,t} + \epsilon_{i,j,t}. \quad (3)$$

where $Y_{i,j,t}$ is an indicator variable that turns one if firm i exported to the top 50 country j in the post-embargo month-year combination t.[21] $\gamma_{i,t}$ is a firm-time specific fixed effect and captures all time-variant firm attributes, such as productivity, that affect the probability of exporting. $\gamma_{j,t}$ is a destination-time specific fixed effect and captures all time invariant (e.g., distance) and time variant variables (e.g., GDP) that affect the export attractiveness of a destination. $\epsilon_{i,j,t}$ is the disturbance term.

Column 3 of Table 3 shows that the coefficient on No exports in Jul22–Oct24$_{i,j}$ is larger in absolute value than in the experimental setting (-0.244 versus -0.066), but as expected smaller than the marginal effect estimated for the control country in the logit estimation without firm fixed effects (-0.244 versus -0.379). In the non-experimental sample of the top 50 destinations, the unconditional post-embargo probability of exporting for firms with export experience between July 2022 and October 2024 is 38.6%. Hence, not having exported for two and a half years reduces the probability of exporting by 63.2% (0.244/0.386). This result indicates that even when controlling for firm-by-time fixed effects, the non-experimental specification yields an estimate of the role of past export experience that is roughly twice as large as in the experimental setting (64.0%/34.0% = 1.9).

In summary, we find that the embargo reduced the post-embargo probability of exporting to Algeria. However, this effect is substantially smaller than what would be inferred from a non-experimental setting.

We also analyzed the post-embargo probability of exporting to Algeria of the 541 Spanish firms that exported to Algeria both before and during the embargo. Applying the same SDiD procedure as in the baseline analysis, we estimate that the post-embargo probability of exporting to Algeria of these firms, relative to the control group, was 9.2 percentage points lower than before the embargo. Since the unconditional probability of exporting to Algeria before the embargo of these firms was 30.7%, the embargo period reduced the probability of exporting by 30.0%. This percentage is similar to the 34.0% we found for firms that did not export during the embargo. That is, exporting during the embargo did not have a significant positive effect on the post-embargo probability of exporting compared to firms affected by the embargo. This result suggests that export operations responded to specific requests in which exporters had little agency.

[21] For each firm we only consider the top 50 countries it exported during the pre-embargo period.

3.3 Robustness

We perform a set of analyses to test the robustness of our results. First, the post-embargo probability of exporting to Algeria is only observable after two and a half years after the treatment starts. Because outcomes materialize with a delay, there is a time window between treatment assignment and outcome realization, during which other observable ex-post actions can take place before the outcome is finally realized. This can bias our results. A key variable that affects the probability of exporting and may change during the embargo period between Algeria and the control country is the economic cycle. To mitigate the effect of this variable, we choose as control a destination whose economic growth during the embargo and post-embargo period is similar to that of Algeria. According to the data from the Bank of Algeria, between the third quarter of 2022 and the second quarter of 2025 (the last available quarter at the time of writing), real GDP in Algeria grew by 13%.[22] According to OECD data, Turkey had the same real GDP growth during that period.[23] We re-estimated the SDiD procedure with Turkey as the control country. Column 1 of Table 4 reports this coefficient: -0.064. This coefficient is very similar to that reported in the baseline regression. Therefore, our estimate of the negative impact of the embargo is robust to using an alternative control country whose economic growth during the embargo and post-embargo period is similar to that of Algeria.

Second, because economic conditions may be correlated across geographically proximate countries, we also consider another Maghreb destination as a control group: Morocco. Other Maghreb countries, such as Libya, Mauritania, and Tunisia, are economically smaller and feature too few Spanish exporters to constitute a viable comparison group. Morocco, however, is not without limitations. First, Spanish exports to Morocco may have been positively affected by Spain's support for the Moroccan solution for the Western Sahara conflict. Second, Spanish firms that were unable to serve the Algerian market during the embargo may have reallocated sales toward Morocco, given its geographic proximity and some similarity in market characteristics. In either case, the Algerian embargo (the treatment) could have indirectly affected exports to Morocco (the control), violating the Stable Unit Treatment Value Assumption (SUTVA) and potentially biasing upward the estimated absolute impact of the embargo.

