Are the Benefits of Export Support Durable? Evidence from Tunisia[#]

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Abstract

This paper evaluates the effects of the FAMEX export promotion program in Tunisia on the performance of beneficiary firms. While much of the literature assesses only the short term impact of such programs, we consider also the longer term impact. Propensity-score matching difference-in-difference and weighted least squares estimates suggest that beneficiaries initially see faster export growth and greater diversification across markets and products. However, three years after the intervention, neither the growth rates nor the export levels of beneficiary firms are significantly different from those of non-beneficiary firms. Exports of beneficiaries remain more diversified, but the diversification does not translate into lower volatility of exports. There is also no evidence that the program produced spillover benefits for non-beneficiary firms. Taken together, these results suggest that export promotion programs may in some cases induce firms to diversify without creating other durable benefits.

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1. Introduction

Since trade liberalization *per se* has not always proved to be a panacea to improve poor export performance, the focus of trade policy has shifted in recent years towards targeted interventions to facilitate trade and especially to promote exports. Significant resources are devoted to export-processing zones, exporter assistance programs, and projects aimed at modernizing border management and customs procedures. The paucity of rigorous evaluations of these interventions, however, has made it hard to assess their desirability and to improve their design. This paper provides new evidence on the firm-level impact of a recent export promotion program in Tunisia. It seeks, in particular, to fill an important gap in the literature: Whereas evaluations have typically focused on the short-term contemporaneous effect of interventions, we know relatively little about their longer-term impact.[1]

The literature on export promotion effectiveness has developed along two strands. The older strand relies on cross-country evidence and examines effects on aggregate export performance. Thus, Rose (2007) uses a gravity equation to show that diplomatic representations had a positive effect on bilateral trade flows. Lederman, Olarreaga, and Payton (2010) show that, after a long history of failure, in particular in developing countries where export promotion was accompanied by misguided policies like import substitution and currency overvaluation, it recently had more success in increasing aggregate exports, particularly when agency management involved the private sector.

A more recent strand of the literature has looked for export promotion effects using quasi-experimental methods, comparing the export performance of treated firms with that of a control group. Since enrolment into export promotion programs is never random, most papers go to great lengths to control for selection effects through matching, fixed effects, and two-step (IV or Heckman) estimation methods. The first broad finding is that export promotion seems to be more successful at affecting the performance of established exporters than at encouraging non-exporting firms to start exporting (Bernard and Jensen, 2004; Görg, Henry and Strobl, 2008; Girma, Gong, Görg and Yu, 2009). This is in accordance with the literature on heterogeneous firms and trade, which suggests that exporters differ from non-exporters in terms of productivity and a host of other firm characteristics (see, e.g. Bernard, Jensen, Redding and Schott, 2007), which export promotion activities may not be able to offset. The second broad finding is that the impact is stronger along the extensive margin than along the intensive one (Alvarez and Crespi,

[1] This gap can lead to what Ravallion (2008) has called “myopia bias”, whereby evaluation focused on short-term effects may tilt incentives toward development projects that yield quick results.

2000, Volpe and Carballo, 2008).[2] Thus assistance may be more successful in helping firms overcome hurdles to break into new markets (product- or destination-wise) than in ramping up export volumes.[3] These papers break new ground in terms of rigorous evaluation of trade interventions, but focus primarily on the short-term effects of interventions.

The present paper examines both the short-term and the longer-term impact of Tunisia's export promotion program, FAMEX, which consists of matching grants provided to Tunisian firms to implement export business plans. We combine several sources of firm-level data—FAMEX program data, National Statistical Institute and Investment Promotion Agency data, and customs transaction data—into a unique, rich dataset on Tunisian exporters. In particular, merging customs data with other sources eliminates the risk of recall bias in outcome variables, which tends to arise when public programs are evaluated ex-post using surveys.[4]

We estimate FAMEX's treatment effects using a menu of estimation methods, including difference-in-differences combined with propensity-score matching (PSM-DID) and difference-in-differences weighted by propensity scores, designated henceforth as weighted least squares regressions (WLS). Relative to existing studies, our rich dataset allows us to extend the analysis in several directions; in particular, we examine the sustainability of the program's effect. We find that, compared to a control group, FAMEX beneficiaries successfully diversify in terms of export destination markets and products, and durably so, as they exhibit higher survival rates in their new markets up to five years after the treatment.[5] However, the beneficiary firms' total exports diverges only temporarily from the control group's total exports. One year after treatment, the differential in export's growth rates is not significant anymore. Three years after treatment, even export *levels* are not significantly different anymore. Only product and destination counts remain significantly different throughout the sample period. However, the treatment group's diversification does not seem to translate into reduced export volatility.

We also examine the existence of program spillovers by estimating FAMEX's indirect impact on the performance of *control* firms through contagion. This is an important—although typically underexplored—part of program impact evaluation, because in the presence of spillovers, the

[2] Girma, Gong, Görg and Yu (2009) find a positive impact along the intensive margin but they consider the special case of production subsidies.
[3] See Rangan and Lawrence (1999) and references therein on the hurdles facing the internationalization of firms. Assistance may have stronger effects for small firms, perhaps because they face relatively greater hurdles, as Volpe and Carballo (2010) find in the case of an export promotion program in Chile.
[4] In the case of FAMEX, the World Bank collected firm-level survey data to analyze the impact of the program and the corresponding analysis is described in Gourdon, Marchat, Sharma, and Vishwanath (2011).
[5] Besedes and Prusa (2011) show the importance of export survival and thus of the increased duration of trade relationships for export growth in developing countries.

absence of a positive measured treatment effect could mean a positive *true* treatment effect transmitted to the control group through positive externalities, which is precisely the combination that would justify government intervention.[6] Catching-up by control firms due to gradual spillovers could also explain why we find only temporary treatment effects on total exports, although one would also expect imitation in terms of diversification. Under the hypothesis that spillovers work through contact with treated firms in the same industry and geographical area, we find that such effects are generally not significant, and in some cases are negative. This could reflect, for instance, poaching of good managers and workers by treated firms.

Finally, we study heterogeneity in treatment effects as a function of the beneficiaries' objectives and use of assistance. We show that a firm's objective when requesting FAMEX assistance matters for performance: firms coming to expand into new markets or to develop new export products benefit more than firms coming to increase their exports. Also, market prospection and promotion activities correlate more significantly with export outcomes than other components of FAMEX like firm or product development, suggesting that informational barriers are the most amenable to effective government assistance. These findings also support the broad view that firms seeking and using assistance to expand along the extensive margin are less likely to be disappointed with the longer-term outcome than those seeking and using assistance to expand along the intensive margin.

The paper is organized as follows. Section 2 describes the export promotion program and Section 3 presents the data. Section 4 discusses estimation issues. Section 5 presents the FAMEX treatment effects and robustness checks. Section 6 focuses on diversification and volatility of exports while Section 7 examines the presence of externalities. Section 8 discusses whether firms' objectives and use of assistance matter. Section 9 presents estimates of the economic magnitude of the FAMEX effects, and Section 10 concludes.

2. Export Promotion in Tunisia

The Tunisian government has worked since the mid-1990s to reduce the traditional anti-export bias of Tunisia's trade policy (World Bank, 2008). Apart from the elimination of tariffs on imported raw materials, equipment and capital goods in a number of sectors, it expanded its use of export promotion tools. The World Bank supported these efforts through a loan for the Export

[6] The fact that potential exporters are not fully informed about foreign market opportunities is not sufficient, in itself, to create a market failure if information production is costly but appropriable. By contrast, a market failure could arise in the presence of imperfect appropriability of the information. Indeed, Volpe and Carballo (2008), citing McDermott (1994), note that "customer lists are the most common target of corporate spies."

Development Project (EDP) implemented in two phases: 2000-2004 for the first phase and 2005-2009 for the second phase. The Export Promotion Centre (CEPEX in its French acronym) was the key agency under the Ministry of Trade responsible for implementing Tunisia's export promotion activities.

Our analysis focuses on FAMEX, a major demand-driven program under the second phase of the EDP project—whose aim was to help Tunisian firms overcome barriers to sell in foreign markets and enhance their competitiveness.[7] The program's rationale was that Tunisian firms were poorly informed about export markets and had difficulty identifying the right target markets, product segments, and sales channels.

The program provided firms with matching grants co-financing 50 percent of the cost of export business plans. In terms of firm size, the minimum annual turnover required for FAMEX eligibility was TND 200,000 (144,000 USD) in manufacturing and TND 100,000 (71,000 USD) in service and craft sectors.[8] In terms of age, only firms that had been in operation for a minimum of two years were eligible for FAMEX, but there were a few exceptions. A firm approaching FAMEX for assistance had to submit an export business plan focused on one of three possible objectives: (i) become a substantive exporter (if the firm had little or no export experience), (ii) to diversify its destination markets, or (iii) to develop new export products.[9] While a single main objective had to be provided for each export business plan, firms could request assistance for other objectives. The export business plan was evaluated by a panel of five local and international experts and, if accepted, the FAMEX team would provide technical assistance—outsourced to specialized consulting firms—to help the firm improve its plan. The panel would draw up, together with the firm, a list of activities eligible for matching grants of up to 50 percent of their cost, with a ceiling of TND 100,000 (71,000 USD).

FAMEX received 1,710 applications and accepted 1,231 from 1,060 firms between 2005 and 2009.[10] In terms of primary objective to request FAMEX assistance, , 31% had little or no export

[7] The FAMEX program also helped to build the institutional capacity of local professional organizations (export associations, chambers of commerce, and professional consulting organizations) to enable them to support Tunisian exporters more efficiently. Another component of the EDP project focused on trade facilitation, including investments and technical assistance to modernize customs procedures, through a combination of investment in hardware and software and procedural improvements. These components—if effective—benefited Tunisian firms broadly and thus do not contaminate the identification of FAMEX effects.

[8] Tunisian dinars are converted to USD using the exchange rate as of October 10, 2011 (1 USD = 1.463 TND).

[9] While there was no clear rule on which firms were deemed to have little export experience, in interviews with the authors, FAMEX management officers indicated that they included in that category firms that either did not export or exported an amount representing less than 20% of their total sales in the recent past.

[10] Some firms applied to FAMEX twice and had two export development plans accepted prior to 2009, some firms started a second export development plan in 2009, and some firms dropped one export development plan before re-

experience while 69% of the beneficiaries were already exporters and wanted to diversify either by expanding into new destination markets (49%) or into new products (20%). The program's coverage was fairly broad in terms of sectors and locations (see Section 3).

FAMEX grants were used mostly to co-finance the cost of technical assistance and marketing services provided by local and foreign experts. Five types of activities were financed: (i) market prospection, (ii) promotion, (iii) product development, (iv) firm development, and (v) foreign subsidiary creation. The amounts disbursed by FAMEX for each type of activity along with a description of the activities are shown in Table 1. In terms of actual disbursements, shares in the program total in the second column add up to 100 percent, but the number of firms in the third column adds up to more than the total number of FAMEX beneficiaries because each firm typically received co-financing to undertake several activities.[11]

As FAMEX was a matching grant program in which firms contributed half the costs, the program's management team expected firms' incentives to be aligned with the program, so that funds were unlikely to be misallocated or devoted to low-value services. The fact that FAMEX operated on a reimbursement basis, whereby firms had to present receipts upon implementing the activities in their plan, gives us reasonable confidence that the matching grant funds were used for their intended purpose.[12] These features of FAMEX make it a particularly attractive program to evaluate.[13]

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applying to FAMEX. The FAMEX management team did not systematically collect information on firms whose applications to FAMEX were turned down.

