Are Export Support Programs Effective? Evidence from Tunisia[#]

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Abstract

The paper evaluates the effect of the FAMEX export promotion program in Tunisia on the export performance of beneficiary firms using firm-level data. Propensity-score matching difference-in-difference and weighted least squares estimates suggest that beneficiaries expand at the intensive and the extensive margin (markets and products). However, this expansion is short-lived for total exports. Our results show a benefit from FAMEX in terms of survival in destination markets. However, our evidence suggests that although treated firms significantly diversified in terms of markets and products, this did not result in reduced export volatility. We also show no evidence of externalities from treated firms to control firms.

JEL classification codes: F13, F14, L15, L25, 017, 024, C23.

Keywords: Export promotion, firms, export margins, Tunisia, impact evaluation, propensity-score matching, matching grant.

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1. Introduction

With the decline in the use of traditional, trade-restricting instruments of trade policy over the last two decades, governments in the developing world have increasingly turned to programmatic interventions aimed at enhancing the competitiveness of domestic firms. These interventions include for example export promotion, export-processing zones, border-modernization programs, and single windows for customs' administrative procedures. How effective are these interventions? Assessing their degree of success is important on at least two counts. First, they mobilize funding from national governments and donors with a potentially high opportunity cost. Second, assessing their impact (or absence thereof) helps to understand the binding constraints to developing country export growth. In particular, export promotion, the subject of this paper, specifically addresses informational problems. Assessing its success may say something about how severe is the informational constraint on exporters.

What do we know about the effectiveness of export promotion? The literature has developed along two strands. One strand—the oldest—relies on cross-country evidence in search of effects on aggregate export performance. Rose (2007) uses a gravity equation to show that diplomatic representations had a positive effect on bilateral trade flows. Lederman, Olarreaga, and Payton (2010) use aggregate export equations to show that, after a long history of failure in developing countries largely due to misguided flanking policies like import substitution and currency overvaluation, export promotion agencies have recently had more success in increasing aggregate exports, in particular agencies whose management involves the private sector.

Another strand—more recent—has looked for effects of export promotion using quasi-experimental methods, comparing the export performance of treated firms with that of a control group. Since enrolment into export promotion programs is never random, most papers go to great lengths to control for selection effects through matching, fixed effects, and two-step (IV or Heckman) estimation methods. The results are mixed, depending, in part, on what performance variable is considered, e.g., export status, diversification, or intensive margin growth. Bernard and Jensen (2004) show an insignificant effect of state-level export promotion expenditures on the probability of exporting for U.S. manufacturing plants. Alvarez and Crespi (2000) conduct a survey of 365 Chilean firms, of which 178 received export assistance in several forms, including access to a business information system and participation in international fairs. Using a two-step approach to control for selection, they show a significant impact only on the number of export destinations; neither product diversification nor overall export growth at the firm level are affected significantly. Görg, Henry and Strobl (2008) use a propensity score matching difference-in-difference (PSM-DID) estimator on Irish data combining plant-level export variables with other characteristics to examine the impact of subsidies. They find that large enough subsidies encourage the export activity of already-exporting firms. However, they find little evidence that non-exporters are encouraged to start exporting by subsidies of any size once unobserved plant effects are controlled for by first-differencing. Volpe and Carballo (2008) also use PSM-DID to explore the effectiveness of Peru's export-promotion program, PROMPEX, using Peruvian firms' customs data. They show that PROMPEX encouraged export growth at the extensive margin (destinations and products) but not at the intensive margin. Girma, Gong, Görg and Yu (2009) use IV techniques on a large panel of Chinese firms to explore the effect of *production* subsidies on exporting activity. They obtain robust evidence of a positive impact at the intensive margin, but little evidence of an encouragement-to-export effect on initially non-exporting firms. Finally, Volpe and Carballo (2010) explore the distributional effects of PROCHILE's export promotion program using Chilean firms' customs data and find stronger positive effects for small firms.