Keeping these caveats in mind, Column 2 of Table 4 reports the estimate obtained when Morocco is used as the control destination. Contrary to our expectations, the estimated $\text{Algeria}_j \times$ Post-embargo$_t$ coefficient is smaller in absolute value than in the baseline spec-

[22] https://www.bank-of-algeria.dz/croissance-du-pib/.
[23] https://data-explorer.oecd.org/.

ification: −0.048 versus −0.066. According to the new estimation, the post-embargo gap between the probability of exporting to Algeria and the probability of exporting to Morocco widened by 4.8 percentage points relative to the pre-embargo gap. This estimate implies a 24.7% decline relative to the pre-embargo unconditional probability of exporting to Algeria.

Third, Spain announced its change of position relative to the Western Sahara conflict in March 2022, and Algeria recalled its ambassador from Madrid immediately afterward. Some Spanish exporters could have anticipated retaliation from Algeria and brought forward their exports in response. Therefore, the last months prior to the embargo may have shown unusually intense export activity. Figure 2 shows that, indeed, between March and May 2022 there was slightly higher export activity compared to previous months. To remove a potential anticipation effect, we shorten the pre-embargo period to June 2020-February 2022 and re-estimate the model with a new control group. Column 3 of Table 4 shows that the interaction coefficient is the same as in the one estimated in the baseline analysis: -0.066.

Fourth, firms could have exported to intermediary firms located in countries such as France, Italy, and Portugal to circumvent the embargo and continue exporting to Algeria.[24] To identify potential rerouters, for each firm that was affected by the embargo, we identify whether export growth during the embargo in France, Italy, and Portugal was substantially higher than in other markets. The logic of this procedure is that a firm affected by the embargo could divert its pre-embargo exports to Algeria toward other destinations where it was already exporting. In the absence of such rerouting, we would expect export growth during the embargo to be similar across destinations. If export growth to France, Italy, and Portugal is substantially higher than to other destinations, this pattern would be consistent with rerouting through these markets. We define a firm as a potential rerouter if the increase in exports to France, Italy, and Portugal was 25% higher than in the other destinations. We only identify as potential rerouters firms for which Algeria was a relevant export market before the embargo.[25]

Only 93 of 3,413 firms affected by the embargo had substantially higher export growth in France, Italy, and Portugal than in other destinations during the embargo. We exclude these from the sample and re-estimate Equation (1). Column 4 of Table 4 reports the estimation. Surprisingly, the removal of potential rerouters reduces the negative effect of the embargo.

[24]See, for example, Francisco Carrión: "Argelia levanta parcialmente el bloqueo a las exportaciones desde España", El Independiente, January 15, 2024.; Ricard González: "Argelia retira parcialmente la prohibición de importar productos españoles", El País, January 16, 2024.; Francisco Carrión: "Argelia mantendrá las restricciones al comercio español y argumenta que está apostando por la industria local", El Independiente, June 15, 2024.

[25]We define Algeria as a relevant market if it represented at least 10% of a firm's total exports in the pre-embargo period.

Table 4: Robustness

	(1) Control similar growth	(2) Control Maghreb	(3) Shorter pre-embargo	(4) Rerouters	(5) Quarterly
$\text{Algeria}_j \times \text{Post-embargo}_t$	-0.064^a	-0.048^a	-0.066^a	-0.060^a	-0.125^a
	(0.007)	(0.004)	(0.006)	(0.009)	(0.010)
Observations	301392	718092	253011	287568	98760

Note: The dependent variable turns one if firm i exported to country j at time t. All equations are estimated using the SDiD procedure. Standard errors clustered at the firm level are in parentheses. a, b, and c: statistically significant at 1%, 5%, and 10%, respectively.

In any case, the new interaction coefficient is quantitatively large and qualitatively similar to that estimated in the baseline scenario.