[11] A different breakdown of the activities in Table 1 provided by FAMEX program data indicates that 25 percent of FAMEX funds covered marketing research costs, 18 percent covered fees from private export-marketing consultants, 15 percent covered the participation in trade fairs, 10 percent went to establishing foreign representations, 10 percent covered printing costs for advertising material, and the rest was scattered over minor items.

[12] Moreover, FAMEX beneficiaries were obliged to supply the FAMEX management team with data to allow a general assessment of the project's impact on export growth, and supervision teams from the World Bank also had access to that information. However, like that of any assistance program, the impact of FAMEX on its stated activities could be reduced by fungibility. That is, $100 given to a firm for a given activity, even through a matching grant, could still have a "windfall effect" and allow the firm to re-optimize and spend less on the activity than it would have in the absence of the program. In that case, the program's money would (at least in part) replace money that the firm would have spent otherwise.

[13] Other World Bank-funded programs, for example in education, have been shown to suffer from a misuse of funds (Reinikka and Svensson, 2004).

Table 1
FAMEX Program Components

	Description of activities	Amounts disbursed (in millions of USD)	Share in program total	Number of firms
Market prospection	*Acquisition of information including for example the purchase of data or trade missions abroad to visit or participate in foreign exhibitions*	2.665	23.9%	313
Promotion	*Production of information and marketing including the design, production and publication of ads in various media, firm representation in fairs and exhibitions, and mailings.*	4.113	36.9%	319
Product development	*Product design modifications asnd production of samples and package design.*	1.515	13.6%	184
Firm development	*Training on organizational issues such as setting up a marketing watch, an export cell, or an export-oriented business plan.*	1.169	10.5%	220
Foreign subsidiary creation	*Assistance for the establishment of a facility abroad including legal, consulting, covering rental and salary costs for the first year of establishment.*	1.688	15.1%	84
Total		11.150	100.0%	

Note: Tunisian dinars were converted into U.S. dollars at the October 10, 2011 exchange rate (1.463 TND per USD).

3. Data and Descriptive Statistics

In order to evaluate rigorously the impact of FAMEX we need data on beneficiary firms as well as a control group. Our dataset combines three main sources: (i) FAMEX program data, (ii) data from the National Statistical Institute (INS in its French acronym) and the Investment Promotion Agency (API in its French acronym), and (iii) customs transaction data.

First, we obtained from FAMEX's management team a complete list of the 1,060 beneficiary firms indexed by their tax ID. After dropping 120 dropouts, 175 firms in the services sector for which customs transaction data is not available, and 310 firms whose first export development plan was still ongoing at the end of 2009 (the before last year in our sample period), we were left with 455 FAMEX beneficiaries. For these firms we obtained program data covering the following variables: years in which the firms joined and terminated the program (which in most cases lasted for one year), firm location, sector, employment and total sales when it joined the program and when it left it, whether the firm had an in-house export unit prior to joining the program, its objective in applying to FAMEX and its grant use in terms of total disbursements and breakdown across activities.

Second, we requested from the INS a stratified sample of control firms with a structure similar to that of the 455 FAMEX beneficiaries. The stratification was performed based on size, prior exporting status, and sector (within manufacturing), resulting in 48 cells. For each cell we asked

INS to provide us with twice as many non-beneficiaries as there were FAMEX beneficiaries, i.e., in total, 910 control firms. To draw the stratified sample of control firms, INS used its 2007 census of firms which includes information on firm location, sector, date of creation, employment, and total sales, with the last two variables being defined in terms of discrete intervals.[14] Since INS data was incomplete for a number of firms, we supplemented it with data obtained from the API. API's database for 2007 includes employment, sector, date of firm creation, social capital, and status (offshore or common law) for 5,000 firms across all sectors (of which 500 are also in the INS census). We extracted a group of 2,000 manufacturing firms from the API database that were neither in the FAMEX sample nor in the INS sample.

Third, we obtained transaction-level export data from Tunisian Customs for the 455 FAMEX beneficiaries, the 910 control firms from INS, and the 2,000 control firms from API. For every year between 2000 and 2010 and every firm (identified by its tax ID) we obtained monthly export transaction values by destination country and product code, the latter using an 11-digit Tunisian nomenclature derived from the Harmonized System (HS).[15] We aggregated monthly data to annual export totals for each firm and year.

Combining data from all three sources, and after data consistency checks and cleaning, we obtained an unbalanced panel of yearly export activity for 2,746 exporting firms with an average of six years of data per firm over the period 2004-2010.[16] Of those, 401 benefitted from FAMEX and 2,346 did not. Among the 2,346 non-beneficiaries, 71 applied to FAMEX but were turned down while 126 dropped out. In some robustness specifications, we will include the 126 dropouts in the treatment group instead of the control group while in others we will eliminate them from the sample. In the combined dataset, firm-level characteristics other than those related to export transactions are time-invariant, being available only for 2007.

Our combined dataset has two important features. First, the merger of customs transaction data with other sources of firm-level data ensures that the outcome variables do not suffer from recall bias, as would be the case if the outcome variables were obtained from survey data. Second, the

[14] The employment intervals are 1-9 employees, 10-19 employees, 20-49 employees, 50-99 employees, 100-199 employees, and more than 200 employees. The total sales intervals are: under TND 50,000, TND 50,000-1 million, TND 1 million-2 million, TND 2 million-5 million.

[15] The data was converted to HS 6-digit by keeping the classification's first six digits.

[16] The merging of the three data sources was possible thanks to the use of unique tax IDs by all Tunisian administrations concerned and their willingness to share the data with us. Some of the data inconsistencies addressed were wrong sectoral classifications in the FAMEX program data which were corrected using INS and API data.

fact that all control firms are exporting firms (as stratification was based on prior exporting status) improves overall sample homogeneity and the identification of FAMEX effects.[17]

Table 2 provides descriptive statistics for FAMEX and control firms in terms of sector, location, and employment categories. The sectoral distribution of FAMEX and control firms is quite similar with the exception of the textiles & apparels sector which is more heavily represented in the control group, although it is also the treatment group's largest sector, accounting for 31 percent of beneficiaries. Location was also used for stratification, hence the geographical distribution of FAMEX and control firms is fairly similar, although FAMEX firms are more concentrated in Tunis. There are also only minor differences across size categories measured in terms of employment and the same is true in terms of sales (unreported).

Table 2
Summary Statistics

a) Distribution by Sector

Sector	Agro-industry (%)	Textile & apparels (%)	Paper, wood & furniture (%)	Chemicals (%)	Metals (%)	Machine & equipment (%)	Electric (%)	Total number of firms
FAMEX firms	15	31	13	12	8	14	6	401
Control firms	11	43	9	11	7	11	7	2346

b) Distribution by Region

Location	Tunis	Grand Tunis	Central Sea	Rest of Tunisia	Total number of firms
FAMEX firms	22	48	28	2	401
Control firms	10	46	37	8	2346

c) Distribution by Employment Category

Employment	[1,9]	[10,19]	[20,49]	[50,99]	[100,199]	>=200	Total number of firms
FAMEX firms	11	9	29	19	16	16	401
Control firms	5	12	31	23	17	12	2346

Source: Authors' calculations using the combined dataset.

[17] By "exporters", we mean firms having a customs code and having conducted at least one export transaction during the sample period 2004-2010.

Table 3 shows export trends between 2003 and 2010 for FAMEX and control firms, as well as for Tunisian manufacturing exports as a whole. There is no prima-facie evidence of superior performance by FAMEX firms. In fact, considering the sample period as a whole, they perform on average worse than control firms, and both groups perform substantially worse than the universe of Tunisian manufacturing exporters (represented by aggregate trade flows). Only in 2004-2005 was the performance of FAMEX firms superior, but this may be an exceptional period since almost half of FAMEX firms were treated in 2005. Interestingly, FAMEX firms seem to have been hit by the global financial crisis in 2007-2008 earlier than control firms, recording a six percent drop in total exports while control firms still had positive export growth. During the recovery, FAMEX firms were growing at a slightly slower pace than control ones. This might indicate risk-taking behavior by treated firms—exposing themselves to destination markets that stood to contract most at the outset of the crisis and that experienced a slower recovery. We will return to this conjecture later on in the paper. Also worth noting is that our overall sample, including FAMEX and control firms, accounts for a substantial share of total Tunisian exports.[18]

Table 3
Growth in Tunisia's Exports

	2003-2004	2004-2005	2005-2006	2006-2007	2007-2008	2008-2009	2009-2010	2003-2010
Growth in total exports of:								
FAMEX firms	16%	27%	3%	12%	-6%	-12%	2%	42%
Control firms	24%	6%	7%	18%	3%	-16%	4%	51%
Tunisia	21%	8%	13%	25%	21%	-21%	8%	95%
Share of exports by FAMEX and control firms in Tunisia total exports	59%	60%	61%	57%	50%	49%	55%	53%

Source: Authors' calculations using the combined dataset and data from COMTRADE.
Note: The row 'Tunisia Total' shows the growth in exports for the country as a whole excluding phosphates based on COMTRADE data.

4. Estimation Issues

The main identification problem in evaluating the impact of FAMEX on firm-level export outcomes is that program assignment is non-random, so FAMEX beneficiaries may differ substantially from other firms in characteristics that affect both participation decisions and

[18] Exports by FAMEX firms account, on average, for about 10 percent of Tunisia's total exports over the sample period.

outcomes. This classical problem of non-experimental impact evaluation methods requires estimation approaches that control for selection bias.[19]

As a first approach, we use a propensity-score matching difference-in-differences (PSM-DID) estimator which has been widely used in program evaluation, in particular for export promotion (Görg, Henry and Strobl, 2008; Volpe and Carballo, 2008).[20] The PSM-DID method controls for selection bias by comparing changes in an outcome for program beneficiaries and 'observationally similar' control firms before and after the program.[21] It is based on the twin assumptions that (i) assignment to treatment (or the decision to undertake it) is independent of potential outcomes, conditional on observed pre-treatment covariates (Hirano, Imbens, and Ridder, 2003); and (ii) there is sufficient overlap in the distribution of propensity scores between the treatment and control groups to find matches for all or most treated firms. By relying on a comparison of changes in an outcome, the PSM-DID estimator also controls for unobserved time-invariant pre-program differences across firms, which could lead to self-selection into the program and influence outcomes (Blundell and Costa Dias, 2009). In our study, 'observationally similar' firms in the control group will be defined using a rich set of observable firm covariates.

