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Are the Benefits of Export Support Durable? Evidence from Tunisia

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

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October 21, 2013

Abstract

This paper evaluates the effects of the FAMEX export promotion program in Tunisia on the performance of beneficiary firms. While most studies assess only the short-term impact of such programs, we consider also the longer-term impact. Estimates suggest that beneficiaries initially saw both faster export growth and greater diversification across destinations and products. However, three years after the intervention, beneficiaries' growth rates and export levels were not significantly different from those of a control group even though they remained more diversified. We confirm that this divergence between export growth and diversification is not due to small export transactions to new markets creating an illusion of diversification; to greater exposure of beneficiary firms to crisis-affected economies leading to stunted export growth; or to spillover benefits for non-beneficiary firms resulting in their catching-up in export sales. We find some evidence that the divergence may be related to constraints within the firm, such as limited experience and in-house export capacity; to external constraints, such as access to finance; and to the design and implementation of the FAMEX program which placed greater emphasis on diversification than on export growth.

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1. Introduction

Since trade liberalization *per se* has not always led to improved 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 being devoted to export-processing zones, exporter assistance programs, and projects aimed at modernizing border management and customs procedures without much evidence of their impact. This paper contributes to the nascent literature on evaluation of export-assistance programs by assessing the firm-level effects of a recent export promotion program in Tunisia. Whereas existing evaluations typically focus on the contemporaneous or short-term effect of interventions, we also assess their longer-term impact.[1] Furthermore, while evaluations usually only assess whether a program works, we consider also alternative explanations for the observed results.

The literature assessing the effectiveness of export promotion has developed along two strands. The older one relies on cross-country evidence and examines effects on aggregate export performance. Thus, Rose (2007) used a gravity equation to show that diplomatic representations had a positive effect on bilateral trade flows. Lederman, Olarreaga, and Payton (2010) show that export promotion activities, after a long history of failure, in particular in developing countries where they coexisted with import substitution policies and currency overvaluation, have recently had more success in increasing aggregate exports, particularly when the private sector was involved in the management of promotion activities.[2]

A more recent strand of the literature has assessed export promotion using quasi-experimental methods, comparing the export performance of treated firms with that of a control group. Since enrollment into export promotion programs is not random, most papers control for selection 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 in affecting the performance of established exporters than in encouraging non-exporting firms to start exporting (Bernard and Jensen, 2004; Görg, Henry and Strobl, 2008; Girma, Gong, Görg and Yu, 2009). This is consistent with the literature on heterogeneous firms and trade, which suggests that exporters and non-exporters differ in terms of productivity and a host of other firm characteristics (see, e.g. Bernard, Jensen, Redding and Schott, 2007) that export promotion may not be able to offset. The second broad finding is that for established exporters, the impact is stronger along the extensive

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

[2] The authors regress country-level exports per capita on the budgets of export-promotion agencies (also per capita) and a host of country-level control variables. The agencies' budgets are instrumented with the agencies' age and interacted, inter alia, with management modes, one of which is private sector involvement.

margin than along the intensive one (Alvarez and Crespi, 2000, Volpe and Carballo, 2008),[3] suggesting that 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.[4] These papers break new ground in terms of rigorous evaluation of trade interventions, but focus primarily on the short-term effects of interventions. To our knowledge, the only paper that looks explicitly at the lingering effects of export promotion is van Biesebroeck, Yu and Chen (2010) who find that Canadian firms who received assistance from Canada's Trade Commissioner Services any time in the past exported significantly more than the control group.

This paper examines both the short-term and the longer-term impact of Tunisia's export promotion program, FAMEX, which provides matching grants 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. The inclusion in the merged data set of customs data on exports eliminates the risk of recall bias in outcome variables, which tends to arise when public programs are evaluated ex-post using surveys.[5]

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, referred to henceforth as weighted least squares (WLS) regressions. Our rich dataset allows us to extend the analysis in several directions, including the sustainability of the program's effect. We find that, compared to a control group, FAMEX beneficiaries successfully diversify in terms of export destinations and products, and durably so. However, unlike in van Biesebroeck et al. (2010), the beneficiary firms' total exports diverge only temporarily from the control group's total exports. One year after treatment, the differential in growth rates of total exports is not significant anymore, and three years after treatment, even export *levels* are no longer significantly different from those of the control group.

We confirm that this divergence between growth and diversification is not due to small export transactions to new markets creating an illusion of diversification; to greater exposure of beneficiary firms to crisis-affected economies leading to stunted export growth; or to spillover benefits to, and hence catch-up in export sales by, non-beneficiary firms. We find some

[3] Girma, Gong, Görg and Yu (2009) find a positive impact along the intensive margin but they consider the special case of production subsidies.

[4] 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.

[5] 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).

evidence that the divergence may be related to within-firm constraints, such as limited experience and in-house export capacity; to external constraints, such as access to finance; and to the design of the FAMEX program which placed greater emphasis on diversification than on export growth.

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 examines alternative explanations for our main results, and estimates the economic magnitude of the FAMEX effects. Section 7 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).[6] Our analysis focuses on FAMEX, a major demand-driven program 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 their export business plans. In terms of firm size, the minimum annual turnover required for FAMEX eligibility was 200,000 Tunisian Dinars (144,000 USD) in manufacturing and 100,000 Tunisian Dinars (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.

[6] 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 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.

[7] The FAMEX program also helped to build the institutional capacity of local professional organizations (export associations, chambers of commerce, and professional consulting organizations) and to strengthen the export consulting sector in Tunisia. Another component of the second phase of the EDP project focused on trade facilitation, including investments and technical assistance to modernize customs procedures, through a combination of investments in hardware and software and procedural improvements. These components—if effective—are likely to have benefited Tunisian firms broadly and thus do not necessarily contaminate the identification of FAMEX effects. Nevertheless, we will control in some robustness specifications for sector-year fixed effects which should absorb the effects of those components.

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

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 also 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 help the firm improve its plan.[10] 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 100,000 Tunisian Dinars (71,000 USD).

FAMEX received 1,710 applications and accepted 1,231 from 1,060 firms between 2005 and 2009.[11] In terms of the primary objective to request FAMEX assistance, 31 percent of the beneficiaries had little or no export experience while 69 percent were already exporters and wanted to diversify either by expanding into new destination markets (49 percent) or into new products (20 percent). 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.[12]

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

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

[10] The FAMEX management team did not base selection decisions on an explicit scoring framework (for example, by weighting selection criteria into a score for each FAMEX application). Instead, they used their "best judgment" for each application.

[11] 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-applying to FAMEX.

[12] 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, the rest being scattered over minor items.

grants 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.[13] These features of FAMEX make it a particularly attractive program to evaluate.[14]

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 (e.g., through the purchase of data or market studies), firm missions abroad to visit trade fairs and foreign exhibitions, and visits of prospective buyers.*	2.665	23.9%	313
Promotion	*Production of information and marketing including design, production and publication of ads in various media (e.g., newspapers, magazines, TV, radio, web, brochures), sending of mailings and samples, and firm representation (stands) in trade fairs and exhibitions.*	4.113	36.9%	319
Product development	*Product design modifications and production of samples, package design and modifications, and trademark registration.*	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 Tunisian Dinars per USD). The figures in the table concern the 455 FAMEX beneficiaries for which data was requested to FAMEX's management team (see Section 3).

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.

[13] 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, as in the case 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 specific 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.

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

First, we obtained from FAMEX's management team a complete list of the 1,060 beneficiary firms indexed by their tax ID. After dropping 126 firms that dropped out of the program, 163 firms in the services sector for which customs transaction data is not available, and 316 firms whose first export development plan was still ongoing at the end of 2009 (the penultimate 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 lasted for one year), firm location, sector, employment and total sales when it joined the program and when it left, 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. We do not have information on firms whose applications were rejected, as the FAMEX management team did not keep track of them.

Second, we obtained 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 (measured by firm employment), prior exporting status, location (Tunis versus non-Tunis) and manufacturing sub-sector, 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.[15] 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, 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 for 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).[16] We aggregated monthly data to annual export totals for each firm and year.