Fifth, we reduce the frequency of our dependent variable. Instead of the monthly probability of exporting to Algeria, we use the quarterly probability, to assess whether our results are robust to changes in the timing and frequency of firms' shipments after the embargo. Because the embargo was lifted in November 2024, we define quarters on an ad-hoc basis to better align with the post-embargo window: Q1 comprises November–January, Q2 February–April, Q3 May–July, and Q4 August–October. To ensure complete quarters, the pre-embargo period runs from May 2020 to April 2022, and the post-embargo period from November 2024 to October 2025. Column 5 of Table 4 reports the estimate. We find that the embargo reduced the quarterly probability of exporting to Algeria by 12.5 percentage points. Relative to the pre-embargo unconditional quarterly probability of 38.3%, this implies a 32.6% decline in the likelihood of exporting. This result is similar to the 34.0% reduction estimated in the baseline analysis using monthly data.

In summary, the robustness analyses confirm that the Algerian embargo had a negative effect on the post-embargo probability of exporting to Algeria. In the next section, we explore the frictions that could explain this negative effect.

4 What explains the negative impact of the embargo?

Our baseline result shows that a firm subject to the embargo had a lower probability of exporting to Algeria once the embargo was lifted. In the introductory section, we highlighted three types of frictions that can explain this lower probability: beachhead sunk costs, cus-

tomer accumulation, and learning. This section explores the contribution of these frictions to the lower post-embargo probability of exporting to Algeria.

Our strategy is to examine whether the negative impact of the Algerian embargo varies across firms and to relate this heterogeneity to the frictions identified above. To guide the analysis, we develop in Appendix B a simple discrete-choice model (with myopic firms) in which firms decide whether to re-enter the Algerian market after the embargo. In the model, firms must pay a sunk cost to re-enter Algeria after a no-export spell such as the embargo. Bernard et al. (2015) show that producers are more likely to export through intermediaries in markets with high entry costs. Building on this insight, we assume that intermediaries have superior technology for overcoming re-entry costs, so that re-entry sunk costs are lower for intermediaries than for producers. The model then predicts that, if sunk costs are an important determinant of export dynamics, the embargo's negative effect should be smaller for intermediaries than for producers.

In our model, demand for a firm's product depends, among other factors, on its product appeal and market visibility. Following Piveteau (2021), market visibility in turn depends on the number of customers the firm has served over its export history in Algeria. The model predicts that firms with higher pre-embargo visibility experience a larger drop in net export profits and, therefore, a larger reduction in the probability of exporting to Algeria between the pre-embargo and post-embargo period.

Following Egger et al. (2025), we assume that the fixed cost of exporting to Algeria is negatively related to the export experience that a firm has accumulated in that market. The model predicts that firms with higher pre-embargo experience a larger drop in net export profits and, therefore, a larger reduction in the post-embargo export probability.

Finally, in the previous section we showed that, after the embargo, firms were less likely to export to Algeria than before it. The model also predicts this pattern. When a firm is prevented from exporting to Algeria by an embargo, its market visibility and knowledge erode, and it must repay sunk costs to re-enter the market. Because export revenues fall with lower market visibility while costs rise due to re-entry sunk costs and higher fixed costs, the firm becomes less likely to export to Algeria after the embargo than before it.

Next, we empirically test the model's predictions regarding trade frictions. AEAT-Customs does not provide information on the main activity of the firm, so we merge AEAT-Customs with Bureau Van Dijk's SABI database using the correspondence described in de Lucio et al. (2018). SABI reports financial and accounting records for firms that file their accounts with the Spanish Business Register and includes the 4-digit NACE classification of each firm's economic activity. We define a Spanish exporter as an intermediary if its

2-digit NACE Rev. 2 code is 46 or 47 (wholesale and retail activities, except sales of motor vehicles and motorcycles). Due to statistical confidentiality constraints, we are only allowed to use information for firms included in the Directorio de Empresas Exportadoras e Importadoras, a database constructed by Cámara de Comercio de España (Spanish Chamber of Commerce). The Directorio provides a representative sample of exporting firms with more than 10 employees, which reduces the number of firms in the sample.