Formally, let T and C be the treatment and control groups, respectively, and S be their common support.[22] Indexing firms by i and years by t, let y_{it} be an export outcome variable (say, total export sales, a product count, or a destination count), $t(i)$ the year in which firm i enrolled in the one-year FAMEX program, and D_{it} a binary treatment indicator defined as:

$$D_{it} = \begin{cases} 1 & \text{if } i \in T \text{ and } t = t(i) \\ 0 & \text{otherwise.} \end{cases} \tag{1}$$

Suppose first, for simplicity, that all firms underwent treatment in the same year τ, and let $y_{i,\tau-1}$ and $y_{i\tau}$ be firm i's outcome in the year before treatment and in the treatment year, respectively. Using log-changes, the PSM-DID estimator is given by:

$$\gamma^{PSM-DID} = \sum_{i \in T \cap S} \left[\Delta ln(y_{i\tau}) - \sum_{j \in C \cap S} w_{ij} \Delta ln\left(y_{j\tau}\right) \right] \tag{2}$$

where

[19] This is the fundamental problem of causal inference defined by Holland (1986).
[20] The seminal study is Heckman, Ichimura, and Todd (1997).
[21] The rationale underlying PSM-DID is the idea of reproducing the treatment group among the control group and thus reestablishing "the experimental conditions in a non-experimental setting" (Blundell and Costa Dias, 2009). The matching assumptions ensure that the only remaining difference between the groups is program participation.
[22] The common support is the range of estimated propensity scores for which the frequency of both treated and control firms is non-zero.

$$\Delta ln(y_{i\tau}) = ln(y_{i\tau}) - ln(y_{i,\tau-1}) \quad (3)$$

j designates control firms and the weights w_{ij} are determined by a propensity-score matching algorithm.[23]

In our setup, a complication arises because the treatment year is not the same for all firms: some firms joined FAMEX in 2005 and others in each subsequent year up to 2009. Letting $t(i)$ be firm i's treatment year, the before-after difference in outcomes is thus:

$$\Delta ln(y_{i,t(i)}) = ln(y_{i,t(i)}) - ln(y_{i,t(i)-1}) \quad (4)$$

instead of Eq. (3). This expression is well defined for treated firms but not for control firms, for which there is no treatment year. In standard statistical packages for propensity-score matching estimation (such as psmatch2 in STATA), treated firms are matched with control firms *in any year*, which may be problematic if calendar time matters for performance.

We address this issue by using a weighted least squares (WLS) estimator shown by Hirano, Imbens, and Ridder (2003) (henceforth HIR) to be a good alternative to PSM-DID.[24] The HIR estimator uses the estimated propensity scores as weights (rather than as a matching variable) in a difference-in-differences treatment-effect regression. Formally, let $\hat{p}_i$ be the estimated propensity score of firm i (be it a treated or control one) and $\hat{r}_i = \hat{p}_i / (1 - \hat{p}_i)$ its estimated odds. The HIR estimator's weighing scheme is

$$\omega_i = \begin{cases} 1 & \text{if } i \in T \cap S \\ \hat{r}_i & \text{if } i \in C \cap S. \end{cases}$$

That is, the scheme assigns a unit weight to all treated firms and a weight equal to $\hat{r}_i$ to each control firm, weighing more heavily those that have a higher propensity score (the more "treatable" firms among the untreated). The advantage of a regression framework is its flexibility

[23] The propensity-score matching algorithm matches each treated firm with the set of control firms with the closest propensity score. Depending on the matching method, there can be for each treated firm either one matched control firm or several, in the latter case with a weighting scheme as in Eq. (2). Nearest-neighbor matching identifies the firm in the control group with the closest propensity score to that of treated firm i, usually with a predefined maximum distance or caliper. If no control firm is found within that distance, i is ignored. Nearest-neighbor matching can be extended to any fixed number of matches. Kernel matching, the alternative method we will use, uses a weighted average of *all* control-group firms in the common support as a match for *each* treated firm i. See Caliendo and Kopeinig (2008) for additional details on matching.

[24] See DiNardo, Fortin and Lemieux (1996) and Van de Walle and Mu (2007) for applications of the method. HIR show that the WLS regression estimator of the average treatment effect is actually more efficient than the PSM-DID estimator.

in including covariates and controls, in particular year fixed effects which matter for us given that our sample period includes large macroeconomic swings.

In a PSM-DID context, one concern with the treatment indicator as defined in Eq. (1) is that it would allow the PSM algorithm to match a treated firm with a treatment group firm (possibly itself) after the treatment is over, in essence considering treated firms after treatment as if they were control firms. In a WLS context, there is no matching so this concern does not apply. However Eq. (1) considers a treated firm symmetrically before the treatment and after it is over, thus building in an assumption that the treatment effect is transient. An alternative coding of D_{it} as equal to one for all years after treatment would build in the opposite assumption, namely that the treatment effect was permanent. In order to get around this, we follow standard practice and code D_{it} as missing value for all $t > t(i)$ and all $t < t(i) - 1$. Thus, we are left with an unbalanced panel in which control firms are observed in all their sample years whereas treated firms are observed only in their treatment year and the year before.

Formally, our baseline equation can be written as a weighted difference-in-differences (DID) regression with the weights defined as above:

$$\Delta ln(y_{it}) = \alpha + \beta D_{it} + \boldsymbol{X_{it}\gamma} + \delta_t + u_{it} \tag{5}$$

where δ_t controls for calendar year effects (macroeconomic cycles) and $\boldsymbol{X_{it}}$ is a vector of firm covariates. Those include firm age and its square, location and sector fixed effects, a categorical variable for firm employment, a dummy variable identifying whether the firm exports 100 percent of its output, lagged total export value, the lagged number of export products and the lagged number of destinations served.

The regression in Eq. (5) restricts the measured treatment effect to its short-term impact on $ln\left(y_{i,t(i)}\right) - ln\left(y_{i,t(i)-1}\right)$. In order to measure the persistence of the treatment effect on the growth and the levels of the outcome variables, we recode either the treatment or the outcome variables as follows.

First, in order to test for the persistence of treatment effects on outcome *growth*, for a given lag $k = 0,\ldots,5$, we lag the treatment variable by k years, rewriting Eq. (5) as:

$$\Delta ln(y_{it}) = \alpha + \beta D_{i,t-k} + \boldsymbol{X_{it}\gamma} + \delta_t + u_{it} \tag{6}$$

We will use this to explore whether a firm treated in, say, 2005 experienced higher growth in the outcome than control firms between 2005 and 2006 (k = 1), between 2006 and 2007 (k = 2), and so on. The regression in Eq. (6) will be estimated separately for each k.

Second, in order to test for the persistence of treatment effects on outcome *levels*, we take "long" differences in the outcome variable relative to the year prior to treatment:

$$\Delta_k ln\left(y_i^{t(i)+k}\right) = ln\left(y_{i,t(i)+k}\right) - ln\left(y_{i,t(i)-1}\right) \quad (7)$$

Eq. (5) can thus be rewritten reflecting the transformation in Eq. (7) to become:

$$\Delta_k ln(y_{it}) = \alpha + \beta D_{it} + \boldsymbol{X_{it}\gamma} + \delta_t + u_{it} \quad (8)$$

This will test for cumulative treatment effects, i.e., whether a firm treated in 2005 was still ahead of control firms in 2006 (k = 1), in 2007 (k = 2), and so on. The regression in Eq. (8) will be estimated separately for each k.

In both cases, sample size is reduced as k rises, as lagged treatment or long differences in the outcome variables are not defined for firms treated in 2009; firms treated in 2008 have only k = 1 lagged treatment or long differences, and so on. Only firms treated in 2005 have a full set of lagged treatments and long differences; so as k rises the treatment group shrinks progressively to that of firms treated in 2005. However, the sample shrinkage is limited as later FAMEX cohorts were relatively small compared to the 2005 cohort. Moreover, since the WLS regressions condition on the year of treatment through year fixed effects, this is not a major problem. Nevertheless we will address this issue in a robustness check in Section 6.

Finally, as an alternative approach to deal with differences in treatment year across firms, we will use as a robustness check a PSM-DID estimator that restricts control firms matched to treated firm i to be taken in year $t(i)$, following Todo (2011).[25]

Before turning to estimation results, a caveat is in point. While Glazerman, Levy, and Myers (2003) show that PSM successfully reduces selection bias in impact evaluation, particularly when combined with DID or WLS regression (these methods are indeed pervasive in the evaluation of public programs), it has well-known limitations. In particular, estimates may still be biased if unobserved *time-varying* firm characteristics affect both participation and outcomes.

[25] Todo (2011) uses PSM-DID estimators to evaluate the impact of Japanese aid-funded technical assistance programs on Indonesian foundry firms, based on propensity score matching done year by year.

In a non-experimental study, selection bias on time-varying unobservables (say, management changes) can never by fully ruled out.

5. FAMEX Effects: Short-Term Impact and Persistence

This section presents and discusses estimates from the first-stage propensity score matching as well as treatment-effect estimates, along the intensive and extensive margins, using both PSM-DID and WLS regressions. We conclude it with an assessment of the robustness of the estimates.

5.1. First-Stage PSM Estimates

Propensity scores are retrieved from a cross-sectional probit regression of the probability of receiving a FAMEX grant in any year between 2005 and 2009 on the same set of firm covariates included in the treatment-effect equation listed above.[26] As shown in Appendix Table A.1, the probability of treatment correlates positively with location in Tunis and the number of export products and destinations. It correlates negatively with total exports and 100-percent exporter status. Sector dummies are all insignificant, suggesting no sectoral targeting.

Appendix Figure B.1 shows the propensity-scores distributions for the treatment and control groups. They have a large common support, which includes 401 FAMEX beneficiaries and 2,346 control firms. They are also fairly similar in shape, although not completely overlapping, highlighting the importance of matching.[27] Following Rosenbaum and Rubin (1985), Dehejia and Wahba (2002) and Smith and Todd (2005), we run balancing tests to assess whether matching corrects for significant differences in the distribution of pre-treatment covariates between the treatment and control groups (Caliendo and Kopeining, 2008). They are all satisfied (see Appendix B).

5.2. Treatment-Effect Estimates

Table 4 shows PSM-DID estimates using kernel matching[28] and WLS regression estimates using HIR weights for three firm-level outcomes: (i) total exports in Tunisian dinars, (ii) the number of export destinations, and (iii) the number of export products, all in log-differences, and pooling together firms treated in different years.

[26] We refrain from matching on sector because the small size of sector sub-samples would drastically reduce the matching's quality.
[27] The importance of a large common support and similarity in the distribution of covariates/propensity scores across treated and control groups for unbiased matching DID estimators is discussed by Heckman, Ishimura and Todd (1997).
[28] We also used five-nearest neighbors matching with caliper 0.1. Results, available upon request, are very similar.

PSM-DID standard errors are based on the asymptotic variance estimator, as bootstrapped standard errors can be invalid in this context because their asymptotic properties are not known (Abadie and Imbens, 2006). However we obtain qualitatively similar findings when using bootstrapped standard errors. WLS regressions use robust White-corrected *t*-statistics.

The first two columns show the short-term impact effect, i.e., the effect in the treatment year $t(i)$ (henceforth designated as 'TY'), using PSM-DID in column (1a) and WLS in column (1b). Columns (2)-(6) show estimates of the persistence of treatment effect on growth using Eq. (6).

Table 4
Year-to-Year Effects of FAMEX on Export Outcomes

Difference Estimator	TY-(TY-1) PSM-DID (1a)	TY-(TY-1) WLS reg. (1b)	(TY+1)-TY WLS reg. (2)	(TY+2)-(TY+1) WLS reg. (3)	(TY+3)-(TY+2) WLS reg. (4)	(TY+4)-(TY+3) WLS reg. (5)	(TY+5)-(TY+4) WLS reg. (6)
Outcome							
Total exports	0.496	0.511***	0.251	-0.042	-0.157	-0.240	0.025
	[2.66]**	[3.08]	[1.55]	[-0.26]	[-0.83]	[-1.06]	[0.11]
R-squared		*0.17*	*0.14*	*0.11*	*0.09*	*0.11*	*0.11*
Nb. destinations	0.144	0.150***	0.086***	0.052**	0.021	0.036	0.059**
	[5.52]***	[6.10]	[3.70]	[2.10]	[0.84]	[1.11]	[2.07]
R-squared		*0.15*	*0.12*	*0.08*	*0.12*	*0.12*	*0.08*
Nb. products	0.145	0.147***	0.071**	0.049	0.008	0.060	0.097***
	[4.33]***	[4.68]	[2.22]	[1.59]	[0.23]	[1.59]	[2.58]
R-squared		*0.15*	*0.13*	*0.13*	*0.12*	*0.13*	*0.13*
Observations		12,263	12,214	9,803	7,401	4,975	2,607

Notes: T-statistics based on robust standard errors in brackets; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated and control firms in the common support. PSM-DID estimates are based on propensity scores obtained using kernel matching. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

The short-term impact effects in columns (1a) and (1b) are very similar and significant at the one percent level for all three outcome variables. Considering total exports (first row of Table 4), the estimate in column (1b) indicates that growth in total exports (measured by the log-difference) is 0.511 higher for FAMEX beneficiaries than for the control group. Short-term treatment effects at TY are also positive and highly significant for destination and product counts.