[15] 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 50,000 Tunisian Dinars, 50,000-1 million Tunisian Dinars, 1 million-2 million Tunisian Dinars, 2 million-5 million Tunisian Dinars.
[16] When necessary the data was converted to HS 6-digit by keeping the classification's first six digits.

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.[17] Of those, 401 benefitted from FAMEX and 2,346 did not.[18] Among the 2,346 non-beneficiaries we include 71 firms that applied to FAMEX but were turned down, and 126 firms that had their application to the FAMEX program approved but dropped out. FAMEX dropouts either did not take the grant because they decided not to implement their export business plan and communicated this to FAMEX management, or used less than 2 percent of their grant money, in which case FAMEX management dropped them from the program. Could they be a good control group? On the one hand, if they dropped out because of negative shocks, using them as a control group would lead to an over-estimation of the effect of FAMEX. On the other hand, if they dropped out for idiosyncratic reasons, they could have constituted an ideal control group but there are too few of them. In order to use the information they provide while preserving sample size, we include them as part of the control group in the baseline specifications. In robustness specifications, we either drop them from the sample or include them in the treatment group, with little effect on the results in either case.

Our combined dataset has two positive features. First, the inclusion of customs transaction data on exports 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 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.[19] Note that in the combined dataset, firm-level characteristics other than those related to export transactions are time-invariant, being available only for 2007.

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

[17] 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. While 2004-2010 will be our main sample period we will consider the entire 2000-2010 period in some specifications in Section 6.

[18] The reduction in the number of FAMEX beneficiaries from 455 potential firms to 401 firms in our final sample is due to missing data on some of the key variables for our analysis.

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

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 (not reported).

Table 2. Summary Statistics

Panel A. Distribution by Sector

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

Panel 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

Panel 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.

Table 3 shows export trends between 2003 and 2010 for FAMEX and control firms, as well as for Tunisian manufacturing exports. We present two indicators: growth in total exports of FAMEX firms as a group compared with control firms as a group; and average growth in firm-level exports of each group. In the crucial year 2003-2004 preceding the introduction of the FAMEX program, the two indicators present different pictures: one suggests that FAMEX firms were growing faster while the other suggests that control firms were growing faster. Thus, there is no clear presumption of either positive or negative selection. Both indicators agree that FAMEX firms grew faster in the first year when the program was introduced, 2004-2005, but the indicators diverge again for the year 2005-2006. They do converge on the view that FAMEX firms grew more slowly in subsequent years. Also worth noting is that our overall sample, including FAMEX and control firms, accounts for a substantial share of total Tunisian exports.[20] Focusing on averages calculated across the entire sample period, the number of destinations per

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

FAMEX firm was 3.8 while that per control firm was 1.8 and the number of products per FAMEX firm was 5.4 while that per control firm was 3.4.

Table 3. Growth in Tunisia's Exports

	2003-2004	2004-2005	2005-2006	2006-2007	2007-2008	2008-2009	2009-2010
Growth in total exports of:							
FAMEX firms as a group	16%	27%	3%	12%	-6%	-12%	2%
Control firms as a group	24%	6%	7%	18%	3%	-16%	4%
Tunisia	21%	8%	13%	25%	21%	-21%	8%
Average firm-level growth in total exports for:							
FAMEX firms	64%	71%	31%	23%	-23%	-62%	-35%
Control firms	18%	39%	13%	30%	-24%	-23%	-57%
Share of exports by FAMEX and control firms in Tunisia total exports	59%	60%	61%	57%	50%	49%	55%

Source: Authors' calculations using the combined dataset and data from COMTRADE.
Notes: The row 'FAMEX firms as a group' shows the growth in exports for FAMEX firms as a whole while the row 'Control firms as a group' shows the growth in exports for control firms as a whole, both based on the combined dataset. The row 'Tunisia' 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 from other firms in characteristics that affect both participation decisions and outcomes. This classical problem of non-experimental impact evaluation methods - the fundamental problem of causal inference defined by Holland (1986) - requires estimation approaches that control for selection bias.

Our starting point is the difference-in-differences estimator with propensity-score matching (PSM-DID) proposed by Heckman, Ichimura, and Todd (1997) which has been widely used in program evaluation, in particular for export promotion (Görg, Henry and Strobl, 2008; Volpe and Carballo, 2008), although our preferred estimator will be PSM-based weighted least squares, for reasons explained below. The PSM-DID method controls for selection bias by comparing changes in an outcome for program beneficiaries and for 'observationally similar' control firms.[21] It is based on the twin assumptions that (i) assignment to treatment (or the decision to

[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.

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 (we will use total exports, the number of destinations, and the number of products as alternative outcome variables), *t(i)* the year in which firm *i* received the one-year FAMEX program, and D_{it} a binary treatment indicator defined as:

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

Throughout, we will transform our outcome variables by taking log-differences:

$$\Delta \ln(y_{it}) = \ln(y_{it}) - \ln(y_{i,t-1}). \tag{2}$$

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:

$$Y^{PSM-DID} = \sum_{i \in T \cap S} \left[\Delta \ln(y_{i\tau}) - \sum_{j \in C \cap S} w_{ij} \Delta \ln(y_{j\tau}) \right] \tag{3}$$

where *j* designates control firms and the weights w_{ij} are determined by a propensity-score matching algorithm. A PSM algorithm matches each treated firm with the set of control firms that are 'most similar', that is, those with the closest propensity score. The latter is the estimated probability obtained from a regression of the probability of receiving the FAMEX treatment in

[22] The common support is the range of estimated propensity scores (i.e., the estimated probabilities of receiving treatment as defined below) for which the frequency of both treated and control firms is non-zero.

any sample year on a set of firm covariates likely to be associated with selection into the program.[23]

One concern with the treatment indicator D_{it} 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. The treatment indicator D_{it} 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 is permanent. In order to get around this, we follow standard practice and recode D_{it} for treated firms as equal to a missing value for all $t > t(i)$ and all $t < t(i) - 1$. That is, letting a period (".") stand for a missing value:

$$\tilde{D}_{it} = \begin{cases} 1 & \text{if} \quad i \in T \cap S \text{ and } t = t(i) \\ . & \text{if} \quad i \in T \cap S \text{ and } t \neq t(i) \\ 0 & \text{if} \quad i \in C \cap S \end{cases} \tag{4}$$

The recoded treatment indicator $\tilde{D}_{it}$ is the dependent variable in the propensity score regression and will be the key regressor in the treatment-effect equations. While we present estimates from the PSM-DID estimator in Section 5, a complication arises in our setup because the treatment year is not the same for all firms: some firms received FAMEX in 2005 and others in each subsequent year up to 2009. Designating as before by $t(i)$ firm i's treatment year, the before-after difference in outcomes is thus:

$$\Delta \ln\left(y_{i,t(i)}\right) = \ln\left(y_{i,t(i)}\right) - \ln\left(y_{i,t(i)-1}\right) \tag{5}$$

instead of Eq.(2). 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

[23] Depending on the algorithm, there can be for each treated firm either one matched control firm - the case of nearest-neighbor matching that we do not use here - or several control firms with a weighting scheme as in Eq. (3). We consider kernel matching which uses a weighted average of *all* control-group firms in the common support as a match for *each* treated firm *i*, with weights being higher for control firms with a propensity score that is closer to the propensity-score of the treated firm. See Caliendo and Kopeinig (2008) for additional details on matching.

estimator uses the estimated propensity scores to construct weights for the observations used for 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 regression-weighing scheme is given by:

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

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 with a higher propensity score (the more "treatable" firms among the untreated). The advantage of a regression framework is its flexibility in including covariates and controls, in particular year fixed effects which matter for us given that our sample period includes large macroeconomic swings.