To identify firms with many customers in the Algerian market, we compute the share that each Spanish firm held in 2021—one year before the embargo—of Algerian imports in the 6-digit Harmonized System (HS) product exported by the firm.[26] To construct this measure, we combine product-level information on Algerian imports from the BACI database (Gaulier and Zignago, 2010) with firm-level export data from AEAT-Customs.

To measure a firm's export experience in Algeria, we simply compute the number of years a firm exported to Algeria five years before the embargo (June 2017-May 2022).[27]

In the SDiD procedure, weights are chosen to match a single treated group with the donor pool. Hence, it does not guarantee that weights are optimal for subgroups, as is the case in heterogeneity analyses. Therefore, following de Roux et al. (2025), we use the standard difference-in-differences design, which offers a straightforward approach for analyzing heterogeneous effects across treated units. We estimate the following equation:

$$\begin{aligned} Y_{i,j,t} = \beta_1(\text{Algeria}_j * \text{Post-embargo}_t) + \beta_2(\text{Algeria}_j * \text{Post-embargo}_t * \text{Producer}_i) + \\ \beta_3(\text{Algeria}_j * \text{Post-embargo}_t * \text{Large Pre-embargo Market Share}_{ij}) + \\ \beta_4(\text{Algeria}_j * \text{Post-embargo}_t * \text{Long Pre-embargo Experience}_{i,j}) + \gamma_{i,j} + \gamma_t + \epsilon_{i,j,t}. \end{aligned} \tag{4}$$

where Producer_i turns one if the main activity of firm i is not related to wholesaling or retailing. Large pre-embargo $\text{market share}_{i,j}$ is an indicator that equals one if firm i's market share in Algeria in 2021 was above the 75th percentile among pre-embargo exporters to Algeria. Long pre-embargo $\text{experience}_{i,j}$ is an indicator that equals one if firm i's pre-embargo export experience in Algeria exceeded the 75th percentile among pre-embargo exporters to Algeria. $\gamma_{i,j}$ and γ_t are firm×destination and time fixed effects, respectively.

To estimate the differences-in-differences regression, we construct a matched sample in which each pre-embargo exporter to Algeria is paired with a pre-embargo exporter to the control destination (Romania). For each treated firm, we select the control firm that minimizes

[26] If a firm exported more than one 6-digit HS product, we calculate an export-weighted market share.
[27] We define a year as a 12-month period that starts in June and ends in May.

Table 5: Contribution of trade frictions to the negative effect of the embargo

$\text{Algeria}_j \times \text{Post-embargo}_t$	-0.026^b
	(0.011)
$\text{Algeria}_j \times \text{Post-embargo}_t \times \text{Producer}_i$	0.017^b
	(0.008)
$\text{Algeria}_j \times \text{Post-embargo}_t \times \text{Large pre-share}_{i,j}$	-0.102^a
	(0.014)
$\text{Algeria}_j \times \text{Post-embargo}_t \times \text{Long pre-experience}_{i,j}$	-0.118^a
	(0.015)
Observations	177192
Adjusted R2	0.378

Note: The dependent variable turns one if firm i exported to country j at time t. The estimation includes firm×destination and time fixed effects. Standard errors clustered at the firm and time levels are in parentheses. a, b, and c: statistically significant at 1%, 5%, and 10%, respectively.

the Euclidean distance in a vector of standardized pre-embargo characteristics.[28]

Table 5 reports the results. For intermediaries with a low market share and little experience in Algeria in the pre-embargo period, the embargo reduces the probability of exporting to Algeria by 2.5 percentage points. Contrary to the prediction of the model, we find that being a producer reduces the negative impact of the embargo. We find this result consistent with a narrative in which sunk costs play, at most, a limited role in explaining the negative effect of the embargo.

Consistent with the model, the negative effect of the embargo becomes larger (in absolute terms) when a firm had a high pre-embargo market share in Algeria and when it also had a long export history there. For a firm with both a large market share and long experience in Algeria before the embargo, the embargo reduces the post-embargo probability of exporting by 22 percentage points. Of this reduction, around 54% is attributable to experience and the remaining 46% to market share. Given that the unconditional monthly probability of exporting to Algeria before the embargo for such firms was 51.5%, the embargo implies a 42.7% decline in their export probability.