Columns (2)-(6) show different patterns of persistence for treatment effects depending on the outcome. For total exports (first row), treatment effects become insignificant as early as the year after treatment. For destination counts (second row) and product counts (third row), by contrast, growth rates remain significantly different between the two groups of firms throughout the sample period, suggesting that once FAMEX firms learnt how to break into new markets, they kept on adding more and more markets and retrenched less than control firms during the global financial crisis.[29]

Table 5
Cumulative Effects of FAMEX on Export Outcomes

Difference Estimator	TY-(TY-1) WLS reg. (1)	(TY+1)-(TY-1) WLS reg. (2)	(TY+2)-(TY-1) WLS reg. (3)	(TY+3)-(TY-1) WLS reg. (4)	(TY+4)-(TY-1) WLS reg. (5)	(TY+5)-(TY-1) WLS reg. (6)
Outcome						
Total exports	0.511***	0.723***	0.571**	0.272	0.043	0.200
	[3.08]	[3.59]	[2.57]	[1.02]	[0.13]	[0.52]
R-squared	*0.17*	*0.22*	*0.23*	*0.22*	*0.23*	*0.25*
Nb. destinations	0.150***	0.191***	0.190***	0.151***	0.143***	0.177***
	[6.10]	[6.93]	[5.91]	[4.18]	[3.03]	[3.22]
R-squared	*0.15*	*0.20*	*0.20*	*0.24*	*0.29*	*0.30*
Nb. products	0.147***	0.175***	0.178***	0.117**	0.156***	0.219***
	[4.68]	[4.70]	[4.42]	[2.51]	[2.66]	[3.37]
R-squared	*0.15*	*0.20*	*0.23*	*0.26*	*0.27*	*0.30*
Observations	12,263	12,124	9,664	7,238	4,839	2,524

Notes: T-statistics based on robust standard errors in brackets; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated and control firms in the common support. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

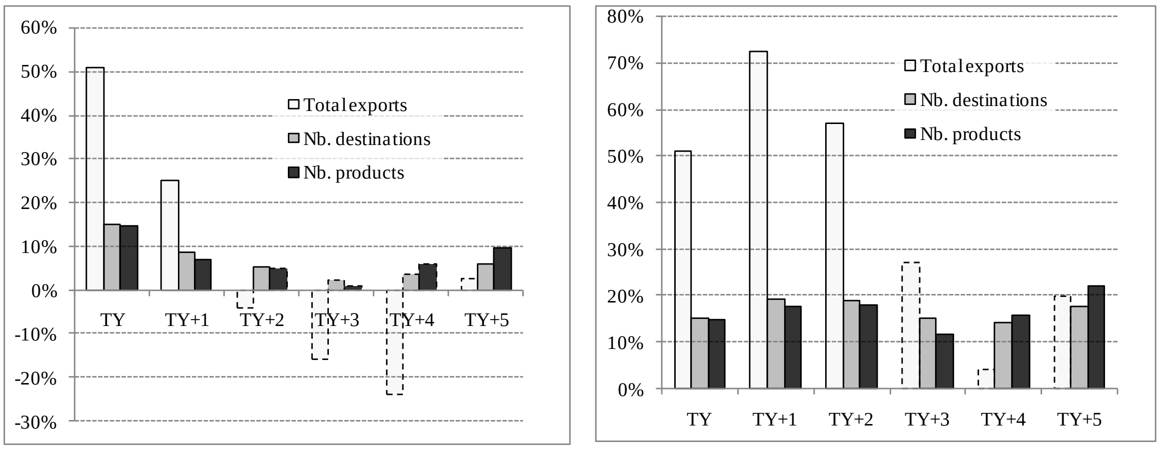
Table 5 shows cumulative treatment effects using long differences in log-outcomes as defined in Eq. (7). Thus, whereas the question in columns (2)-(6) of Table 4 was "for how long do growth trajectories diverge", the question in Table 5 is "for how long do outcome levels remain different after a temporary growth surge". Column (1) reproduces column (1b) of Table 4. Columns (2)-(6), which rely on increasingly longer differences, show again different patterns depending on the outcome. In the first row (export values), columns (4)-(6) show that cumulative effects on levels disappear three years after treatment. In the second and third rows (destination and product counts respectively), again cumulative effects persists, which is to be expected if growth rates keep on diverging.

Figure 1 summarizes in a bar chart the findings of Tables 4 and 5: the temporary effect on the intensive margin and the durable effect on the extensive margin.

[29] The latter part of the statement reflects the fact that column (3) onwards captures the years of the global financial crisis. In columns (3) and (4) the treated firms in the estimating sample are those receiving FAMEX in 2005 or in 2006 for which the TY+3 and TY+4 years are, respectively, 2008 and 2009 or 2009 and 2010. In column (5) the treated firms in the estimating sample are those receiving FAMEX in 2005 for which the TY+5 year is 2010, the last year in our sample.

Figure 1
Year-to-Year and Cumulative Effects

a. Year-to-Year Effects (Table 4) b. Cumulative Effects (Table 5)



Note: Bar heights show the point estimates of the coefficients in Table 4 and Table 5. Insignificant effects are shown as dotted bars.

All in all, results so far suggest a contrast between a large but transient treatment effect on total exports and moderate but persistent effects on destination and product counts.

5.3 Robustness

The treatment effects shown in Tables 4 and 5 are based on a sample which includes 126 FAMEX program dropouts in the control group. If those are more similar to FAMEX recipients than other control firms, their inclusion in the control group should improve the quality of the matching and the accuracy of the estimates of the treatment effect.[30] However, as a robustness check we re-estimate Eqs. (6) and (8) using two alternative sample constructions: (i) eliminating dropouts from the sample or (ii) including them in the treatment group. Results are shown in Appendix Table C.1. In accordance with intuition, the elimination of dropouts gives a slightly larger treatment effect, whereas the opposite is true when they are included in the treatment group. That is, dropouts performed better than other control firms but worse than the FAMEX firms that took their export business plan to full completion.

Second, the sample size shrinks across columns (2)-(6) of Tables 4 and 5 due to the fact that several differences in outcomes are not defined for firms that enrolled in the FAMEX program later in the sample period (as discussed in Section 4) and due to the unbalanced nature of the

[30] An ideal specification would consider a control group including only dropouts. However, such a specification cannot be estimated given the small number of dropouts relative to the number of FAMEX firms.

panel for control firms (as well as for treated firms).[31] We re-estimate the specifications in Tables 4 and 5 using a fixed sample across columns that is restricted to include only firms enrolled in FAMEX in 2005 and control firms (in the common support) operating continuously in export markets between 2004 and 2010. The results shown in Appendix Table C.2 are qualitatively maintained.

Third, as an alternative to the HIR procedure, we address the problem of 'time-wise mismatch', i.e., matching a firm treated in $t(i)$ with a control firm at time $t' \neq t(i)$, by using a procedure suggested by Todo (2011).[32] Todo's procedure pairs each treated firm with control firms observed in i's treatment year $t(i)$, generating, for each firm i treated in $t(i)$, a fictitious composite control with an outcome calculated as a weighted average of the outcomes observed in that year for control firms with propensity scores close to i's. This results in a new dataset whose size is just twice that of the treatment group and where each treated firm is matched with a unique composite control. We pool across all treatment years and re-estimate the equivalent of Eq. (5) by OLS. Results are shown in Appendix Table C.3 and are very similar to those of Table 5.

Finally, we re-estimate the regressions in Tables 4 and 5 using the number of HS 6-digit products exported instead of the number of 11-digit products (in Tunisia's nomenclature) exported, with very similar results (see Appendix Table C.4).

6. Extensions

6.1 Did FAMEX Assistance Encourage Risk-Taking or Facilitate Hedging?

The experimental economics literature has long noticed that individuals tend to be more willing to take risks out of windfall gains than out of regular earnings, a phenomenon called the "house money effect" (a term borrowed from gambling).[33] We noted in Table 3 that FAMEX firms performed worse in terms of export growth than control firms in the early stages of the global financial crisis. Although FAMEX was a matching-grant program rather than a pure subsidy—precisely in order to limit moral hazard—could it be nevertheless that export promotion encouraged beneficiary firms to take more risks?

[31] We are grateful to Beata Javorcik for attracting our attention to this issue.
[32] We are grateful to Yasusuki Todo for sharing his matching routine. See also Arnold and Javorcik (2009) for a discussion of time-wise mismatch.
[33] For instance, Thaler and Johnson (1990) asked two groups of individuals to choose between two lotteries with the same expected value, one riskier than the other. The treatment group was given a prior endowment (the windfall) while the control group was given no endowment. They report that 77 percent of the treatment group's individuals chose the risky lottery against only 44 percent for the control group.

As a preliminary, it is worth noting that the treatment-induced growth at the extensive margin (destinations and products) observed in Tables 4 and 5 did translate into lower concentration. To verify this, we re-estimate Eq. (5), our basic treatment-effect equation, using, as new outcome variables, Herfindahl and Theil concentration indices across product-destination cells at the firm level. The results are reported in Table 6 and suggest that concentration indeed declines for treatment firms relative to control ones, with an effect that seems to have been persistent.

Table 6
Effects of FAMEX on Concentration and Volatility

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)	Before-After 2005
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome							
Herfindahl index	-0.131***	-0.116***	-0.151***	-0.183***	-0.172***	-0.303***	
	[-4.35]	[-2.91]	[-3.61]	[-3.93]	[-3.03]	[-4.26]	
R-squared	*0.08*	*0.11*	*0.09*	*0.11*	*0.11*	*0.14*	
Theil index	-0.021***	-0.021***	-0.027***	-0.030***	-0.031***	-0.052***	
	[-4.82]	[-3.57]	[-4.35]	[-4.33]	[-3.68]	[-4.82]	
R-squared	*0.10*	*0.14*	*0.12*	*0.14*	*0.16*	*0.17*	
Observations	7,743	7,308	5,627	4,059	2,629	1,326	
Coeff. variation of exports							0.023
							[0.36]
R-squared							*0.17*
Observations							320

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample in columns (1)-(6) includes all treated firms and control firms in the common support. The sample in column (7) includes only treated firms in 2005 and control firms operating in export markets continuously from 1999 to 2010 (one observation per firm). The WLS regressions in columns (1)-(7) include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

However, a crude measure of export volatility at the firm level suggests that increased diversification failed to translate into lower risk. We use the time-wise coefficient of variation of total export turnover at the firm level before versus after FAMEX, considering only firms that enrolled in 2005, for which five years are observed both before and after treatment. For those firms as well as for a set of control firms which exported continuously between 1999 and 2010, we obtain additional customs data going back to 1999 to calculate the coefficient of variation over the five years prior to 2005. We then re-estimate Eq. (5) by WLS using as dependent variable the before 2005-after 2005 difference in the coefficient of variation. The results are shown in column (7) of Table 6 .[34] In spite of enhanced diversification, the evidence does not show any benefits from FAMEX in reducing export volatility for beneficiary firms relative to

[34] A large number of observations is lost by imposing the constraint of considering only firms with uninterrupted exports between 1999 and 2010 due to scant customs data for 1999.

control firms. Several reasons might lie behind this. Relative to control firms, FAMEX beneficiaries might have experimented with new destinations/products and as a result failed with higher probability; or they might have ventured into inherently more risky markets; or, finally, they might have expanded into markets whose returns (in terms of export sales) were correlated to their existing ones, thus failing to diversify their portfolio in a financial sense.