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

$$\Delta \ln(y_{it}) = \alpha + \beta \tilde{D}_{it} + \mathbf{X}_{it}\boldsymbol{\gamma} + \delta_t + u_{it} \tag{6}$$

where δ_t controls for calendar year effects (macroeconomic cycles), and $\mathbf{X}_{it}$ is a vector of firm covariates including age and its square, 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 destinations served, lagged number of export products, and location and sector fixed effects.[25]

A limitation of the regression in Eq. (6) is that it restricts the measured treatment effect to its short-term impact on $\ln(y_{i,t(i)}) - \ln(y_{i,t(i)-1})$. In order to measure the FAMEX treatment effect's persistence on outcome growth and levels, we recode either the treatment or the outcome

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

[25] As Bernard, Redding, and Schott (2010) show, more productive firms tend to export more products to more destinations, therefore controlling for the lagged number of products and destinations is a way of controlling indirectly for TFP, for which we have no data. On this, see the discussion in Footnote 24 of Volpe and Carballo (2008). The specification in Eq. (6) implies that firm covariates in *levels* explain outcome *growth rates*, as the outcome variable is log-differenced. First-differencing time-variant firm characteristics and dropping time-invariant ones (i.e. treating Eq. (6) as a full first-difference equation) returns similar results.

variable as follows. First, in order to test for the persistence of treatment effects on outcome *growth*, we lag the treatment variable by k years, with $k = 0,\ldots,5$, rewriting Eq. (6) as:

$$\Delta \ln\left(y_{it}\right) = \alpha + \beta \tilde{D}_{i,t-k} + \mathbf{X}_{it}\boldsymbol{\gamma} + \delta_t + u_{it}. \tag{7}$$

This can be thought of equivalently as "forwarding" the left-hand-side variable by k periods. We will use this to explore whether a firm treated in, say, 2005 experienced higher outcome growth than control firms between 2005 and 2006 (k = 1), between 2006 and 2007 (k = 2), and so on. The regression in Eq. (7) will be estimated separately for each k.[26]

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 for each of the subsequent k years, with $k = 0,\ldots,5$,

$$\Delta_k \ln\left(y_{it}\right) = \ln\left(y_{it}\right) - \ln\left(y_{i,t-k}\right) \tag{8}$$

and estimate the modified regression:

$$\Delta_k \ln\left(y_{it}\right) = \alpha + \beta \tilde{D}_{it} + \mathbf{X}_{it}\boldsymbol{\gamma} + \delta_t + u_{it} \tag{9}$$

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, (always relative to 2004, its pre-treatment year). The regression in Eq. (9) will also be estimated separately for each k. [27]

For both Eqs. (7) and (9), sample size is reduced as k rises, since for firms treated in 2009 lagged treatment or long differences in the outcome variables are not defined; for firms treated in 2008 only k = 1 lagged treatment or long differences are defined, and so on. Only firms treated in 2005 have a full set of lagged treatments and long differences defined. So as k rises the treatment group shrinks and for k = 5 lagged treatments and long differences are defined only for 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

[26] The estimating sample for each regression includes all available one-year differences for control firms, but for treated firms the one-year differences included differ across regressions. For k=1, the regression sample includes the one-year difference ($t(i)$+1) - $t(i)$ for treated firm i, for k=2, the regression sample includes the one-year difference ($t(i)$+2) - ($t(i)$+1) for treated firm i , and so on.

[27] For each k, the estimating sample includes all available long differences of k length for control firms but for treated firms the long differences included differ across regressions. For k=1, the regression sample includes the long difference ($t(i)$+1) - ($t(i)$-1) for treated firm i, for k=2, the regression sample includes the long difference ($t(i)$+2) - ($t(i)$-1) for treated firm i , and so 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 5.2.

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), as discussed in Section 5.2.

Before turning to estimation results, a caveat is necessary. 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. In a non-experimental study, selection bias on time-varying unobservables (say, management changes) can never by fully ruled out, although controlling for the lagged number of products and destinations goes some way toward controlling for unobserved management changes.

5. Main Findings: Impact and Persistence of FAMEX Effects

This section presents our main findings on the effects of FAMEX on Tunisian firms' export outcomes, based on both PSM-DID and WLS regressions. We also discuss a series of checks to establish the robustness of the main findings.

5.1. Treatment Effects

As a key ingredient to the PSM-DID and WLS regressions, we obtain propensity scores from a 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.[28] The probability of treatment correlates positively with location in Tunis and the lagged number of export products and destinations but correlates negatively with total exports and 100-percent exporter status (see Appendix Table A.1). Sector dummies are all insignificant, suggesting no sectoral targeting. The propensity-scores distributions for the treatment and control groups have a large common support, which includes 401 FAMEX beneficiaries and 2,346 control firms (see Appendix Figure B.1).[29] Following Rosenbaum and Rubin (1983), Dehejia and Wahba (2002)

[28] The probit regression for propensity score matching includes sector fixed effects but it is not possible to do matching sector-by-sector because of the small size of sector sub-samples.

[29] The propensity scores distributions are also fairly similar in shape, although not completely overlapping, highlighting the importance of matching. The importance of a large common support and similarity in the

and Smith and Todd (2005) we verify that all balancing tests to assess whether matching corrects for significant differences in the distribution of pre-treatment covariates between the treatment and control groups are satisfied (see Appendix A).

Table 4 shows PSM-DID estimates using kernel matching 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 year-to-year log-differences, and pooling together firms treated in different years. 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).[30] 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 effects on outcome growth using Eq. (7) addressing the question of how long growth trajectories of FAMEX firms and control firms diverge.

Table 4. 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	0.496**	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 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)-(1b) are similar and significant at the one percent level for all three outcome variables. Considering total exports, the estimate in column (1b)

distribution of covariates/propensity scores across treated and control groups for unbiased PSM-DID estimators is discussed by Heckman, Ishimura and Todd (1997).

[30] As an alternative to kernel matching we also use five-nearest neighbors matching with caliper 0.1 and the results are maintained. Moreover, qualitatively similar findings are obtained when using bootstrapped standard errors.

indicates that growth in total exports (measured by the log-difference) is 66.7 percent higher for FAMEX beneficiaries than for the control group.[31] Short-term treatment effects 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 export growth, treatment effects become insignificant as early as the year after treatment. By contrast, for destination counts and product counts growth rates remain significantly different between the two groups of firms for more years.

Table 5 shows cumulative treatment effects estimated from Eq. (9), using long differences in log-outcomes (as defined in Eq. (8)). Thus, the question addressed by this table is for how long outcome levels of FAMEX firms relative to control firms remain different after a temporary growth surge. Column (1) reproduces column (1b) of Table 4. Columns (2)-(6), which rely on increasingly longer time-differences, show again different patterns depending on the outcome. For total exports, columns (4)-(6) show that the FAMEX positive cumulative effects on levels disappear three years after treatment. For destination and product counts cumulative effects persist, which is to be expected if growth rates keep on diverging as was shown in Table 4.

Table 5. 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	0.511***	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 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.

Additionally we show in Table 6 that the FAMEX treatment-induced persistent growth at the extensive margin in terms of destinations and products observed in Tables 4 and 5 does translate into lower concentration at the product-destination level. We re-estimate Eq. (9) using as

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

outcome variables Herfindahl and Theil concentration indices calculated across product-destination cells at the firm level. The estimates suggest that concentration declines persistently - thus diversification increases persistently - for FAMEX firms relative to control ones.

Table 6. Cumulative Effects of FAMEX on Concentration Indices

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						
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

Notes: T-statistics based on robust standard errors in brackets;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes all treated firms and control firms in the common support. The WLS regressions include 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.

5.2 Robustness of Main Findings

In this section, we examine the robustness of our main findings which suggest a contrast between a large but transient treatment effect on firm total exports and moderate but persistent effects on firm destination and product counts.

5.2.1 Dropouts, FAMEX Cohorts, and Estimation Issues

The treatment effects shown so far are based on a sample which includes 126 FAMEX program dropouts in the control group. We re-estimate Eq. (9) using two alternative sample constructions: (i) eliminating dropouts from the sample or (ii) including them in the treatment group. The results are qualitatively maintained but the elimination of dropouts gives a slightly larger treatment effect, whereas the opposite is true when they are included in the treatment group (see Appendix Table B.1). That is, dropouts seem to have 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 the columns in Table 5 due to the fact that several lagged treatments and long 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 panel for control firms (as well as for treated firms). We re-estimate Eq.