All in all, the evidence so far suggests two remarks. First, export promotion seems to be more successful at affecting the performance of established exporters than at encouraging non-exporting firms to start exporting. This is in accordance with the literature on heterogeneous firms and trade, which suggests that exporters differ from non-exporters in terms of productivity and a host of other characteristics (see, e.g. Bernard, Jensen, Redding and Schott 2007). After all, one could hardly expect export promotion to change ducks into swans. Second, with the exception of Girma, Gong, Görg and Yu (2009) who consider the special case of production subsidies, the evidence seems to be stronger for impacts at the extensive margin than at the intensive margin. This is somewhat natural, and perhaps even desirable, if the information hurdles to break into new markets (product- or destination-wise) are larger than to simply ramp up export volumes.[1]

Underlying the debate about the effectiveness of export promotion as a public policy is a key—but largely un-discussed—assumption, namely that there is a market failure. But the fact that potential exporters are not fully informed about foreign market opportunities is not sufficient, in

[1] See Rangan and Lawrence (1999) and references therein on the hurdles facing the internationalization of firms.

itself, to create a market failure if information production is costly but appropriable. 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." The work of Rauch (1996, 1999) and Rauch and Trindade (2002) also stresses the importance of networks and externalities in the search for foreign trade partners. By contrast, in a recent World Bank survey of African exporters, competitors are cited only marginally as sources of first contacts with foreign customers (Cadot, Iacovone, Pierola and Rauch, 2011) and Bernard and Jensen (2004) find little evidence of cross-firm spillovers in their study of the determinants of the decision to export. Thus, the notion that export entrepreneurship creates externalities that need to be supported by public action, discussed in Hausmann and Rodrik (2003), is still largely an open question. Alternatively, the market failure could arise from a dysfunctional credit market, if adverse selection or moral hazard prevented trustworthy exporters from obtaining credit. In the first case (information as a public good), the absence of a treatment effect from a program aimed at reducing information problems in export markets could be the result of non-appropriability rather than ineffective treatment, and should therefore be interpreted cautiously. Only in the second case (adverse selection or moral hazard) could the absence of a treatment effect from the program be safely interpreted as treatment failure. In this paper, we will attempt to tackle the issue of externalities directly.

This paper explores the impact of Tunisia's export promotion program, FAMEX, which consists of matching grants provided to firms to implement an export business plan. We combine several sources of firm-level data—FAMEX program data, national statistical institute and investment promotion board data, and customs transaction data—into a unique, rich dataset of Tunisian exporters. The advantage of using merged customs data with other data sources is that with customs data there is no recall (or other) bias in the outcome variables compared to the use of firm-level surveys which is a standard approach used to evaluate public programs ex-post.[2]

We use a menu of estimation methods, including PSM-DID and weighted least squares (WLS) regressions to estimate the FAMEX treatment effects, and we extend the analysis in several directions relative to previous studies. First, we examine the sustainability of the effect of export promotion. We find that, compared to a control group, FAMEX beneficiaries successfully

[2] 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).

diversify in terms of destination markets and products; however, the rate of growth of their total exports diverges only temporarily from that of the control group. After two years, the growth rate of FAMEX beneficiaries' total exports converges back to that of the control group.

Second, we show that FAMEX brings a different type of benefit, albeit weak, to export survival.[3] FAMEX beneficiary firms exhibit higher survival rates in their destination markets, up to five years after the treatment. This finding suggests that FAMEX assistance seems to be most successful at mitigating asymmetric information about export market destinations.

Third, we show that although FAMEX beneficiary firms significantly diversify along the destination and product dimensions, they fail to transform this diversification into reduced export volatility. A potential rationale for this finding is that FAMEX firms diversified into riskier product or destination markets.

Fourth, we examine the presence of externalities by estimating the impact on the performance of control firms of the number of FAMEX beneficiary firms in the same industry and geographical area. This is an important—but typically underexplored—part of program impact evaluation, because in the presence of informational spillovers, the absence of a positive measured treatment effect could mean a positive *true* treatment effect transmitted to the control group through positive externalities, which is precisely the combination that would justify government intervention. As it turns out, our evidence of spillovers is weak and if any suggests *negative* spillovers, which may reflect poaching of good managers and workers by treated firms.

Finally, we explore the matching-grant dimension of the program by estimating a "dose-response" function following Fryges (2008) and Fryges and Wagner (2008) and find that only intermediate-sized grants have a significant positive effect on export performance. Moreover, we study several heterogeneity dimensions in the effects of the FAMEX program. We show that the firm's objective in requesting assistance matters for performance: firms coming to FAMEX to expand into new markets or to develop new export products benefit more than firms coming to FAMEX to increase their exports. We show that market prospection and promotion activities correlate more significantly with export outcomes than other components of FAMEX, suggesting that informational barriers are the most amenable to effective government assistance. Firm size

[3] Besedes and Prusa (2011) show the importance of export survival and thus of the increased duration of trade relationships for export growth in developing countries.

does not affect the benefits of FAMEX assistance while the presence of an in-house export unit contributes to stronger benefits.

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 discusses the evidence on survival while Section 7 focuses on diversification and risk. Section 8 examines the presence of externalities. Section 9 discusses extensions, and Section 10 concludes.