6.2. Did FAMEX Generate Spillover Benefits?

Estimated treatment effects can be biased in the presence of general equilibrium effects—not very likely in the case of an assistance program of limited scale like FAMEX—or by externalities "polluting" the control group's outcomes.[35] In particular, program benefits will be underestimated if the control group's export outcomes improve as a result of spillovers from beneficiaries. Thus, if one does not control for externalities, the absence of treatment effects has two possible interpretations: Either the program was ineffective, or it was effective but its benefits were not appropriable. This is not just a technical issue. Non-appropriability of benefits from information production (say, marketing research) in the presence of externalities can be seen as the fundamental reason for subsidized intervention by the Tunisian government and the World Bank, so the second case is precisely the one where government action can be justified by a market failure.[36]

In our case, the central result—lack of persistence of treatment effects on export values—might reflect catching up by control firms rather than vanishing benefits for treated firms, although catching up through imitation should apply equally—perhaps even more—to the extensive margin, for which we do observe permanent divergence. For instance, FAMEX beneficiaries' actions, such as participating in trade fairs or hiring export-marketing consultants, could have been visible to and easily imitable by other firms in their sector or location. Information acquired by FAMEX beneficiaries might even have been shared voluntarily with other firms, as exporters from the same country do not necessarily see themselves as competitors on foreign markets.[37]

[35] Formally, treatment effects are measured under the assumption of "stable unit treatment value assumption" (Rubin 1961), which means that a treated individual's outcome is independent of the treatment's mode of administration and of the status of other individuals (treated or not).
[36] Credit rationing is another market failure that could justify government intervention. However, if the government were to provide export credit services in lieu of deficient financial markets, the benefits would be appropriable and the services should be extended on a full-cost recovery basis rather than as a matching grant.
[37] Cadot, Iacovone, Pierola, and Rauch (2011) show that, for African exporters, expected survival rises with the number of firms from the same country exporting the same product to the same destination. Whether export entrepreneurship creates externalities that need to be supported by public action, as argued e.g. in Hausmann and Rodrik (2003), is still largely an open question.

The difficulty in investigating this issue explicitly is that the measurement of spillovers is elusive, especially when the transmission channel is unknown. Our exposure variable is a time-variant count of the number of FAMEX beneficiaries in each sector-region-year cell, assuming that firms are more likely to benefit from externalities if they produce similar goods in the same region.[38] We then regress *control-firm* outcomes on exposure to FAMEX beneficiaries. That is,

$$\Delta ln(y_{jsrt}) = \alpha + \sum_k \beta_k n_{jsr,t-k} + \delta_j + \delta_{st} + \delta_{rt} + v_{jsrt} \tag{9}$$

where s and r designate, respectively, sectors and regions and $n_{jsr,t-k}$ is the number of FAMEX beneficiaries in control firm j's sector-region in year $t-k$. Eq. includes firm fixed effects δ_j to account for unobserved firm heterogeneity in growth of export outcomes. It also includes sector-year fixed effects δ_{st} and region-year fixed effects δ_{rt} to control for shocks that could affect both outcomes and the number of firms receiving FAMEX support in a sector or region. The exposure variable enters with various lags to mitigate endogeneity and, more importantly, to allow for the slow diffusion of externalities.

Estimates for Eq. (10) shown in Table 7 fail to suggest any positive externalities; indeed, the only instances of significant coefficients for total exports in column (1) and for the number of products in column (12) are negative. We re-estimate a variant of Eq. (11) with a sample including all firms, both FAMEX beneficiaries and control firms, and again find no evidence of externalities (see Appendix Table C.5).

One might argue that spillovers to control firms are more likely to emerge from FAMEX firms whose objective was to expand into new destinations or to export new products. To address this possibility, we re-estimate Eq. (12) using a variant of the exposure variable that counts only FAMEX firms whose objective was to reach more export destinations or export more products. Not even this type of firms generated externalities to control firms (see Appendix Table C.5).

[38] Our spillover proxy follows standard practice. For instance, Krautheim (2011) develops a trade model with heterogeneous firms including a spillover effect from the number of exporters to the fixed costs of exporting. Empirical studies on export spillovers such as Aitken, Hanson, and Harrison (1997), Bernard and Jensen (2004) and Kneller and Pisu (2007) use the presence of exporters in an industry and location to capture spillovers. Recent studies using customs data such as Koenig, Mayneris, and Poncet (2010) and Mayneris and Poncet (2010) also test for the presence of spillovers using the numbers of exporters in the same region exporting similar products and/or to similar destinations. We also test for spillovers from the exporting activity itself and find no significant effect.

Table 7
Effect of Exposure to FAMEX Firms on Control Firms' Export Outcomes

Estimator	Within reg.				Within reg.				Within reg.			
Difference	t-(t-1)				t-(t-1)				t-(t-1)			
Outcome	Total exports				Nb. destinations				Nb. products			
	Sample of control firms only				Sample of control firms only				Sample of control firms only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure to FAMEX benef. t-1	-0.052*	-0.050	-0.016	-0.122	-0.003	-0.004	0.004	-0.000	-0.006	-0.006	-0.000	-0.022*
	[-1.79]	[-1.64]	[-0.39]	[-1.39]	[-1.04]	[-1.27]	[0.87]	[-0.03]	[-1.49]	[-1.56]	[-0.03]	[-1.95]
Exposure to FAMEX benef. t-2		0.004	0.037	-0.019		-0.002	0.005	-0.005		-0.001	0.005	-0.020
		[0.14]	[0.85]	[-0.18]		[-0.75]	[1.25]	[-0.47]		[-0.33]	[0.83]	[-1.44]
Exposure to FAMEX benef. t-3			0.012	-0.028			0.005	-0.004			0.006	-0.015
			[0.31]	[-0.28]			[1.39]	[-0.43]			[1.12]	[-1.14]
Exposure to FAMEX benef. t-4				-0.060				-0.008				-0.022**
				[-0.76]				[-1.11]				[-2.05]
Number of firms	2,620	2,620	2,618	2,618	2,620	2,620	2,618	2,618	2,620	2,620	2,618	2,618
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.02*	*0.02*
Observations	12,785	12,785	10,316	7,802	12,785	12,785	10,316	7,802	12,785	12,785	10,316	7,802

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%. The sample includes only control firms in the common support or outside.

6.3. Did Firms' Objectives and Use of FAMEX Support Matter?

The detailed information available on FAMEX allows us to exploit two dimensions potentially affecting the treatment effect: (i) the objective of the individual project supported by FAMEX, and (ii) the specific activities supported by the FAMEX grant. To our knowledge, this type of analysis is novel in the export promotion literature.

Tunisian firms had to state an objective when applying for FAMEX assistance, whether they wanted to: (i) become a significant exporter, (ii) export to a new destination market, or (iii) export a new product. Given the way the FAMEX application packages were structured, firms could state only *one* of these three objectives. Hence the project objectives partition the 401 treated firms into three non-overlapping groups: 95 came to FAMEX to become a more substantive exporter, 194 came to FAMEX to export to a new destination, and 112 came to FAMEX to export a new product.

We re-estimate treatment effects of FAMEX allowing the treatment effect to differ across objectives and show the results in Appendix Table D.1. Irrespective of the initial objective, new destinations show a significant and relatively long-lasting treatment effect. Conversely, firms that approached FAMEX wishing to expand into new destinations recorded significant treatment effects across all three outcomes, and those effects were typically larger than for firms coming with different objectives. Firms that approached FAMEX wishing to introduce new products recorded smaller treatment effects on all three dimensions. Lastly, firms that came to FAMEX wishing to become a significant exporter in terms of the exports to sales ratio recorded few significant results on all three dimensions. Together, these results suggest that export promotion is most effective in helping firms break into new markets. This is intuitive, as the penetration of new markets can be assisted by actions such as subsidizing visits to or firm/country stands in foreign fairs, which are standard business for export promotion agencies. By contrast, assisting firms in the introduction of new products is more technical and would require knowledge that export promotion teams or even local consulting firms may not possess.

Consider now the types of activities supported by the FAMEX program financed listed in Table 1. We replace the binary treatment variable used so far with a vector of continuous variables measuring, for each firm, the amount of FAMEX funding earmarked under each type of activity (the equivalent of the first column of Table 1, but at the firm level) which is available for 328 FAMEX beneficiaries.[39] Again, detailed results are omitted for brevity and shown in Appendix Table D.2. They show that market prospection activities and promotion activities have a significant effect on all outcome variables up to four years after treatment, with both activities exhibiting similar marginal returns on the dinar. Firm development has a significant positive effect on all outcomes but only four or five years after treatment, which may be due to the longer gestation period needed for such activities bring export benefits. By contrast, the other two types of activities have insignificant returns. Again, we see a broad correspondence with our earlier results: activities that support diversification have a clear positive return.

A large number of FAMEX firms—200—had an in-house export unit prior to the start of FAMEX assistance. It is natural to expect such firms to exhibit stronger performance since their in-house dedicated export units may enable them to make better use of the FAMEX assistance. To address this possibility we allow the treatment effect to differ according to whether the firm had an in-house export unit. Results in Appendix Table D.3 suggest that FAMEX beneficiaries with a dedicated in-house export unit exhibit stronger growth in total exports and in the number of destinations served, relative to other FAMEX beneficiaries.[40]

[39] Using the vector of amounts instead of bins for the different types of activities allows us to avoid the problems of multicollinearity across the individual components of the treatment that would arise due to their large overlap. For each activity, the amount entering in the WLS regressions is the amount co-financed at 50 percent by FAMEX. The total amount spent by the firm in that activity is twice as large. In this exercise we control for selection into FAMEX through the usual propensity score matching weighting scheme, but we do not control for selection into particular levels of support for each activity.

[40] We also examine whether firm size affects the magnitude of the impact of FAMEX assistance, classifying firms into two broad size categories: below 50 workers (196 firms) and above 50 workers (205 firms). We re-estimate Eq. (8) by WLS, allowing the FAMEX effect to differ across size categories. The estimates suggest that FAMEX has a

6.4 Survival of New Destinations and Products

We now explore whether new destinations and products added by FAMEX firms to their portfolios survive better than new destinations and products launched in the same years by control firms.[41] Let n_{it} be the number of destinations served by firm i in year t (with any of its products, so $n_{it} = \sum_d n_{idt}$), and $z_{i,t+k}$ the number of those destinations that are still served (with any product) in year $t + k$ and have been served uninterruptedly between t and $t + k$. Let

$$s_{itk} = z_{i,t+k} / n_{it} .$$

That is, s_{it1} is the one-year-forward survival rate, s_{it2} is the two-year forward survival rate, and so on. For treated firms, we restrict t to the be treatment year and the year before, so our dependent variable is the survival rate of new destinations (respectively products) introduced in the year before treatment or in the treatment year and surviving uninterruptedly for k years. Our estimable equation is thus:

$$s_{itk} = \alpha + \beta D_{it}\boldsymbol{X_{it}\gamma} + \delta_t + \varepsilon_{it} \tag{13}$$

We estimate Eq. (14) by weighted Tobit using HIR weights with left and right censoring since the dependent variable varies between zero and one. The results are shown in

Table 8 for k = 1,…,5 across columns (1)-(5).