(9) using a fixed sample across columns 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 are qualitatively maintained (see Appendix Table B.2).

Third, as an alternative to the HIR procedure, we address the problem of 'time-wise mismatch', which involves 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. (9) by OLS. The results are very similar to those in Table 5 (see Appendix Table B.3).

Finally, other reforms occurring simultaneously with the FAMEX program over our sample period may have boosted the export performance of control firms. The separate sector and year fixed effects included in our specifications may not account properly for such reforms. Thus we estimate a more stringent specification where Eq. (9) is modified to include sector-year fixed effects. The results are again very similar to those in Table 5 (see Appendix Table B.4).

5.2.2 Selection and Endogeneity Issues

In order to control for the possibility that FAMEX firms were, for reasons unrelated to the program's effect, on a different growth trajectory than others (i.e., the presence of non-parallel trends in pre-treatment outcomes), we turn to an alternative specification in which the first-stage probit regression controls for 2-year lagged outcome *growth*, not just lagged outcome levels.[33] This ensures that FAMEX firms are matched with control firms characterized by similar pre-treatment growth. Table 7 shows that when re-estimating Eq. (9) by WLS with weights based on

[32] We are grateful to Yasusuki Todo for sharing his matching routine. Todo (2011) uses this procedure to evaluate the impact of Japanese aid-funded technical assistance programs on Indonesian foundry firms. See also Arnold and Javorcik (2009) for a discussion of time-wise mismatch.

[33] To conduct this exercise we make use of additional customs data from 2000 to 2004 for treatment and control group firms. Similar results were obtained controlling for 1-year, 3-year or 4-year lagged outcome growth rates. The longer is the time-difference used in the calculation of the growth rates the smaller is the number of FAMEX firms in the sample, since a longer time-difference requires FAMEX firms to have been exporting consecutively in a larger number of years prior to receiving the grant.

the corresponding new propensity scores the effects of the FAMEX program are largely unaffected.[34]

Table 7. Cumulative Effects of FAMEX Controlling for Pre-Treatment Growth

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.
	Propensity score accounts for pre-treatment growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.610***	0.915***	0.772***	0.572**	0.417	0.559
	[3.20]	[3.91]	[3.17]	[1.99]	[1.14]	[1.37]
R-squared	*0.15*	*0.22*	*0.22*	*0.24*	*0.25*	*0.28*
Nb. destinations	0.165***	0.209***	0.205***	0.163***	0.176***	0.217***
	[6.20]	[7.05]	[6.18]	[4.59]	[3.90]	[4.18]
R-squared	*0.13*	*0.17*	*0.18*	*0.21*	*0.27*	*0.27*
Nb. products	0.163***	0.206***	0.197***	0.151***	0.187***	0.259***
	[4.86]	[5.14]	[4.71]	[3.21]	[3.17]	[4.14]
R-squared	*0.14*	*0.19*	*0.22*	*0.26*	*0.26*	*0.30*
Observations	11,646	11,525	9,112	6,745	4,406	2,263

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 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.

As another way to address the issue of unobserved heterogeneity, we consider specifications with firm fixed effects. Formally, since the outcome variable in Eq. (9) is a log-difference, firm fixed effects cannot be included (see footnote 27). Thus, we estimate an equation whose left-hand-side is an outcome variable in levels by WLS controlling for firm fixed effects. Table 8 shows again that the effects of the FAMEX program are on total exports are less durable than on the numbers of destinations and products.[35]

[34] As a further check, we run a standard placebo exercise in which random pseudo-treatment years are generated and attributed to each treated firm before its true treatment year (i.e., for a firm treated in 2005, random times were drawn between 2000 and 2004). Replicating the exercise a thousand times and retrieving each time the treatment effect coefficient and its standard error, we compute the t-statistic as the average estimate divided by the average standard error and find that the treatment effect is insignificant at the 10% level (results are available upon request).

[35] The dummies included in the regressions in Table 8 allow us to have a specification for outcomes in levels which is close to that for outcomes in long differences in Table 5. The FAMEX TY dummy is equal to 1 only for FAMEX firms in their year of treatment, equal to 0 for FAMEX firms prior to treatment, missing for FAMEX firms in years after treatment and equal to 0 for control firms in any year. The FAMEX TY+1 dummy is equal to 1 only for FAMEX firms one year after treatment, equal to 0 for FAMEX firms prior to treatment, missing for FAMEX firms in the year of treatment and two or more years after treatment and equal to 0 for control firms in any year. The FAMEX TY+2 dummy is equal to 1 only for FAMEX firms two years after treatment, equal to 0 for FAMEX firms prior to treatment, missing for FAMEX firms in the year of treatment, the year after treatment and three or more years after treatment and equal to 0 for control firms in any year. The other dummies are defined analogously.

Table 8. Cumulative Effects of FAMEX on Export Outcomes in Levels with Firm Fixed Effects

Dep. Variable	Total exports	Total exports	Total exports	Total exports	Total exports	Total exports	Nb. destinations	Nb. destinations	Nb. destinations	Nb. destinations	Nb. destinations	Nb. destinations	Nb. products	Nb. products	Nb. products	Nb. products	Nb. products	Nb. products
Estimator	WLS reg. (1)	WLS reg. (2)	WLS reg. (3)	WLS reg. (4)	WLS reg. (5)	WLS reg. (6)	WLS reg. (7)	WLS reg. (8)	WLS reg. (9)	WLS reg. (10)	WLS reg. (11)	WLS reg. (12)	WLS reg. (13)	WLS reg. (14)	WLS reg. (15)	WLS reg. (16)	WLS reg. (17)	WLS reg. (18)
FAMEX TY	0.723***						0.148***						0.158***					
	[3.66]						[5.54]						[4.92]					
FAMEX TY+1		1.012***						0.173***						0.197***				
		[3.16]						[4.23]						[3.87]				
FAMEX TY+2			0.433						0.122***						0.149***			
			[1.63]						[3.51]						[3.43]			
FAMEX TY+3				0.093						0.077**						0.071		
				[0.35]						[2.14]						[1.56]		
FAMEX TY+4					-0.042						-0.107						-0.074	
					[-0.06]						[-1.06]						[-0.70]	
FAMEX TY+5						-0.49						0.044						0.098*
						[-1.43]						[0.90]						[1.71]
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	*0.822*	*0.794*	*0.793*	*0.784*	*0.757*	*0.736*	*0.872*	*0.861*	*0.851*	*0.836*	*0.815*	*0.797*	*0.83*	*0.812*	*0.807*	*0.782*	*0.772*	*0.763*
Observations	17,356	17,356	17,354	17,156	16,844	16,778	17,356	17,356	17,354	17,156	16,844	16,778	17,356	17,356	17,354	17,156	16,844	16,778

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.

6. Why Does Diversification Persist but Export Growth Does Not?

Our main findings in Section 5 are that the FAMEX program led to durable diversification in terms of higher numbers of destinations and products but to much less durable differences in total exports of beneficiary firms compared to control firms. In this section, we explore possible reasons for this divergence.

6.1 Is Diversification an Optical Illusion?

If FAMEX beneficiaries start exporting tiny shipments for marketing purposes that are registered as positive export transactions and therefore are treated as new destinations or new products, one could observe diversification increasing significantly but no meaningful long-lasting effects on total exports. To address this possibility, we re-estimate Eq. (9) eliminating small export transactions below the equivalent to 1,000 USD (1,500 Tunisian dinars) from the sample.[36] Table 9 shows that the results are not affected. Hence, our findings on diversification are not an optical illusion caused by the presence of small transactions.