2. Export Promotion in Tunisia

Since the mid-1990s the Tunisian government has attempted to reduce the traditional anti-export bias of Tunisia's trade policy (World Bank, 2008). Among several measures, including the elimination of tariffs on imported raw materials, equipment and capital goods in many sectors, the Tunisian government expanded its use of export promotion tools to help exporters get access to foreign markets. The World Bank supported Tunisia through a loan for the Export Development Project (EDP) which was implemented in two phases: the first phase lasted from 2000 to 2004 and the second phase lasted from 2005 to 2009. The Export Promotion Centre (CEPEX in its French acronym) was the key agency under the Ministry of Trade responsible for implementing Tunisia's export promotion activities.

Our analysis focuses on a matching grant fund called FAMEX—which was a key program under the second phase of the EDP project—whose aim was to help Tunisian firms overcome barriers to sell in foreign markets and increase their market access and competitiveness on a demand-driven basis.[4] The rationale for putting the FAMEX program in place was the pervasive lack of information on export markets by Tunisian firms which had difficulty in identifying the right target market, the right product segment, and the right selling channel.

The FAMEX program provided non-reimbursable matching grants to co-finance 50 percent of the cost of firms' export business plans. In terms of firm size, the minimum annual turnover required for FAMEX eligibility was TND 200,000 (144,000 USD) in manufacturing and TND

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

100,000 (71,000 USD) in service and craft sectors.[5] In terms of age, only firms in operation for a minimum of two years were eligible to FAMEX, though in practice were a few exceptions. A firm approaching FAMEX for assistance had to submit an export business plan focused on one of three main objectives: (i) to start exporting if the firm had little or no export experience, (ii) to diversify its destination markets, or (iii) to develop new export products.[6] While a single main objective had to be provided for each export business plan, firms could request assistance for other objectives. The export business plan was evaluated by a panel of five local and international experts and, if accepted, the FAMEX team would provide technical assistance to help the firm improve its plan and would draw up, together with the firm, a list of activities eligible for matching grants of up to 50 percent of their cost, with a ceiling of TND 100,000 (71,000 USD). FAMEX received 1,710 applications and accepted 1,231 of those corresponding to 1,060 firms.[7] In terms of the main objective to request FAMEX assistance, nearly half of the beneficiaries (49%) were already exporters and wanted to expand into new destination markets; 31% had never exported, and 20% wanted to grow out of sole-buyer relationships. The FAMEX 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 by local and foreign experts required to enable Tunisian firms to enter and expand in export markets. Specifically, several 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, note that shares in the program total in the second column add to 100%, but the number of firms in the third column adds up to more than the

[5] The conversions from Tunisian dinars to USD are made based on the exchange rate as of October 10, 2011 (1 USD = 1.39 TND).

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

[7] 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. Unfortunately the FAMEX management team did not systematically collect information on firms whose applications to FAMEX were turned down.

number of FAMEX beneficiaries since each firm typically received co-financing to undertake several activities.[8]

An important remark on the FAMEX program should be made at this point. By taking the form of a matching grant program, the expectation of the FAMEX management team was a low probability of misallocation of funds, little support for low-value services, and aligned incentives in the sense that firms were expected to be highly committed to their projects given their high contribution to the costs.[9] The fact that FAMEX operated under 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.[10] This feature of FAMEX makes it a particularly attractive program to evaluate.[11]

Table 1. FAMEX Program Components

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

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

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

[9] One concern with an export promotion program such as FAMEX is its compatibility with WTO rules. The grants are actually considered as “non-actionable subsidies” (Article 8 of the Agreement on Subsidies and Countervailing Measures) because they were not specific to particular sectors nor particular export markets.

[10] 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.

[11] Other World Bank-funded programs for example in the education area have been shown to suffer from a misuse of funds for purposes other than the intended purpose by Reinikka and Svensson (2004).

3. Data and Descriptive Statistics

To rigorously evaluate the impact of the FAMEX program we need data on FAMEX beneficiary firms as well as on a group of control firms. Our dataset combines three main sources of data: (1) FAMEX program data, (2) data from the National Statistical Institute (INS in its French acronym) and the investment-promotion agency (API in its French acronym), and (3) customs exporter-level data. Next, we provide more details on each of the sources of data.