There is no evidence of reduced sustainability of new destinations and products introduced in the treatment year, be it because of experimentation or a ‘windfall effect’ inducing excessive risk-taking. Indeed, there is evidence of improved sustainability of new destinations up to three years forward for treated firms, compared to new destinations introduced by control firms.

strong effect in the year of treatment for both small and large firms, and the magnitudes of the effects are quite similar across size categories for all export outcomes (see Appendix Table D.3).

[41] We focus on the survival of new products at the HS 6-digit level.

Table 8
Survival of New Destinations and Products

Duration	TY to TY+1	TY to TY+2	TY to TY+3	TY to TY+4	TY to TY+5
Estimator	Weighted Tobit	Weighted Tobit	Weighted Tobit	Weighted Tobit	Weighted Tobit
	(1)	(2)	(3)	(4)	(5)
Outcome					
New destination survival rate	0.261	0.262	0.259	0.192	0.359
	(2.83)***	(2.76)***	(2.28)**	(1.43)	(2.37)**
R-squared	*0.019*	*0.036*	*0.041*	*0.067*	*0.076*
Observations	4,046	3,342	2,578	1,738	956
New HS 6d product survival rate	0.034	0.033	-0.032	0.056	0.135
	(0.52)	(0.44)	(-0.33)	(0.42)	(0.90)
R-squared	*0.030*	*0.040*	*0.046*	*0.043*	*0.059*
Observations	5,553	4,569	3,538	2,390	1,278

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes treated firms and control firms in the common support. The dependent variable is a survival rate. The weighted Tobit regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

6.5. A Tentative Cost-Benefit Assessment

Our baseline results suggest that FAMEX had a large and positive—albeit short-lived—effect on total exports of treated firms. We turn in this section to a very tentative cost-benefit calculation to estimate the rate of return of the FAMEX program *per firm*, laying out clearly at each step the assumptions made. The details of the cost-benefit calculations are provided in Appendix E which also presents the implied trajectory of exports for FAMEX and control firms.

We first consider FAMEX benefits based on the estimated effect of FAMEX on total export growth in the year of treatment: 0.511 (in Tables 4 and 5). This implies that FAMEX beneficiaries had 66.7 percentage points higher export growth than control firms.[42] Since the average annual total export growth for control firms in the 2004-2008 period was 8.35 percent, the estimated annual growth in total exports for a FAMEX beneficiary in the year of treatment is

[42] This figure is obtained as exp(0.511)-1.

13.9 percent.[43] Given average total exports per firm in 2004 (prior to FAMEX) of 2,308 thousand TND, the growth rates above imply that, in the year of treatment, exports for a typical FAMEX beneficiary would have increased to 2,629 thousand TND against only 2,501 thousand TND for a control firm, a gain of TND 129. After two years, the difference would have peaked at TND 483, and after three years it would become insignificant.

The average grant amount disbursed by the FAMEX program per firm was 21.7 thousand TND. Thus, on impact in the treatment year, the implied rate of return on public funds would be almost 6 TND of additional exports per TND of grant. Over the three years where FAMEX exports were significantly higher than those of control firms, the additional total exports per Tunisian firm generated per TND of publicly-funded grant would be 22.

Although consistent with the emerging body of empirical results on the impact of export promotion, these rates of return are surprisingly high. It is important to note that they are an upper bound, first because they are based on the grant-component cost of FAMEX and do not take into account the overhead administrative costs of the FAMEX program for which we have no information. Second, a more meaningful cost-benefit analysis would focus on the increase in producers' surplus generated by FAMEX, rather than simply the increase in aggregate exports but again we do not have the necessary data.

7. Concluding Remarks

Trade promotion policies are increasingly popular, but evidence on their impact is limited, in particular over the long run. Our paper adds to the small existing literature by taking a longer view of the impact of interventions. This longer-term perspective enabled us to ask three questions: (i) Is any export expansion along the intensive and extensive margins durable? (ii) If there is indeed diversification of exports, does it reduce the longer-term volatility of exports? (iii) Are there spillover benefits for other firms, especially over time? We find the Tunisian export promotion program –FAMEX– had a durable impact along the extensive margin in terms of destinations and products but provided along a temporary boost to exports along the intensive margin. The results are robust and supported by evidence on the specific ingredients of the program: market prospection and promotional activities prove most fruitful.

However, our results on the long-term impact of export promotion must be interpreted cautiously given that the later years of our sample period were characterized by the collapse of world trade,

[43] The choice of the 2004-2008 period instead of the 2004-2010 period to calculate the average annual total export growth is made to avoid the global financial crisis years after 2008. The figure of 13.9 percent is obtained as 8.35% * (1+66.7%).

which may not have affected all firms equally. In particular, treated firms may have ventured into riskier markets. Our evidence shows that the increased diversification of beneficiary firms did not result in reduced volatility of exports.

In terms of policy implications, the quasi-experimental approach suffers from a fundamental drawback, namely, that treatment effects, which are generally construed as favorable to policy intervention when they are significant, only reflect appropriable benefits. They give no indication on the presence of a market failure. Indeed, if anything, it is the absence of treatment effects that is consistent with the non-appropriability of benefits. We attempt to address this problem by testing directly for the presence of externalities. We find no evidence of positive externalities from treated firms to control ones.

All in all, whereas our results do suggest, like previous ones, that export promotion has a strong contemporaneous effect on the export performance of treated firms, they do not provide a compelling case for government intervention, as the effects we identify are neither sustained nor spilling over to untreated firms, two issues that had so far been largely overlooked in the literature.

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Appendix

Appendix A: Propensity Score Estimation

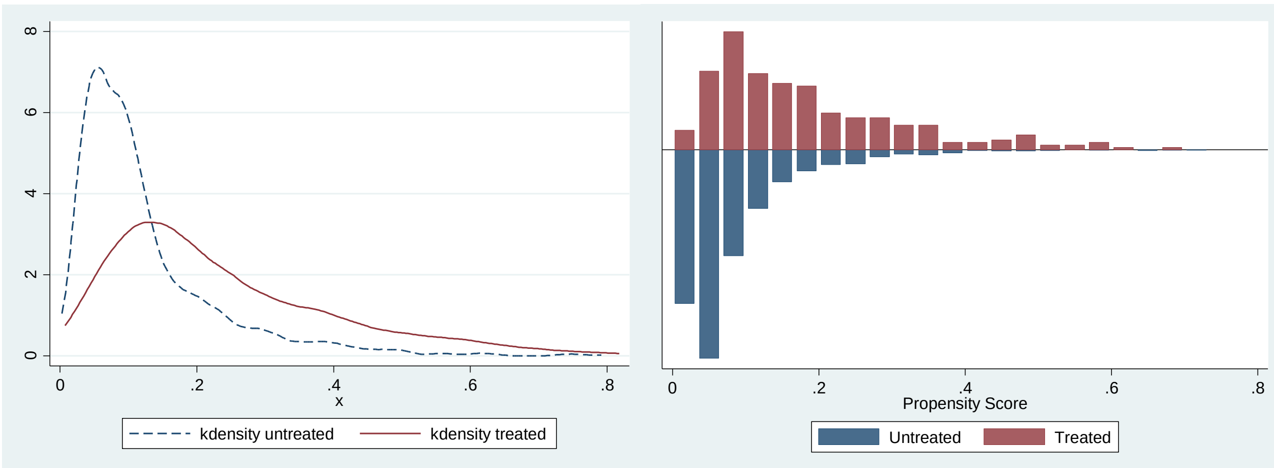
Table A.1 Probit Regression for the Propensity to receive FAMEX treatment

	FAMEX treatment status
Age	0.355
	[1.00]
Age squared	-0.098
	[-1.51]
Lagged total exports	-0.038***
	[-4.35]
Lagged number of exported products	0.158***
	[3.40]
Lagged number of export destinations	0.497***
	[8.58]
100% exporter	-0.341***
	[-4.91]
10-19 employees	-0.491***
	[-4.22]
20-49 employees	-0.359***
	[-3.61]
50-99 employees	-0.393***
	[-3.71]
100-199 employees	-0.385***
	[-3.41]
More than 200 employees	-0.411***
	[-3.37]
Textiles and apparels	-0.067
	[-0.76]
Paper, wood, and furniture	0.019
	[0.19]
Chemicals	-0.041
	[-0.41]
Metals	-0.021
	[-0.18]
Machine and equipment	0.017
	[0.17]
Electric	-0.111
	[-0.91]
Grand Tunis	-0.352***
	[-4.86]
Central Sea	-0.950***
	[-6.06]
Rest of Tunisia	-0.448***
	[-5.81]
Year fixed effects	Yes
Observations	12,263

Notes: T-statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. Unless noted, firm characteristics refer to 2007. The omitted sector is agro-industry, the omitted location is Tunis, and the omitted category in terms of employment is less than 10 workers.

Appendix B: Propensity Score Matching

Appendix Figure B.1. Densities and histogram of propensity scores, treatment and control groups



Source: Authors' calculations.

To assess the quality of the propensity score matching in balancing adequately the covariates between treatment and control groups, we conduct four types of tests. The first test is the balancing or stratification test proposed by Dehejia and Wahba (2002) which divides observations into strata based on the estimated propensity score and uses t-tests within each strata to test if the distribution of covariates is similar between the treatment and control group. such that there are no statistical differences between the mean of the propensity score in the treatment and control group. Implementing the test in stata as in Becker and Ichino (2002) over 6 strata of the propensity score shows that the balancing property is satisfied for our data.

The second set of tests shown in the first columns of Appendix Table B.1 consists in two-sample t-tests for the equality of sample means for all the covariates between treated and matched control groups. The t-tests indicate no significant differences in the means suggesting that the covariates are balanced in the two groups and thus the quality of our matching is high.

The third set of tests shown in the last columns of Appendix Table B.1 are the standardized biases for the covariates defined as the corresponding difference in sample means between treated and matched control groups normalized by the square root of the average of sample variances in both groups. The results show that the standardized bias for our covariates is in most cases lower than 5%. Caliendo and Kopeining (2008) suggest that a standardized bias of that magnitude after matching indicates high quality of the matching.

The fourth test is based on the comparison of the pseudo-R-squared of the propensity score estimated on the full sample versus on the matched sample, which explains how well the covariates explain the propensity to participate in the program. With a high quality matching, the pseudo-R-squared should be very low after matching because there should be no differences in the distribution of the covariates that can explain the propensity to participate in the program. Indeed, our pseudo-R-squared is 0.208 before matching and 0.006 after matching. Moreover, the associated likelihood-ratio test of the joint insignificance of covariates in the propensity score estimation on the full sample versus on the matched sample should indicate that the covariates are jointly insignificant in explaining participation after matching. Indeed our likelihood-ratio chi-squared test is 733.92 with a p-value of 0 before matching and 6.22 with a p-value of 1 after matching.