Table 9. Cumulative Effects of FAMEX – Dropping Small Transactions

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	0.423**	0.629***	0.475**	0.275	-0.089	0.103
	[2.37]	[2.96]	[2.04]	[1.00]	[-0.25]	[0.26]
R-squared	*0.15*	*0.21*	*0.21*	*0.22*	*0.23*	*0.25*
Nb. destinations	0.129***	0.168***	0.170***	0.153***	0.131***	0.173***
	[5.25]	[6.25]	[5.38]	[4.31]	[2.82]	[3.23]
R-squared	*0.13*	*0.17*	*0.18*	*0.21*	*0.25*	*0.27*
Nb. products	0.103***	0.113***	0.125***	0.084**	0.097*	0.172***
	[3.58]	[3.31]	[3.37]	[1.98]	[1.76]	[2.82]
R-squared	*0.13*	*0.17*	*0.20*	*0.23*	*0.24*	*0.28*
Observations	12,263	12,124	9,664	7,238	4,839	2,524

Notes: Robust t-statistics in parentheses. The sample includes treated and control firms in the common support. The WLS regressions include 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. Outcomes measures are computed dropping export transactions below the equivalent to 1,000 USD (1,500 Tunisian dinars).

Further confirmation that product diversification is indeed enhanced by FAMEX comes from re-estimating Eq. (9) using the number of HS 6-digit products exported instead of the number of 11-

[36] The numbers of observations in Table 9's various columns are similar to those in the corresponding columns of Table 5 because eliminating those small export transactions does not result in the elimination of any firm from the sample as no firm exports in total less than 1000 USD.

digit products (in Tunisia's nomenclature) exported. These results are similar to those in Table 5 (see Appendix Table C.1).

6.2 Were Beneficiaries Exposed to Crisis-Affected Markets?

For firms that benefitted from the FAMEX program in 2005, the vanishing effects of the program on total exports in columns (3)-(6) of Appendix Table B.2 and Table 5 are occurring during or after the 2007-2008 global financial crisis.[37] Thus, it is possible that the FAMEX firms exposed themselves more to stagnant destination markets that contracted most during the crisis and experienced a slow recovery thereafter. To address this possibility, we examine whether the cumulative treatment effects for FAMEX 2005 firms are robust to controlling for GDP growth in the destination markets. We focus on FAMEX 2005 firms that export continuously until 2010 and on control firms that export continuously from 2004 to 2010 and construct for each firm a measure of exposure to GDP growth in destination markets. This measure is obtained as a weighted average of GDP growth in the firm's destination markets, with weights given by the firm's initial share (as of 2005) of exports to each destination market**.**[38] We re-estimate Eq. (9) including such firm-level exposure to GDP growth measure as a control variable. The results are shown in Table 10 and indicate that the pattern of transient total export growth but persistent destination and product diversification is maintained.[39] So we can rule out the consequences of the global financial crisis for destination market growth as an explanation.[40]

[37] While in Appendix Table B.2 only firms treated in 2005 enter the estimating samples, in Table 5 firms treated in any year enter the estimating samples. Thus in columns (3)-(4) of Table 5 treated firms are those receiving FAMEX in 2005 or in 2006, for which TY+3 and TY+4 correspond, respectively, to 2008 and 2009 or to 2009 and 2010. In column (5) of Table 5 treated firms are those receiving FAMEX in 2005 for which TY+5 corresponds to 2010.

[38] Growth rates of GDP in destination markets are calculated over the various time periods in columns (2)-(6) of Table 5 relative to 2005, and they are used to construct the measures of exposure to GDP growth that enter the specifications in Table 10. For example, if the dependent variable is growth in an export outcome between 2004 and 2006 (shown in column (2) in Table 10), the measure of exposure to GDP growth included is based on GDP growth in destination markets between 2005 and 2006.

[39] The estimating sample in Table 10 is smaller than that in Appendix Table B.2 since it includes only FAMEX 2005 firms and control firms that export in every single year between 2004 and 2010 whereas in Appendix Table B.2, FAMEX 2005 and control firms are part of the estimating sample even if their exports are zero in a given year between 2004 and 2010. Moreover in Table 10, we drop a firm from the sample if any of its destination markets has missing data on GDP growth. Note that the results from estimating Eq. (9) for the smaller sample used in Table 10 are qualitatively similar to those in Appendix Table B.2 and in Table 5.

[40] We also estimated a different specification for the sample of FAMEX 2005 firms and control firms exporting continuously between 2000 and 2009 (788 observations) whose dependent variable was the difference across the 2000-2004 and the 2005-2009 periods in the firm-level measure of exposure to GDP growth in destination markets, with weights given by the firm's beginning of the period share of exports to each destination market. OLS estimates do not show a significant decrease in GDP growth in destination markets of FAMEX firms relative to control firms.

Table 10. Cumulative Effects of FAMEX controlling for Exposure to GDP Growth in Destination Markets

Difference	(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.
	Exposure to GDP growth in destin. markets included as control				
	(1)	(2)	(3)	(4)	(5)
Outcome					
Total exports	0.229**	0.144	0.103	0.234*	0.191
	[1.97]	[1.18]	[0.84]	[1.73]	[1.21]
R-squared	*0.30*	*0.27*	*0.39*	*0.36*	*0.30*
Nb. destinations	0.158***	0.174***	0.193***	0.244***	0.244***
	[4.02]	[4.13]	[4.19]	[5.58]	[4.81]
R-squared	*0.22*	*0.17*	*0.27*	*0.38*	*0.35*
Nb. products	0.133**	0.152***	0.175***	0.254***	0.281***
	[2.33]	[2.79]	[2.80]	[4.00]	[4.39]
R-squared	*0.23*	*0.24*	*0.27*	*0.32*	*0.35*
Observations	1,156	1,156	1,156	1,156	1,156

Notes: Robust t-statistics in parentheses. The sample in column (1) includes only treated firms in 2005 and control firms operating in export markets continuously from 2000 to 2009 (one observation per firm). The sample in columns (2)-(6) includes only treated firms in 2005 exporting continuously until 2010 and control firms exporting continuously from 2004 to 2010. The sample includes treated and control firms in the common support. The WLS regressions include 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. All regressions include the firm-level measure of exposure to GDP growth in destination markets defined in the text.

6.3 Did Control Firms Benefit from Spillovers?

The main finding —lack of persistence of treatment effects on total exports—might reflect catching up by control firms rather than vanishing benefits for treated firms, although catching up through imitation should apply also 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.[41] This is not just a technical issue. Non-appropriability of benefits from information production (e.g., marketing research) in the

[41] Cadot, Iacovone, Pierola, and Rauch (2013) 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.

presence of externalities can be seen as the market failure justifying the subsidized intervention by the Tunisian government and the World Bank.[42]

The difficulty in investigating this issue explicitly is that the measurement of spillovers is elusive, especially when the transmission channel is unknown. Following standard practice in the literature, our spillover proxy is defined as 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.[43] We then regress *control firm* export outcomes on exposure to FAMEX beneficiaries.[44] That is,

$$\Delta \ln\left(y_{jsrt}\right) = \alpha + \sum_k \beta_k n_{jsr,t-k} + \delta_j + \delta_{st} + \upsilon_{jsrt} \tag{10}$$

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 $t-k$. Firm fixed effects δ_j account for unobserved firm heterogeneity in growth of export outcomes. Eq. (10) 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 11 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. (10) with a sample including all firms, both FAMEX beneficiaries and control firms, and again find no evidence of externalities (see Appendix Table C.2). One might argue that spillovers to control firms are more

[42] 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." Credit rationing is another market failure that could justify intervention. However, if the government provided 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.

[43] Empirical studies 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 export spillovers while Koenig, Mayneris, and Poncet (2010) and Mayneris and Poncet (2010) use the numbers of exporters in the same region exporting similar products and/or to similar destinations. Krautheim (2012) develops a trade model with heterogeneous firms including a spillover effect from the number of exporters to the fixed costs of exporting.

[44] One weakness with this approach is that we are unable to measure spillovers on Tunisian firms other than the control firms in our sample because we were not allowed access to data on the universe of Tunisian firms. Another weakness is that we cannot explore spillovers on outcomes other than exports, again due to data limitations.

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. (10) 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.2). Thus, spillovers from beneficiary firms to control firms are not a reason for the lack of persistence in total export growth.

Table 11. 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.