First, we obtained from the FAMEX management team the complete list of the 1,060 FAMEX beneficiary firms indexed by their tax ID. After dropping (i) 310 firms for whom the impact of FAMEX could not be assessed since they had their first export development plan still ongoing at the end of 2009, (ii) 120 firms which dropped out of the FAMEX program, and (iii) 175 firms in the services sector for which customs export data is not available, we are left with 455 FAMEX beneficiaries.[12] For these beneficiaries we obtained FAMEX program data covering the following variables: the years when the firm joined and terminated the program, firm location, sector, number of employees and turnover when it joined the program and when it left it, whether the firm had an in-house export unit prior to joining the program, the firm's objective in applying to FAMEX and its grant use in terms of total disbursements and breakdown across activities.

Second, we requested from the National Statistical Institute (INS in its French acronym) a stratified sample of control firms with a structure similar to that of the 455 FAMEX beneficiaries. The stratification was performed based on size, prior exporting status, and sector (within manufacturing), resulting in 48 cells. For each cell we asked INS to provide us with roughly twice as many non-beneficiaries as there were FAMEX beneficiaries. To draw the stratified sample of control firms INS used its 2007 census of firms that includes information on firm location, sector, number of employees, turnover by stratum, and date of firm creation.[13] From the INS census, we obtained a group of 910 control firms. Since the INS data was incomplete for a number of firms, we supplemented it with data obtained from Tunisia's investment-promotion agency (API in its French acronym). The API database includes employment, sector, date of firm creation, social capital, and offshore status for 5,000 firms across all sectors (of which 500 are also in the INS census).We extracted a group of 2,000

[12] Dropout firms are those that dropped out of FAMEX and did not apply for a second FAMEX plan.

[13] The turnover strata are: under TND 50,000, TND 50,000-1 million, TND 1 million-2 million, TND 2 million-5 million.

manufacturing control firms from the API database that were neither in the FAMEX sample nor in the INS sample.

Third, we obtained from the Tunisian Customs authority transaction-level export data for the 455 FAMEX beneficiaries, the 910 control firms from INS, and the 2,000 control firms from API. For each firm we obtained the tax ID, monthly transaction value, country of destination, product code (using an 11-digit Tunisian nomenclature derived from the Harmonized System), and weight, for every year between 2000 and 2010. Transaction-level export data is aggregated up to annual totals.

We combine data from the three sources and after checks for data consistency and cleaning procedures, we obtain our combined dataset which is an unbalanced panel of yearly export activity for 2,746 exporting firms with an average of 6 years of data per firm during the 2004-2010 period.[14] Of these firms, 401 benefitted from FAMEX and 2,346 did not. Among the 2,346 non-beneficiaries, 71 were firms who applied to the FAMEX program but were rejected while 126 were firms whose applications to the FAMEX program were accepted but who dropped out of the program for varied reasons. The set of 2,346 firms constitutes our control group in the baseline specifications. In some robustness specifications we will include the 126 FAMEX dropouts in the treatment group instead of the control group while in others we will eliminate the 126 FAMEX dropouts from the sample. In the combined dataset, firm-level characteristics other than those related to export transactions are time-invariant, being available only for 2007. Two important features of our combined dataset are that: (i) the merger of customs transaction data with other sources of firm-level data ensures that the outcomes variables do not suffer from recall bias (as would be the case if the outcomes were part of survey data) and (ii) the fact that all control firms are exporting firms (due to the fact that stratification was based on prior exporting status.) ensures that the sample is more homogeneous and potentially improves the identification of FAMEX effects.[15]Table 2 provides descriptive statistics for FAMEX firms and for control firms in terms of their sector, location, employment and sales categories. The sectoral distributions of FAMEX firms and control firms are quite similar, with the exception of textiles and apparel which is more highly represented among control firms though that sector is also the

[14] 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.

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

major sector for FAMEX firms (accounting for 31% of firms). Although location was not used for stratification, the distributions of FAMEX firms and control firms across locations are not too different: FAMEX firms are more concentrated in Tunis while control firms are more concentrated in the rest of Tunisia. Only minor differences are shown for the distributions of FAMEX firms and control firms across size categories measured in terms of employment or sales.

Table 2. Summary Statistics

(a) Distribution by Sector

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

(b) Distribution by Region

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

(c) Distribution by Employment Category

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

Source: Authors' calculations using the combined dataset.