Table B.1 Balancing Tests

Covariates	Mean in Matched Sample		T-test		Percentage Bias	Percentage Bias Reduction
	Treatment	Control	T-statistic	P-value		
Age	2.707	2.710	-0.06	0.948	-0.5	96.3
Age squared	7.651	7.665	-0.06	0.955	-0.4	96
Lagged total exports	10.038	9.961	0.17	0.862	1.3	93.2
Lagged number of exported products	1.266	1.250	0.22	0.824	1.7	94.7
Lagged number of export destinations	1.048	1.032	0.27	0.79	2.1	96
100% exporter	1.365	1.368	-0.09	0.927	-0.7	97.6
10-19 employees	0.099	0.097	0.09	0.929	0.6	94.3
20-49 employees	0.292	0.302	-0.26	0.798	-1.9	33.3
50-99 employees	0.199	0.203	-0.13	0.901	-0.9	83.3
100-199 employees	0.155	0.163	-0.31	0.758	-2.3	-434.1
More than 200 employees	0.149	0.137	0.47	0.639	3.7	72.7
Textiles and apparels	0.334	0.329	0.15	0.884	1.1	95.1
Paper, wood, and furniture	0.136	0.119	0.64	0.519	5.2	49
Chemicals	0.113	0.125	-0.5	0.619	-3.8	-28.2
Metals	0.075	0.075	-0.02	0.982	-0.2	97.7
Machine and equipment	0.135	0.141	-0.21	0.831	-1.7	81.7
Electric	0.064	0.065	-0.06	0.95	-0.5	88
Grand Tunis	0.478	0.486	-0.21	0.834	-1.6	66.6
Central Sea	0.017	0.027	-0.98	0.33	-5.1	81
Rest of Tunisia	0.301	0.296	0.16	0.874	1.2	93.6
Year 2005	0.616	0.564	1.4	0.162	12	88.8
Year 2006	0.06077	0.08974	-1.46	0.146	-9.3	74.8
Year 2007	0.215	0.21	0.17	0.867	1.4	78.7
Year 2008	0.10773	0.1276	-0.82	0.414	-5.9	68.7
Year 2009	0	0.00837	-1.75	0.081	-3	94.3

Appendix C: Robustness of Main Results and Spillover Results

Table C.1 Effects of FAMEX on Export Outcomes – Alternatives for Dropouts

Panel A. Excluding Dropouts from the Sample

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.548***	0.743***	0.599***	0.269	0.051	0.279
	[3.26]	[3.65]	[2.65]	[0.99]	[0.14]	[0.69]
R-squared	*0.17*	*0.22*	*0.22*	*0.22*	*0.22*	*0.24*
Nb. destinations	0.154***	0.191***	0.192***	0.145***	0.143***	0.183***
	[6.20]	[6.88]	[5.85]	[3.93]	[2.93]	[3.22]
R-squared	*0.15*	*0.19*	*0.20*	*0.23*	*0.29*	*0.31*
Nb. products	0.150***	0.180***	0.185***	0.120**	0.167***	0.238***
	[4.74]	[4.77]	[4.51]	[2.52]	[2.72]	[3.48]
R-squared	*0.15*	*0.20*	*0.23*	*0.26*	*0.27*	*0.30*
Observations	11,645	11,506	9,172	6,872	4,598	2,405

Panel B. Including Dropouts in the Treated Group

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	DID reg.	DID reg.	DID reg.	DID reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.522***	0.609***	0.370*	0.239	-0.060	0.244
	[3.32]	[3.23]	[1.76]	[0.95]	[-0.18]	[0.65]
R-squared	*0.17*	*0.22*	*0.22*	*0.22*	*0.23*	*0.26*
Nb. destinations	0.139***	0.148***	0.153***	0.116***	0.119***	0.167***
	[6.13]	[5.78]	[5.19]	[3.45]	[2.60]	[3.18]
R-squared	*0.14*	*0.18*	*0.20*	*0.23*	*0.29*	*0.31*
Nb. products	0.116***	0.145***	0.143***	0.105**	0.132**	0.232***
	[4.18]	[4.44]	[3.92]	[2.49]	[2.31]	[3.64]
R-squared	*0.14*	*0.18*	*0.23*	*0.26*	*0.28*	*0.32*
Observations	11,950	11,735	9,327	6,970	4,653	2,453

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The sample includes treated and control firms in the common support. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products. In Panel A the sample includes 401 treated firms and 2220 control firms while in Panel B the sample includes 526 treated firms and 2220 control firms.

Table C.2 FAMEX Effects for Firms Treated in 2005

Panel A. Year-to-Year Effects of FAMEX on Export Outcomes

Difference	TY-(TY-1)	TY-(TY-1)	(TY+1)-TY	(TY+2)-(TY+1)	(TY+3)-(TY+2)	(TY+4)-(TY+3)	(TY+5)-(TY+4)
Estimator	PSM-DID	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1a)	(1b)	(2)	(3)	(4)	(5)	(6)
Outcome							
Total exports	0.425*	0.485**	0.387*	-0.007	-0.156	-0.308	0.058
	[1.67]	[2.32]	[1.89]	[-0.03]	[-0.73]	[-1.33]	[0.25]
R-squared		*0.16*	*0.15*	*0.11*	*0.09*	*0.11*	*0.11*
Nb. destinations	0.148***	0.156***	0.094***	0.061*	0.023	0.042	0.061**
	[4.30]	[4.81]	[3.00]	[1.76]	[0.68]	[1.18]	[2.13]
R-squared		*0.14*	*0.12*	*0.08*	*0.13*	*0.14*	*0.07*
Nb. products	0.162***	0.156***	0.073*	0.061	0.006	0.065	0.099***
	[3.56]	[3.72]	[1.65]	[1.45]	[0.14]	[1.58]	[2.62]
R-squared		*0.16*	*0.17*	*0.11*	*0.11*	*0.13*	*0.13*
Observations		2,524	2,524	2,524	2,524	2,524	2,524

Panel B. Cumulative Effects of FAMEX on Export Outcomes

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.485**	0.838***	0.636**	0.386	0.023	0.200
	[2.32]	[3.40]	[2.29]	[1.22]	[0.06]	[0.52]
R-squared	*0.16*	*0.21*	*0.21*	*0.21*	*0.22*	*0.25*
Nb. destinations	0.156***	0.208***	0.214***	0.175***	0.157***	0.177***
	[4.81]	[5.69]	[4.64]	[3.54]	[3.07]	[3.22]
R-squared	*0.14*	*0.19*	*0.20*	*0.26*	*0.30*	*0.30*
Nb. products	0.156***	0.190***	0.200***	0.158***	0.177***	0.219***
	[3.72]	[3.95]	[3.66]	[2.61]	[2.75]	[3.37]
R-squared	*0.16*	*0.21*	*0.22*	*0.24*	*0.26*	*0.30*
Observations	2,524	2,524	2,524	2,524	2,524	2,524

Notes: T-statistics based on robust standard errors in brackets; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated firms in 2005 and control firms in the common support that are present in the sample in every year from 2004 to 2010. The PSM-DID estimates are estimated based on propensity scores obtained using kernel matching. The WLS regressions include firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

Table C.3 Effects of FAMEX on Export Outcomes using Year-by-Year Matching

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	DID reg.	DID reg.	DID reg.	DID reg.	DID reg.	DID reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.503***	0.689***	0.486**	0.139	-0.25	-0.148
	[2.95]	[3.18]	(2.10)**	[0.51]	[-0.71]	[-0.37]
Nb. destinations	0.135***	0.161***	0.147***	0.100***	0.076	0.100*
	[5.35]	[5.64]	[4.51]	[2.69]	[1.54]	[1.81]
Nb. products	0.139***	0.155***	0.137***	0.058	0.094	0.130**
	[4.32]	[3.97]	[3.27]	[1.17]	[1.56]	[1.99]
Observations	802	802	798	716	560	516
Treated	401	401	399	359	280	258

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The DID regressions include treatment year effects.

Table C.4 FAMEX Effects on the Number of HS 6-Digit Products

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Nb. Products HS 6-digit	0.132***	0.165***	0.168***	0.109**	0.140**	0.208***
	[4.44]	[4.75]	[4.42]	[2.50]	[2.54]	[3.43]
R-squared	*0.144*	*0.201*	*0.232*	*0.270*	*0.274*	*0.309*
Observations	12,263	12,124	9,664	7,238	4,839	2,524

Notes: T-statistics based on robust standard errors in brackets; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated and control firms in the common support. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

Table C.5 Further Effects of Exposure to FAMEX Firms on Export Outcomes

Panel A. Spillovers for Sample of All Firms

Estimator	Within reg.				Within reg.				Within reg.			
Difference	t-(t-1)				t-(t-1)				t-(t-1)			
Outcome	Total exports				Nb. destinations				Nb. products			
	Sample of all firms				Sample of all firms				Sample of all firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure to FAMEX benef. t-1	-0.050*	-0.049*	-0.034	-0.114	-0.003	-0.004	0.001	-0.001	-0.004	-0.005	-0.003	-0.018*
	[-1.90]	[-1.76]	[-0.92]	[-1.52]	[-1.14]	[-1.22]	[0.16]	[-0.18]	[-1.16]	[-1.18]	[-0.65]	[-1.77]
Exposure to FAMEX benef. t-2		0.004	0.017	-0.041		-0.001	0.003	-0.007		-0.001	0.000	-0.024*
		[0.13]	[0.43]	[-0.44]		[-0.34]	[0.74]	[-0.79]		[-0.18]	[0.07]	[-1.89]
Exposure to FAMEX benef. t-3			-0.004	-0.055			0.001	-0.010			-0.000	-0.023*
			[-0.12]	[-0.62]			[0.21]	[-1.15]			[-0.01]	[-1.90]
Exposure to FAMEX benef. t-4				-0.072				-0.011				-0.023**
				[-1.02]				[-1.51]				[-2.39]
Number of firms	3,024	3,024	3,022	3,022	3,024	3,024	3,022	3,022	3,024	3,024	3,022	3,022
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.02*	*0.02*
Observations	14,786	14,786	11,926	9,013	14,786	14,786	11,926	9,013	14,786	14,786	11,926	9,013

Panel B. Spillovers from FAMEX Firms Looking to Increase Export Destinations and Exported Products

Estimator	Within reg.				Within reg.				Within reg.			
Difference	t-(t-1)				t-(t-1)				t-(t-1)			
Outcome	Total exports				Nb. destinations				Nb. products			
	Sample of control firms only				Sample of control firms only				Sample of control firms only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure to FAMEX benef. t-1	-0.052*	-0.050	-0.016	-0.122	-0.003	-0.004	0.004	-0.000	-0.006	-0.006	-0.000	-0.022*
	[-1.79]	[-1.64]	[-0.39]	[-1.39]	[-1.04]	[-1.27]	[0.87]	[-0.03]	[-1.49]	[-1.56]	[-0.03]	[-1.95]
Exposure to FAMEX benef. t-2		0.004	0.037	-0.019		-0.002	0.005	-0.005		-0.001	0.005	-0.020
		[0.14]	[0.85]	[-0.18]		[-0.75]	[1.25]	[-0.47]		[-0.33]	[0.83]	[-1.44]
Exposure to FAMEX benef. t-3			0.012	-0.028			0.005	-0.004			0.006	-0.015
			[0.31]	[-0.28]			[1.39]	[-0.43]			[1.12]	[-1.14]
Exposure to FAMEX benef. t-4				-0.060				-0.008				-0.022**
				[-0.76]				[-1.11]				[-2.05]
Number of firms	2,620	2,620	2,618	2,618	2,620	2,620	2,618	2,618	2,620	2,620	2,618	2,618
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.02*	*0.02*
Observations	12,785	12,785	10,316	7,802	12,785	12,785	10,316	7,802	12,785	12,785	10,316	7,802

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. In Panel A, the sample includes all FAMEX firms and control firms in the common support or outside. In Panel B, the sample includes only control firms inside the common support or outside that are in sector-location cells where FAMEX firms that required assistance with the objective of increasing their destinations or their exported products were present.