6.4 Did Growth at the Extensive Margin Come at the Expense of the Intensive Margin?

So far we have looked at whether FAMEX encouraged growth at the extensive margins (in terms of numbers of destinations and products) and at the "overall" intensive margin measured by total exports. Next, we perform a decomposition of year-to-year export growth (used in Table 4) for each FAMEX and control firm across four categories: the change in exports of old products to old destinations, the change in exports of old products to new destinations, the change in exports of new products to old destinations, and the value of exports of new products to new destinations.[45] Then we re-estimate Eq. (7) using alternatively each of these components of export growth as dependent variables and show the results in Table 12. The idea behind this

[45] For this decomposition we rely on a measure of proportional export growth following Davis, Haltiwanger and Schuh (1996) – $(X_{it} - X_{it-1})/0.5*(X_{it}+X_{it-1})$ - which can be decomposed into the four categories and can account for entry and exit of destinations and products. Note that the export growth measure used in Table 4 is different as it is a log-difference. However, as seen by the first row in Table 12 the pattern of year-to-year export growth is similar for proportional and log-difference growth rates.

decomposition is to look for differential treatment effects on exports to destinations and products that were already in the firms' export portfolios when they joined the FAMEX program relative to new destinations and new products. The estimates suggest that the contribution of new products to export growth is no different for FAMEX firms relative to control firms, but there is a significant difference concerning new destinations whose contribution to year-to-year growth in the first three years is substantially higher for FAMEX firms relative to control firms and these gains persist in cumulative terms over the whole period. There are several instances of export declines for FAMEX firms relative to control firms in terms of old products exported to old destinations, new products exported to new destinations, and new products exported to old destinations. The fact that none of them are significant may have to do with the fact that while the expansion is on a single dimension, the contractions are spread across three dimensions. The finding of eventually stagnant total exports but persistently larger exports to new destinations for FAMEX firms relative to control firms, along with the signs of relative contraction by the FAMEX firms in old destinations and old products (as well as new products), provide weak evidence that the gains on the new destination front are coming at the expense of old destinations, old products and poor results from the introduction of new products.

Table 12. Year-to-Year Effects of FAMEX on Components of Export Growth

Difference	TY-(TY-1)	(TY+1)-TY	(TY+2)-(TY+1)	(TY+3)-(TY+2)	(TY+4)-(TY+3)	(TY+5)-(TY+4)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Exports proportional growth rate	0.132***	0.055	-0.019	-0.061	-0.026	-0.017
	[2.77]	[1.15]	[-0.40]	[-1.25]	[-0.46]	[-0.31]
R-squared	*0.109*	*0.098*	*0.088*	*0.075*	*0.114*	*0.099*
Old destinations old products	0.065*	0.034	-0.017	-0.051	-0.018	-0.032
	[1.78]	[0.91]	[-0.46]	[-1.26]	[-0.40]	[-0.72]
R-squared	*0.073*	*0.059*	*0.075*	*0.079*	*0.113*	*0.113*
Old destinations new products	0.003	-0.007	-0.003	-0.009	0.002	0.033**
	[0.21]	[-0.72]	[-0.31]	[-0.92]	[0.14]	[2.17]
R-squared	*0.032*	*0.033*	*0.032*	*0.035*	*0.048*	*0.072*
New destinations old products	0.033**	0.035***	0.024**	0.019	0.016	-0.004
	[2.53]	[2.61]	[1.96]	[1.42]	[1.27]	[-0.30]
R-squared	*0.06*	*0.036*	*0.053*	*0.055*	*0.049*	*0.061*
New destinations new products	0.031	0.005	-0.019	-0.013	-0.017	-0.017
	[1.20]	[0.24]	[-0.94]	[-0.61]	[-0.65]	[-0.69]
R-squared	*0.197*	*0.2*	*0.153*	*0.11*	*0.126*	*0.121*
Observations	12,263	12,124	9,664	7,238	4,839	2,524

Notes: Robust t-statistics in parentheses. The sample includes treated and control firms in the common support. The WLS regressions include 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.

As a complementary exercise we explore whether new destinations and new products added by FAMEX firms to their portfolios survive better than new destinations and products added in the same years by control firms. Our estimates from WLS Tobit regressions shown in Appendix Table C.3 provide evidence that this is indeed the case for new destinations but not for new products.

6.5 Do Firm-Specific Constraints Explain Limited Export Growth?

The evidence that FAMEX firms were unable to expand persistently along both the intensive and extensive margins suggests that some (or all) firms may have faced internal or external constraints. For example, a recent paper by Akcigit and Peters (2013) shows that better managers are necessary for firms to scale up easily and to expand into new product lines. We do not have information on managerial quality in Tunisian firms, but we do have information on whether FAMEX firms had an in-house export unit prior to the start of the program (200 firms had such a unit). Such firms may exhibit stronger performance because they are better equipped to make better use of the FAMEX assistance. To examine this possibility, we allow the treatment effect to differ according to whether the firm had an in-house export unit. The results suggest that FAMEX beneficiaries with a dedicated in-house export unit exhibit stronger growth in total exports and in the number of destinations served than other FAMEX beneficiaries, relative to the control group (see Appendix Table C.4). In particular, FAMEX beneficiaries with an in-house export unit exhibit more durable growth in total exports than firms without such a unit.

Another attempt to examine the role of internal constraints is to consider firm age as a proxy for experience and see whether it affects the impact of FAMEX assistance. We do so by classifying FAMEX firms into two age categories determined by the median age in the sample (16 years of age) and re-estimating Eq. (9) allowing the FAMEX effect to differ across age categories. The estimates suggest stark differences across firms depending on their age.[46] FAMEX has no effect on export growth for younger firms, relative to control firms, while it has a strong effect for export growth of older firms up to three years after treatment (see Appendix Table C.4). FAMEX does have positive and significant effects on growth in the number of products and destinations for younger firms but these are less durable than the effects for older firms.[47]

[46] Note that age is included as a control in the WLS regressions and it is also included in the propensity score regression. Thus any effects that age may have had on selection into FAMEX are accounted for.

[47] We also examine whether firm size affects the magnitude of the impact of FAMEX assistance, classifying FAMEX firms into two those below 50 workers and those above 50 workers and re-estimating Eq. (9) allowing the FAMEX effect to differ across size categories. However, the unreported results do not point to any significant differences across firm size categories.

Credit constraints are an obvious example of external limits on firms' ability to expand on multiple fronts. There is considerable evidence that firms in Tunisia, as in most developing countries, are credit constrained for reasons related to the uncertain value of collateral, the difficulty of recovering bad debts, and weak accounting and reporting standards. It seems likely that these constraints are felt more strongly by firms operating in sectors that rely more on external finance. We re-estimate Eq. (9) allowing the FAMEX treatment effects to be interacted with a measure of financial dependence calculated following Rajan and Zingales (1998) (see Appendix Table C.5).[48] Interestingly, for total exports, the interaction terms kick in when direct terms are no longer significant—i.e., starting two years after treatment, and they grow over time. Thus, financially-dependent firms show weaker initial and stronger longer-term treatment effects than the treatment group's average, suggesting that the FAMEX matching grant made a positive difference for these firms with a lag. It might be that even though FAMEX grants were small, they had a positive signaling effect on access to bank finance which took time to be felt and observed. In the absence of firm-level data on access to finance in Tunisia, these findings must remain speculative.

6.6 Did FAMEX Design Favor Diversification over Export Growth?

Another potential explanation for the strong and persistent effect of FAMEX on diversification and its more transient effect on total exports relates to the design and implementation of the FAMEX program itself. As shown in Table 1, over 60 percent of FAMEX grants were used to co-finance the cost of activities related to market prospection and to promotion. 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 and re-estimate Eq. (9) (see Appendix Table C.6).[49] Market prospection activities and promotion activities have a significant effect on all firm-level export outcome variables up to

[48] The measure of financial dependence proposed by Rajan and Zingales (1998) is an industry-level variable calculated for 27 3-digit ISIC industries and nine 4-digit ISIC industries using Compustat firm-level data for the US. Let k be capital expenditure and c be operational cash flow at the firm level. Rajan and Zingales' index for industry j, r_j, is the median value of $(k-c)/k$ across all Compustat firms in industry j. We construct our r_i variable at the firm level by using concordance tables between ISIC3 and HS6 classifications, assigning to each HS exported product the Rajan-Zingales index of the ISIC code to which that product belongs, and then taking a weighted average at the firm level using product weights in the firm's export portfolio in the firm's initial year in the sample.