Our results suggest that firms persist in exporting primarily to preserve market experience

[28]These characteristics summarize both the level and the dynamics of a firm's export participation across all destinations. They include: (i) the average monthly probability of exporting, (ii) the average number of destinations served, (iii) the linear trend and (iv) the quadratic curvature of the firm's monthly export participation rate, and (v) the volatility of this participation. Each variable is standardized to have zero mean and unit variance before matching. This procedure ensures that the comparison group mirrors not only the average export intensity of treated firms but also the shape and stability of their pre-embargo export trajectories.

and visibility and to maintain inertia in sales to foreign customers. These mechanisms are in line with the findings of Timoshenko (2015), Piveteau (2021), and Eaton et al. (2025).

To conclude, our heterogeneity analysis shows that firms with long pre-embargo experience in Algeria and a large pre-embargo market share experienced the largest decline in their probability of exporting after the embargo. Importantly, this pattern is not inconsistent with the model: the same characteristics that make firms more exposed to the shock may also imply that they remain the most likely exporters once the embargo is lifted.

To assess the model prediction that experience and pre-embargo market share increase post-embargo export participation, we estimate:

$$\begin{aligned} Y_{i,t}^{\text{Algeria}} = \beta_1 \text{Producer}_i + \beta_2 \text{Large Pre-embargo Market Share}_i + \\ \beta_3 \text{Long Pre-embargo Experience}_i + \gamma_t, \end{aligned} \tag{5}$$

where $Y_{i,t}^{\text{Algeria}}$ is an indicator equal to one if firm i exports to Algeria in post-embargo month–year t, and γ_t denotes month–year fixed effects.

We estimate Equation (5) using a logit model. Table A.1 in Appendix A reports the results. Contrary to the model's prediction, producers exhibit higher export participation than wholesalers: their odds of exporting are about 26% higher ($\exp(0.235)$). In line with the model, we find that (i) firms with a large pre-embargo share of the Algerian market have approximately 2.2 times higher odds of exporting to Algeria after the embargo than firms with smaller shares; and (ii) firms with long pre-embargo experience have about 4.3 times higher odds of exporting after the embargo.

Overall, although high-share and high-experience firms experienced the largest post-embargo declines in export participation, they remained the firms with the highest likelihood of exporting to Algeria after the embargo.

5 Conclusions

This paper analyzes whether Spanish firms affected by the Algerian embargo experienced a lower probability of exporting to Algeria once the embargo was lifted. The Algerian embargo provides a clean setting for identifying the causal effect of a market shutdown on the probability of exporting in the future because its imposition and subsequent lifting were unexpected by firms.

We find that the embargo significantly reduced the post-embargo probability of exporting to Algeria. However, this negative effect is smaller than the impact of an observationally

similar situation in which a firm simply does not export to a market for two and a half years, the length of the embargo. This result suggests that studies relying on non-experimental variation are likely to overstate the effect of past export experience on the probability of exporting.

We also show that the embargo's negative effects continued even one year after it was lifted. Finally, we find that the probability of exporting to Algeria after the embargo decreased more in firms that had accumulated a deeper understanding of the Algerian market and a larger market share before the embargo. By contrast, sunk costs did not play a significant role in the post-embargo probability of exporting. These results suggest that preserving market visibility and experience and maintaining inertia in sales to foreign customers are key motives for firms to persist in exporting.

Appendix A Additional tables

Table A.1: Probability of exporting to Algeria after the embargo

Producer_i	0.235^a
	(0.049)
$\text{Large pre-share}_{i,j}$	0.808^a
	(0.048)
$\text{Long pre-experience}_{i,j}$	1.455^a
	(0.051)
Observations	29532
Log likelihood	-7150.127

Note: The dependent variable turns one if firm i exported to country Algeria after the embargo. The regression includes time fixed effects. Robust standard errors clustered at the firm level are in parentheses. a, b, and c: statistically significant at 1%, 5%, and 10%, respectively.