Appendix D: Extensions

Table D.1 Effect of Treatment Interacted with Project Objective

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Objective						
Total exports	Start exporting	0.467	0.898*	0.591	-0.028	-1.650*	-0.669
		[1.15]	[1.68]	[1.11]	[-0.04]	[-1.94]	[-0.76]
	New destinations	0.563**	0.515**	0.215	0.109	0.154	0.314
		[2.48]	[2.01]	[0.70]	[0.31]	[0.36]	[0.66]
	New products	0.184	0.650**	0.780***	0.946**	0.851	0.596
		[0.78]	[2.29]	[2.62]	[2.42]	[1.60]	[0.94]
	R-squared	*0.17*	*0.22*	*0.23*	*0.23*	*0.24*	*0.25*
Outcome	Objective						
Nb. destinations	Start exporting	0.144***	0.190***	0.178**	0.104	-0.065	0.068
		[2.66]	[2.79]	[2.50]	[1.34]	[-0.62]	[0.60]
	New destinations	0.171***	0.190***	0.176***	0.183***	0.173***	0.223***
		[4.95]	[5.20]	[4.01]	[3.74]	[2.89]	[3.26]
	New products	0.085**	0.166***	0.169***	0.104*	0.194***	0.147*
		[2.42]	[3.97]	[3.48]	[1.87]	[2.71]	[1.78]
	R-squared	*0.16*	*0.20*	*0.20*	*0.24*	*0.30*	*0.31*
Outcome	Objective						
Nb. products	Start exporting	0.130*	0.227***	0.256***	0.119	-0.004	0.082
		[1.89]	[2.62]	[2.87]	[1.23]	[-0.04]	[0.67]
	New destinations	0.156***	0.148***	0.093*	0.108*	0.164**	0.221***
		[3.43]	[3.08]	[1.71]	[1.74]	[2.31]	[2.84]
	New products	0.082*	0.148**	0.212***	0.158**	0.215**	0.278**
		[1.72]	[2.35]	[3.29]	[2.05]	[2.06]	[2.35]
	R-squared	*0.15*	*0.20*	*0.23*	*0.26*	*0.27*	*0.30*
	Observations	12,263	12,124	9,664	7,238	4,839	2,524

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The sample includes treated and control firms in the common support. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

Table D.2 Effects of FAMEX Program Components

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Activity (amounts in TND)						
Total exports	Market prospection	0.039**	0.048*	0.072**	0.101***	0.018	0.027
		[2.03]	[1.86]	[2.49]	[2.98]	[0.36]	[0.53]
	Promotion	0.028***	0.039***	0.029**	0.005	0.027	0.001
		[3.06]	[3.04]	[2.20]	[0.31]	[1.15]	[0.03]
	Product development	-0.014	-0.013	0.003	-0.045	-0.023	-0.023
		[-0.96]	[-0.55]	[0.18]	[-1.41]	[-0.67]	[-0.64]
	Firm development	-0.022	0.004	0.000	0.048*	0.087***	0.101**
		[-1.12]	[0.19]	[0.02]	[1.83]	[2.80]	[2.33]
	Foreign subs. creation	-0.003	0.026*	-0.019	-0.008	-0.047	-0.063*
		[-0.15]	[1.91]	[-0.64]	[-0.29]	[-1.59]	[-1.87]
	R-squared	*0.21*	*0.26*	*0.26*	*0.25*	*0.25*	*0.26*
Outcome	Activity (amounts in TND)						
Nb. destinations	Market prospection	0.007**	0.013***	0.013***	0.009*	0.010	0.008
		[2.07]	[3.29]	[2.95]	[1.72]	[1.56]	[1.08]
	Promotion	0.006***	0.006***	0.011***	0.006**	0.002	0.004
		[3.20]	[3.57]	[4.70]	[2.34]	[0.47]	[0.79]
	Product development	0.000	-0.001	0.003	-0.003	-0.003	-0.002
		[0.06]	[-0.28]	[0.98]	[-0.70]	[-0.69]	[-0.41]
	Firm development	0.001	0.004	0.007	0.009	0.017***	0.020***
		[0.27]	[1.02]	[1.39]	[1.49]	[3.09]	[2.73]
	Foreign subs. creation	0.000	0.001	-0.004	-0.004	-0.001	-0.009
		[0.00]	[0.68]	[-1.05]	[-1.23]	[-0.30]	[-1.58]
	R-squared	*0.17*	*0.21*	*0.24*	*0.25*	*0.31*	*0.32*
Outcome	Activity (amounts in TND)						
Nb. products	Market prospection	0.009**	0.009*	0.015***	0.014**	0.012	0.012
		[2.05]	[1.78]	[2.99]	[2.32]	[1.40]	[1.36]
	Promotion	0.004	0.006**	0.004	0.002	0.002	0.000
		[1.11]	[2.02]	[1.44]	[0.54]	[0.53]	[0.01]
	Product development	-0.003	0.004	0.004	-0.003	0.001	0.006
		[-1.39]	[0.90]	[1.09]	[-0.53]	[0.20]	[0.99]
	Firm development	-0.002	0.000	0.003	0.014*	0.025***	0.024***
		[-0.41]	[0.07]	[0.59]	[1.84]	[3.34]	[2.59]
	Foreign subs. creation	0.002	0.004	-0.003	-0.006	-0.005	-0.009
		[0.49]	[1.13]	[-0.75]	[-1.31]	[-0.79]	[-1.27]
	R-squared	*0.16*	*0.22*	*0.26*	*0.27*	*0.29*	*0.31*
	Observations	12,157	12,018	9,590	7,188	4,808	2,496

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The sample includes treated and control firms in the common support. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

Table D.3 FAMEX Effects, Firm Size and In-House Export Unit

Panel A. Firm Size

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Firm size						
	FAMEX*Less than 50 workers	0.512*	1.015***	0.673*	0.398	0.190	0.666
Total		[1.84]	[3.15]	[1.91]	[0.99]	[0.37]	[1.18]
exports	FAMEX*More than 50 workers	0.511***	0.444*	0.474*	0.149	-0.074	-0.138
		[2.82]	[1.83]	[1.74]	[0.42]	[-0.16]	[-0.26]
	R-squared	*0.17*	*0.23*	*0.23*	*0.22*	*0.23*	*0.25*
Outcome	Firm size						
	FAMEX*Less than 50 workers	0.151***	0.223***	0.182***	0.149***	0.099	0.166**
Nb.		[3.98]	[5.41]	[3.80]	[2.95]	[1.54]	[2.32]
destinations	FAMEX*More than 50 workers	0.149***	0.161***	0.197***	0.154***	0.178***	0.184**
		[4.69]	[4.40]	[4.59]	[2.99]	[2.63]	[2.35]
	R-squared	*0.15*	*0.20*	*0.20*	*0.24*	*0.29*	*0.30*
Outcome	Firm size						
	FAMEX*Less than 50 workers	0.179***	0.219***	0.215***	0.140**	0.153*	0.172**
Nb.		[3.58]	[3.91]	[3.57]	[2.13]	[1.84]	[1.98]
products	FAMEX*More than 50 workers	0.115***	0.133***	0.143***	0.094	0.159*	0.253***
		[2.97]	[2.68]	[2.66]	[1.43]	[1.95]	[2.80]
	R-squared	*0.15*	*0.20*	*0.23*	*0.26*	*0.27*	*0.30*
	Observations	12,263	12,124	9,664	7,238	4,839	2,524

Panel B. In-House Export Unit

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Export Unit Status						
	FAMEX*Has in-house export unit	0.532***	0.893***	0.767***	0.553	0.097	0.168
Total		[2.94]	[3.96]	[2.97]	[1.56]	[0.21]	[0.33]
exports	FAMEX*No in-house export unit	0.491*	0.554*	0.377	-0.003	-0.016	0.235
		[1.93]	[1.77]	[1.12]	[-0.01]	[-0.03]	[0.45]
	R-squared	*0.17*	*0.23*	*0.23*	*0.22*	*0.23*	*0.25*
Outcome	Objective						
	FAMEX*Has in-house export unit	0.162***	0.225***	0.248***	0.208***	0.197***	0.222***
Nb.		[4.96]	[6.32]	[6.00]	[4.13]	[3.02]	[2.97]
destinations	FAMEX*No in-house export unit	0.138***	0.158***	0.132***	0.096**	0.083	0.125*
		[3.95]	[4.00]	[2.93]	[2.01]	[1.41]	[1.85]
	R-squared	*0.15*	*0.20*	*0.21*	*0.24*	*0.29*	*0.31*
Outcome	Objective						
	FAMEX*Has in-house export unit	0.134***	0.200***	0.192***	0.132**	0.211***	0.288***
Nb.		[3.42]	[4.11]	[3.71]	[2.08]	[2.58]	[3.20]
products	FAMEX*No in-house export unit	0.159***	0.150***	0.163***	0.103	0.097	0.141*
		[3.47]	[2.86]	[2.89]	[1.63]	[1.31]	[1.75]
	R-squared	*0.15*	*0.20*	*0.23*	*0.26*	*0.27*	*0.30*
	Observations	12,263	12,124	9,664	7,238	4,839	2,524

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The sample includes treated and control firms in the common support. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, lagged exports, lagged number of destinations served, and lagged number of exported products.

Appendix E: Economic Magnitude of FAMEX Effects

Table E.1 Rates of Return on the FAMEX Program

		Baseline (BL)	TY	TY+1	TY+2	TY+3	TY+4	TY+5
						Non-significant coefficients		
			1	2	3	4	5	6
A	β Coefficient		0.511	0.723	0.571	*0.272*	*0.043*	*0.200*
B = exp(A)-1	Change in total export growth (treatment effect)		0.667	1.061	0.770	*0.313*	*0.044*	*0.221*
C	Cumulative total export growth, control a/		0.084	0.174	0.272	*0.378*	*0.493*	*0.618*
*D = C * (1+B)*	Predicted cumulative total export growth, treated		0.139	0.358	0.481	*0.496*	*0.515*	*0.755*
*E = BL * (1+C)*	Total exports, control b/	2,308	2,501	2,710	2,936	*3,181*	*3,447*	*3,734*
*F = BL * (1+D)*	Total exports, treated c/	2,308	2,629	3,135	3,419	*3,454*	*3,497*	*4,050*
G = F - E	Difference in total exports due to FAMEX		129	426	483	*273*	*50*	*316*
H	Average FAMEX grant per treated		21.7	21.7	21.7	*21.7*	*21.7*	*21.7*
I = G/H	Return on public investment d/		5.9	19.6	22.3	*12.6*	*2.3*	*14.6*

Notes:
a/ Obtained directly from the sample as the average annual export growth over the sample period for control firms;
b/ Expressed in thousand TND;
c/ Given the matching procedure, pre-treatment average total exports of treated firms are assumed to be similar to those of control firms;
d/ In TND of additional exports per firm per dinar of publicly-funded FAMEX grant.

Figure E.1 Evolution of Total Exports for Typical FAMEX Beneficiary and Control Firm