[49] 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.

four years after treatment, with both activities exhibiting similar marginal returns on the dinar. Firm development has a significant positive effect on all export outcomes but only four or five years after treatment, which may be due to the longer gestation period needed for such activities to bring export benefits. By contrast, the other two types of activities, assistance for product development and the creation of foreign subsidiaries, have insignificant returns. These results suggest that FAMEX may have been better equipped to help firms reduce informational barriers in foreign markets than to enhance export capacity.

Additionally, in Section 2 we noted that among the 401 FAMEX beneficiaries, the majority were already exporters and their main objective in requesting the assistance was to diversify either by expanding into new destination markets (194 beneficiaries) or into new products (112 beneficiaries), and only 95 beneficiaries came to FAMEX to become a more substantive exporter (in terms of the exports to sales ratio).[50] Exploiting this information, we re-estimate Eq. (9) allowing the treatment effect to differ across the objectives of the individual projects supported by FAMEX (see Appendix Table C.7). Firms that approached FAMEX with the objective of expanding into either new destinations or new products, saw a significant and sustained expansion in terms of the both the number of destinations and products, and a significant but temporary expansion in total exports. Firms which had the objective of expanding total exports saw less significant benefits along all dimensions. Taken in conjunction with our other findings, we suggest that there is some evidence that FAMEX, either by design or in implementation, helped firms break into new markets and new products more durably than to expand total exports.

6.7 A Tentative Cost-Benefit Assessment

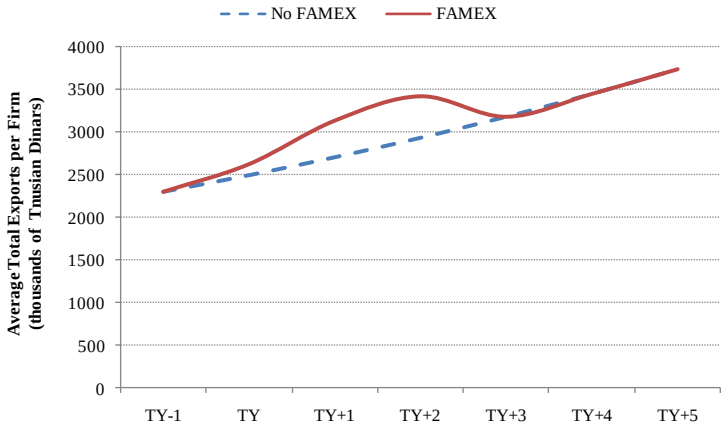
Our baseline results suggest that FAMEX had a large and positive—albeit transient—effect on total exports of treated firms. We turn in this section to a tentative cost-benefit calculation to estimate the rate of return of the FAMEX program *per firm*, laying out clearly at each step the assumptions made.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

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. Since the average annual total export growth for control firms in the 2004-2008 period was 8.35 percent,

[50] The three groups are mutually exclusive given the way the FAMEX application packages were structured. When applying for assistance firms could state only one of the three objectives (i) become a significant exporter, (ii) export to a new destination market, or (iii) export a new product.

the estimated annual growth in total exports for a FAMEX beneficiary in the year of treatment is 13.9 percent.[51] Given average total exports per firm in 2004 (prior to FAMEX) of 2,308 thousand Tunisian Dinars, the growth rates above imply that, in the year of treatment, exports for a typical FAMEX beneficiary would have increased to 2,629 thousand Tunisian Dinars against only 2,501 thousand Tunisian Dinars for a control firm, a gain of 129 thousand Tunisian Dinars. After two years, the difference would have peaked at 483 thousand Tunisian Dinars, and after three years it would become insignificant. The implied trajectory of exports for a typical FAMEX beneficiary and a typical control firm are illustrated in Figure 2.

Figure 2. Evolution of Total Exports for Typical FAMEX Beneficiary and Control Firm



Note: The figure is based on the numbers presented in Appendix Table D.1.

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

[51] We calculate the average annual total export growth over the period 2004-2008 to avoid including the years after the onset of the global financial crisis. The figure of 13.9 percent is obtained as 8.35% * (1+66.7%).

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 evaluation of the effects of the FAMEX export promotion program in Tunisia revealed unambiguously positive results in the short-run: beneficiaries initially saw both faster export growth and greater diversification across destinations and products. However, the evaluation of longer-term effects revealed an interesting divergence: three years after the intervention, beneficiaries' growth rates and export levels were not significantly different from those of a control group even though beneficiaries remained more diversified.

We subjected these results to a battery of robustness tests. Instead of including firms that dropped out of the FAMEX program in the control group, we estimated a specification which either included dropouts in the treatment group or excluded them completely. Instead of using a sample that varied with the year of the outcome due to the inclusion of lagged treatments and long-differences in outcomes, we used the fixed sample for FAMEX 2005 recipients (the largest cohort). Instead of using a procedure which involved matching a firm to firms at different points of time, we paired each treated firm with control firms observed in the same year. Instead of using separate sector and year fixed effects, we included sector-year fixed effects to account properly for the possibility of other reforms occurring simultaneously with the FAMEX program. Instead of using just lagged outcome levels in the first-stage probit regression, we included controls for 2-year lagged outcome growth in order to account for the possibility that FAMEX firms were, for reasons unrelated to the program's effect, on a different growth trajectory than others. Instead of using a specification where the outcome was a log-difference, we estimated a specification where the outcome was in levels so as to include firm fixed effects and more adequately address the issue of unobserved heterogeneity at the firm level. None of these tests had a major effect on our basic results.

We then turned to alternative explanations for the observed divergence between export growth and diversification. We established that the divergence was not due to small export transactions to new markets creating an illusion of diversification; to greater exposure of beneficiary firms to crisis-affected economies leading to stunted export growth; or to spillover benefits to non-beneficiary firms resulting in their catching-up in export sales. The last test was conditional on the assumption that spillovers took a particular form: that their impact on a particular firm is

related to the number of beneficiary firms located in the same sector and location. The issue of whether there are in fact spillover benefits of programs is vital and deserves much more attention because their existence creates both a rationale for assistance and a difficulty in discerning its impact.

Our attempt to identify positive explanations for the divergence faced severe challenges, notably the absence of richer firm-level data. Nevertheless, we were able to present some circumstantial evidence that the divergence may be related to within-firm constraints, such as limited experience (measured by the age of the firm) and in-house export capacity (measured by the existence of an export unit prior to receiving FAMEX assistance). The evidence on the role of external constraints, such as access to finance was less straightforward: firms dependent on external finance saw smaller benefits from assistance initially but greater benefits in later periods. We speculated that this may have been due to a positive signaling effect on access to finance, but this is again an area where much more research with better data is needed.

This brought us, finally, to the issue of whether the FAMEX program itself played siren, luring firms into diversification with short-term benefits but at the expense of longer-term growth. First of all, over 60 percent of aggregate FAMEX grants were used to co-finance the cost of activities related to market prospection and promotion, and much smaller amounts were devoted to firm and product development. Second, we found that market prospection and promotion activities correlated more significantly with export outcomes along all dimensions than other components of FAMEX, suggesting that informational barriers are most amenable to effective government assistance. Third, the firms that sought and used FAMEX assistance to establish a presence in new markets and new products were less likely to be disappointed with the longer-term outcome than those seeking and using assistance to expand total exports. Taken together, we would suggest that there is some evidence that FAMEX, either by design, implementation, or simply effectiveness, helped firms to achieve durable diversification but not sustained export growth.