Appendix B A simple model of export re-entry after an embargo

This appendix presents a simple discrete-choice model that organizes the mechanisms behind our heterogeneity analysis. A Spanish firm decides whether to re-enter the Algerian market once the embargo is lifted. The model captures three frictions discussed in the main text: (i) sunk costs of (re-)entry, (ii) the accumulation and erosion of a customer base and market visibility, and (iii) the accumulation and erosion of market-specific knowledge. It delivers predictions about how the embargo's impact on the post-embargo probability of exporting varies with firms' pre-embargo export experience, market share, and whether they operate as producers or intermediaries.

Consider a firm that produces a differentiated variety and competes monopolistically with other firms in Algeria. There is a single factor of production, labor, with normalized wage equal to 1. The firm has productivity φ, drawn from a distribution $G(\varphi)$ after paying an initial entry cost into production (not specific to Algeria). When selling to Algeria it faces an iceberg trade cost $\tau \geq 1$, so the marginal cost of serving Algeria is

$$MC = \frac{\tau}{\varphi}. \tag{B.1}$$

Demand in Algeria is CES with elasticity of substitution $\sigma > 1$. Under monopolistic competition, the firm sets a constant markup $\sigma/(\sigma - 1)$, which implies the optimal export price

$$p = \frac{\tau}{\varphi} \frac{\sigma}{\sigma - 1}. \tag{B.2}$$

Let R denote aggregate expenditure on the final good in Algeria and P the associated CES price index. Demand for the firm's variety at time t is

$$q_t = D_t R P^{\sigma-1} p^{-\sigma},$$

where D_t is a firm-specific demand shifter. We model this shifter as

$$D_t = A V_t,$$

where A is the exogenous appeal of the product and V_t is the firm's market visibility in Algeria at the beginning of period t. Following Arkolakis (2010), V_t can be interpreted as depending on accumulated advertising and marketing effort, while, following Piveteau (2021), it can also be determined by the accumulated customer base.

Revenue from selling to Algeria at time t is then

$$r_t = AV_t R \left(\frac{p}{P}\right)^{1-\sigma}. \tag{B.3}$$

Under CES demand and monopolistic competition, operating (i.e., variable) profits from serving Algeria are a constant share of revenue. Using the standard CES result, the firm chooses p so that operating profits equal a fraction $1/\sigma$ of revenue:

$$\pi_t^{\text{op}} = \frac{1}{\sigma} r_t = \frac{1}{\sigma} AV_t R \left(\frac{p}{P}\right)^{1-\sigma}.$$

For convenience, define the constant

$$S \equiv \frac{1}{\sigma} AR \left(\frac{p}{P}\right)^{1-\sigma},$$

so that $\pi_t^{\text{op}} = SV_t$.

To export in period t, the firm must also pay a fixed operating cost $F_t(\lambda_t)$, where $\lambda_t \in [0, 1]$ measures the firm's stock of market-specific knowledge or experience in Algeria. Following Egger et al. (2025), we assume that experience lowers the fixed cost:[29]

$$F_t = F(1 - \lambda_t), \tag{B.4}$$

with $F > 0$ and $0 \leq \lambda_t \leq 1$. A firm with no export experience in Algeria has $\lambda_t = 0$ and thus $F_t = F$.

In addition, whenever a firm starts exporting to Algeria after a spell of non-exporting (this includes both first-time entry and any subsequent re-entry), it must pay a sunk cost F_s^k, where $k \in \{\text{prod}, \text{int}\}$ indexes producers and intermediaries. We assume that intermediaries face lower sunk costs than producers:

$$F_s^{\text{int}} < F_s^{\text{prod}}.$$

This captures that intermediaries, which handle a wide range of products and clients, can reuse part of their commercial infrastructure across destinations, whereas producers incur higher market-specific sunk costs.

The type-specific sunk cost is paid whenever the firm transitions from no exports to positive exports in Algeria ($Y_{t-1} = 0$ and $Y_t = 1$), regardless of whether this is the first time

[29] Another option, as in Timoshenko (2015), is to model experience as a demand shifter.

it serves Algeria or a re-entry after a previous exit.