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Appendix

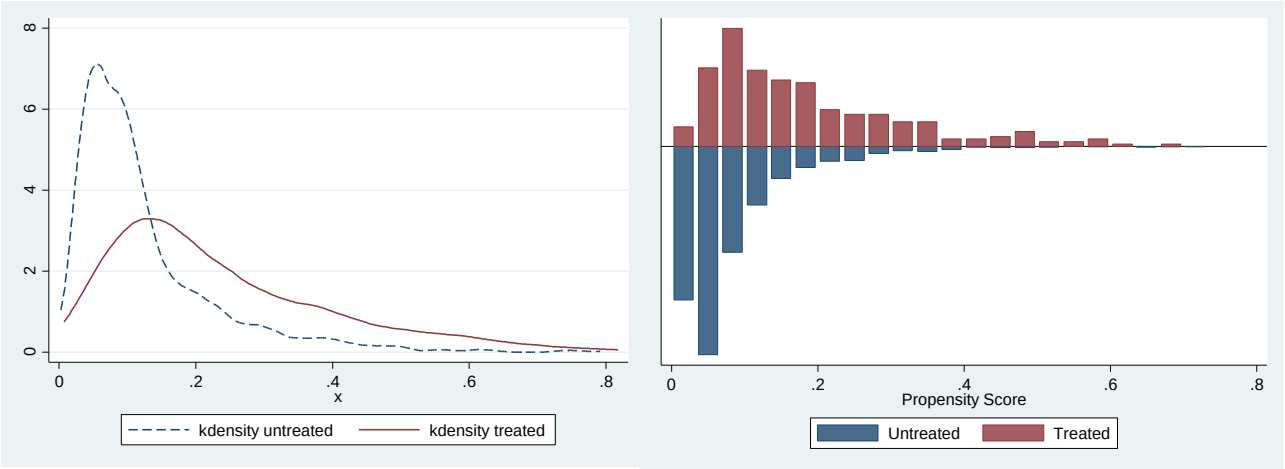
Appendix A: Propensity Score Estimation and Propensity Score Matching

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

	FAMEX treatment status
Age (log)	0.355
	[1.00]
Age squared (log)	-0.098
	[-1.51]
Lagged total exports (log)	-0.038***
	[-4.35]
Lagged number of exported products (log)	0.158***
	[3.40]
Lagged number of export destinations (log)	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 Figure A.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 A.2 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 A.2 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 A.2
Balancing Tests

Covariates	Mean in Matched Sample		T-test		Percentage Bias	Percentage Bias Reduction
	Treatment	Control	T-statistic	P-value		
Age (log)	2.707	2.710	-0.06	0.948	-0.5	96.3
Age squared (log)	7.651	7.665	-0.06	0.955	-0.4	96
Lagged total exports (log)	10.038	9.961	0.17	0.862	1.3	93.2
Lagged number of exported products (log)	1.266	1.250	0.22	0.824	1.7	94.7
Lagged number of export destinations (log)	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 B: Robustness of Main Results

Table B.1. Cumulative Effects of FAMEX – 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 B.2. Cumulative Effects of FAMEX for Firms Treated in 2005

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 B.3. Cumulative Effects of FAMEX 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 B.4. Cumulative Effects of FAMEX Controlling for Sector-Year Fixed Effects

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.
			Sector-year fixed effects included			
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.499***	0.722***	0.561**	0.279	0.038	0.200
	[3.05]	[3.65]	[2.55]	[1.05]	[0.11]	[0.52]
R-squared	*0.180*	*0.232*	*0.231*	*0.228*	*0.234*	*0.246*
Nb. destinations	0.148***	0.191***	0.187***	0.151***	0.142***	0.177***
	[6.11]	[7.03]	[5.86]	[4.20]	[3.05]	[3.22]
R-squared	*0.161*	*0.203*	*0.208*	*0.240*	*0.295*	*0.303*
Nb. products	0.146***	0.174***	0.174***	0.116**	0.157***	0.219***
	[4.74]	[4.76]	[4.40]	[2.51]	[2.68]	[3.37]
R-squared	*0.163*	*0.208*	*0.242*	*0.265*	*0.271*	*0.299*
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 sector-year interaction fixed effects, firm age and age squared, location 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 C: Explanations for Main Results

Table C.1. Cumulative Effects of FAMEX 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.2. 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.

Effects of FAMEX on Survival of New Destinations and Products

We explore whether new destinations and products (at the HS 6-digit level) added by FAMEX firms to their portfolios survive better than new destinations and products launched in the same years by control firms. 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 $z_{i,t+k}$ be the number of those destinations still served (with any product) in year $t + k$ and that 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 be the treatment year and the year before, so our dependent variable is the survival rate of new destinations (or products) introduced in the year before treatment or in the treatment year and surviving uninterruptedly for k years. Our estimable equation is thus:

$$s_{itk} = \alpha + \beta D_{it} \mathbf{X}_{it} \boldsymbol{\gamma} + \delta_t + \varepsilon_{it} \qquad \text{(C.1)}$$

This equation is estimated 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 C.3 for $k = 1,\ldots,5$ 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. There is improved sustainability of new destinations up to three years forward for treated firms, compared to control firms.

Table C.3. Effects of FAMEX on Survival of New Destinations and New 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 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.4. Cumulative Effects of FAMEX, In-House Export Unit and Firm Age

Panel A. In-House Export Unit

	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	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	Export Unit Status						
	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	Export Unit Status						
	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

Panel B. Firm Age

	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	Firm Age						
	FAMEX*Less than 16 years old	0.091	0.085	-0.071	-0.519	-0.539	-0.120
Total		[0.38]	[0.26]	[-0.20]	[-1.24]	[-1.02]	[-0.21]
exports	FAMEX*More than 16 years old	0.931***	1.359***	1.218***	1.060***	0.507	0.437
		[4.18]	[5.63]	[4.27]	[3.09]	[1.11]	[0.85]
	R-squared	*0.177*	*0.231*	*0.231*	*0.229*	*0.228*	*0.247*
Outcome	Firm Age						
	FAMEX*Less than 16 years old	0.111***	0.114***	0.095*	0.078	0.046	0.088
Nb.		[3.26]	[2.74]	[1.95]	[1.50]	[0.69]	[1.21]
destinations	FAMEX*More than 16 years old	0.189***	0.268***	0.285***	0.224***	0.220***	0.242***
		[5.27]	[7.17]	[6.49]	[4.44]	[3.37]	[3.18]
	R-squared	*0.155*	*0.201*	*0.210*	*0.239*	*0.293*	*0.306*
Outcome	Firm Age						
	FAMEX*Less than 16 years old	0.090**	0.136**	0.113*	0.052	0.085	0.175*
Nb.		[2.12]	[2.34]	[1.74]	[0.73]	[0.96]	[1.82]
products	FAMEX*More than 16 years old	0.204***	0.214***	0.243***	0.181***	0.213***	0.251***
		[4.30]	[4.29]	[4.66]	[2.97]	[2.70]	[2.92]
	R-squared	*0.149*	*0.196*	*0.233*	*0.260*	*0.267*	*0.300*
	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 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 Cumulative Effects of FAMEX Interacted with Financial Dependence

	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	FAMEX	0.521**	0.601**	0.307	0.114	-0.313	-0.401
		[2.28]	[2.22]	[0.98]	[0.32]	[-0.71]	[-0.78]
	FAMEX*Financial Dependence	0.491	1.127*	1.690**	1.517*	1.931*	3.109***
		[0.86]	[1.72]	[2.22]	[1.68]	[1.73]	[2.58]
	Financial Dependence	-0.788**	-0.121	-0.190	-0.435	0.214	0.470
		[-2.19]	[-0.30]	[-0.38]	[-0.72]	[0.26]	[0.44]
	R-squared	*0.210*	*0.284*	*0.270*	*0.259*	*0.259*	*0.284*
	Observations	11,090	10,967	8,740	6,540	4,375	2,283

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 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.6. Cumulative 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 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.7. Cumulative Effects of FAMEX Interacted with Project Objective

	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	Objective						
Total exports	More substantive exporter	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	More substantive exporter	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	More substantive exporter	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 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 D: Cost-Benefit Analysis of FAMEX

Table D.1. Rates of Return on the FAMEX Program

		Baseline	TY	TY+1	TY+2	TY+3	TY+4	TY+5
		(BL)				*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 b/ 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 b/		129	426	483	*273*	*50*	*316*
H	Average FAMEX grant per treated b/		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 thousands of Tunisian Dinars;
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 Tunisian Dinars of additional exports per firm per Tunisian dinar of publicly-funded FAMEX grant.

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