Let $Y_t \in \{0, 1\}$ denote the export status in period t, where $Y_t = 1$ if the firm exports to Algeria. Firms are not forward looking: at each date, they compare current-period export profits to zero and ignore the future consequences of their export decision. In particular, when deciding whether to export in period t, they do not anticipate the embargo, do not account for the erosion of visibility and experience if they stop exporting, and do not internalize that they will have to pay the sunk cost F_s^k again to resume exports after a no-export spell.

Consider firms that have already been active in Algeria for some time before the embargo. For these firms, in the last pre-embargo period t we have $Y_t = 1$ and $Y_{t-1} = 1$, so they do not pay the sunk cost at t. Their net export profit is

$$\pi_t = \pi_t^{\text{op}} - F_t = SV_t - F(1 - \lambda_t).$$

To generate export probabilities rather than deterministic choices, we introduce an idiosyncratic profit shock ε_t that is independent across firms and over time and has cumulative distribution function $H(\cdot)$. The firm exports in period t if

$$\pi_t + \varepsilon_t \geq 0.$$

The export probability in period t is therefore

$$\mathbb{P}_t \equiv \Pr(Y_t = 1) = \Pr(\varepsilon_t \geq -\pi_t) = 1 - H(-\pi_t), \tag{B.5}$$

which is strictly increasing in π_t.

At $t + 1$ Algeria imposes an embargo on Spanish products. As a result, the firm cannot export to Algeria in period $t + 1$, so $Y_{t+1} = 0$ and it does not observe current-period sales in that market. The embargo is unanticipated: in line with the myopic behavior described above, firms do not foresee this shock when they choose Y_t.

The embargo reduces both the firm's visibility and its market-specific knowledge. We model this erosion as

$$V_{t+2} = (1 - \mu)V_t, \qquad 0 < \mu \leq 1, \tag{B.6}$$

and

$$\lambda_{t+2} = (1 - \gamma)\lambda_t, \qquad 0 < \gamma \leq 1. \tag{B.7}$$

That is, between the last pre-embargo period t and the first post-embargo period $t + 2$, the firm loses a fraction μ of its visibility and customer base and a fraction γ of its accumulated

market knowledge. A longer embargo or a more severe disruption can be interpreted as leading to higher values of μ and γ.

At $t+2$ the embargo is lifted and the firm decides whether to re-enter the Algerian market. Since $Y_{t+1} = 0$, re-entry requires paying both the per-period fixed cost $F_{t+2} = F(1 - \lambda_{t+2})$ and the type-specific sunk cost F_s^k. Using (B.3)–(B.7), operating profits in $t + 2$ are

$$\pi_{t+2}^{\text{op}} = SV_{t+2} = S(1 - \mu)V_t.$$

The export profit in the post-embargo period, net of the per-period fixed cost and the sunk cost, is

$$\pi_{t+2}^k = S(1 - \mu)V_t - F(1 - \lambda_{t+2}) - F_s^k = S(1 - \mu)V_t - F\big(1 - (1 - \gamma)\lambda_t\big) - F_s^k. \tag{B.8}$$

With the idiosyncratic shock ε_{t+2}, the re-entry decision is

$$Y_{t+2} = \begin{cases} 1 & \text{if } \pi_{t+2}^k + \varepsilon_{t+2} \geq 0, \\ 0 & \text{otherwise,} \end{cases} \tag{B.9}$$

and the post-embargo export probability is

$$\mathbb{P}_{t+2}^k = \Pr(Y_{t+2} = 1) = 1 - H\big(-\pi_{t+2}^k\big).$$

Using (B.8) and the expression for π_t, the change in net export profits between t and $t + 2$ for a firm of type k is

$$\begin{aligned} \Delta\pi^k &\equiv \pi_{t+2}^k - \pi_t \\ &= \Big[S(1 - \mu)V_t - F\big(1 - (1 - \gamma)\lambda_t\big) - F_s^k\Big] - \Big[SV_t - F(1 - \lambda_t)\Big] \\ &= S\big[(1 - \mu)V_t - V_t\big] + F\big[(1 - \lambda_t) - (1 - (1 - \gamma)\lambda_t)\big] - F_s^k \\ &= -S\mu V_t - F\gamma\lambda_t - F_s^k. \end{aligned} \tag{B.10}$$

Since $\mu > 0$, $\gamma > 0$, $F > 0$ and $F_s^k > 0$, we have

$$\Delta\pi^k = -\big(S\mu V_t + F\gamma\lambda_t + F_s^k\big) < 0,$$

so the embargo always reduces current-period export profitability in the first post-embargo period.

Moreover, (B.10) implies the following comparative statics:

$$\frac{\partial \Delta\pi^k}{\partial V_t} = -S\mu < 0, \qquad \frac{\partial \Delta\pi^k}{\partial \lambda_t} = -F\gamma < 0, \qquad \frac{\partial \Delta\pi^k}{\partial F_s^k} = -1 < 0.$$

Hence, holding all other characteristics fixed, firms with higher pre-embargo visibility V_t, higher pre-embargo experience λ_t, and higher sunk costs F_s^k suffer a larger fall in profits between t and $t+2$. Since $F_s^{\text{prod}} > F_s^{\text{int}}$, the drop in profitability is larger for producers than for intermediaries.

Because the export probability $\mathbb{P}_s = 1 - H(-\pi_s)$ is strictly increasing in π_s, a larger fall in profits translates into a larger fall in the probability of exporting, except possibly at corner cases where probabilities are arbitrarily close to zero or one. The change in the export probability for a firm of type k is

$$\Delta\mathbb{P}^k \equiv \mathbb{P}_{t+2}^k - \mathbb{P}_t = \left[1 - H(-\pi_{t+2}^k)\right] - \left[1 - H(-\pi_t)\right] = H(-\pi_t) - H(-\pi_{t+2}^k) < 0, \quad \text{(B.11)}$$

and the magnitude of this change is increasing in the size of the profit loss $|\Delta\pi^k|$.

The model therefore yields the following qualitative predictions about heterogeneity in the embargo's impact on export probabilities:

1. **Visibility.** Firms with higher pre-embargo visibility V_t (e.g., higher pre-embargo market share in Algeria) experience a larger drop in net export profits and, therefore, a larger reduction in the probability of exporting to Algeria between t (pre-embargo) and $t+2$ (post-embargo).

2. **Experience.** Firms with higher pre-embargo experience λ_t (e.g., longer or more intense pre-embargo export spells) experience a larger drop in net export profits and, therefore, a larger reduction in the post-embargo export probability.

3. **Intermediaries vs. producers.** Because producers face higher sunk costs than intermediaries ($F_s^{\text{prod}} > F_s^{\text{int}}$), producers suffer a larger drop in net export profits between t and $t+2$. Consequently, the embargo reduces the probability of re-entering the Algerian market more for producers than for intermediaries.

The comparative statics above also deliver a prediction about levels of post-embargo export participation. From (B.8) and the monotonicity of $\mathbb{P}_{t+2}^k = 1 - H(-\pi_{t+2}^k)$ in π_{t+2}^k, firms with higher pre-embargo visibility V_t and higher pre-embargo experience λ_t are more likely to export to Algeria once the embargo is lifted. Formally, letting $h(\cdot)$ denote the

density associated with $H(\cdot)$,

$$\frac{\partial \mathbb{P}^k_{t+2}}{\partial V_t} = h\big(-\pi^k_{t+2}\big)\ S(1-\mu) > 0, \qquad \frac{\partial \mathbb{P}^k_{t+2}}{\partial \lambda_t} = h\big(-\pi^k_{t+2}\big)\ F(1-\gamma) > 0.$$

Moreover, because producers face higher sunk costs of (re-)entry than intermediaries ($F_s^{\text{prod}} > F_s^{\text{int}}$), we have $\pi^{\text{prod}}_{t+2} < \pi^{\text{int}}_{t+2}$ and therefore

$$\mathbb{P}^{\text{prod}}_{t+2} < \mathbb{P}^{\text{int}}_{t+2}.$$

Hence, the model predicts that (i) firms with larger pre-embargo market share (higher V_t) and longer pre-embargo experience (higher λ_t) should be more likely to export after the embargo; and (ii) producers should be less likely to export after the embargo than intermediaries.

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