Table 3 shows the export trends between 2003 and 2010 for FAMEX beneficiaries and for control firms, as well as for Tunisian manufacturing exports as a whole. There is no prima-facie evidence of superior performance for FAMEX firms. In fact, considering the sample period as a whole they perform on average worse than control firms and both groups perform substantially worse than the universe of Tunisian manufacturing exporters (represented by aggregate trade flows). Moreover, in the early stage of the global financial crisis in 2007-2008, FAMEX firms recorded a 6% drop in total exports while control firms still had positive export growth. In the recovery phase FAMEX firms are growing at a smaller pace (2%) than their counterparts that did not receive assistance. This might be indicative of risk-taking behavior by treated firms:

exposing themselves to destination markets that stood to contract most at the outset of the crisis and that experienced a slower recovery. We will return to this conjecture later in the paper. Also note that our sample of FAMEX and control firms accounts for an important share of total Tunisian exports.[16]

Table 3. Growth in Tunisia's exports

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

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

4. Estimation Issues

To understand whether the FAMEX intervention was effective in its objective of promoting export competitiveness in Tunisia we need to estimate the causal effect of FAMEX on firm-level export outcomes. The main identification problem in this evaluation is that the assignment to FAMEX assistance is far from random: FAMEX beneficiaries may differ substantially from other firms in characteristics that affect both their participation as well as the export outcomes. Hence it is vital to use non-experimental impact evaluation methods which address that selection bias to obtain a credible estimate for the counterfactual of what would have happened to FAMEX beneficiaries if they had not received assistance.[17]

As a first approach to identify the impact of FAMEX, we consider a propensity score matching difference-in-differences (PSM-DID) estimator. This estimator has been widely used in evaluation of programs in several areas and in particular in the area of export promotion by Görg, Henry and Strobl (2008) and Volpe and Carballo (2008).[18] The PSM-DID method controls for selection bias by comparing the change in outcomes for program beneficiaries relative to the

[16] The exports by FAMEX firms account for about 10% of Tunisia's total exports.
[17] This is the fundamental problem of causal inference defined by Holland (1986).
[18] A seminal study employing this method for program evaluation is Heckman, Ichimura, and Todd (1997).

change in outcomes for 'observationally similar' control firms before and after the program.[19] In our study, 'observationally similar' firms will be defined based on a propensity score which is the probability that a firm receives FAMEX assistance, given a rich set of observable firm covariates, and on a metric of proximity between propensity scores. The PSM-DID estimator is based on the twin assumptions that (i) assignment to treatment (or the decision to undertake it) is independent of potential outcomes, conditional on observed pre-treatment covariates; and (ii) there is sufficient overlap in the distribution of propensity scores between the treatment and control groups (i.e., it is possible to find matches for all or most treated firms).[20] While the PSM-DID estimator is based on assumption (i) designated as selection on observables, by relying on a comparison of changes in outcomes, it does control for unobserved time-invariant pre-program differences across firms potentially leading to self-selection into the program and influencing outcomes (Blundell and Costa Dias, 2009).

Let y_{it} be an export outcome variable with *i* indexing firms and *t* years, *T* be the treatment group, *t(i)* the time at which firm *i* enrolls into FAMEX and D_{it} the treatment indicator variable as in:

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

Suppose first for simplicity that all firms undergo treatment, i.e., receive FAMEX assistance, in the same year τ. Let *T*, *C*, and *S* be, respectively, the treatment and control groups and their common support and$y_{i,\tau-1}$ and $y_{i\tau}$ be firm *i*'s outcome before and after treatment. The PSM-DID estimator is given by:

$$\hat{\gamma}^{PSM-DID} = \sum_{i \in T \cap S} \left[\Delta y_i - \sum_{j \in C \cap S} w_{ij} \Delta y_j \right] \qquad (2)$$

where

$$\Delta y_i = y_{i\tau} - y_{i,\tau-1} \qquad (3)$$

and the weights w_{ij} are determined by the matching method.[21]

[19] 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.

[20] Assumption (i) indicates that assignment to treatment is unconfounded or independent of outcomes conditional on covariates (Hirano, Imbens, and Ridder, 2003).

[21] The control group $C \cap S$ is formed by picking for each treated firm the control firms with closest propensity score. Depending on the matching method there can be for each treated firm either one matched control firm or several using a weighted scheme. Nearest-neighbor matching identifies the firm in the control group with the closest propensity score to that of treated firm *i* but because the distance is unrestricted, it can be large. Caliper maching solves this problem by imposing a predefined maximum distance. If no control group firm is found within that

While we will show PSM-DID estimates in Section 5, a complication arises in our setup as the treatment year is not the same for all firms: some joined FAMEX in 2005, others in subsequent years up to 2009. Let $t(i)$ be the year of enrollment into FAMEX or treatment year for firm i. The expression for the before-after difference in outcomes is thus:

$$\Delta y_i = y_{i,t(i)} - y_{i,t(i)-1} \tag{4}$$

instead of Eq. (3). This expression is well defined for treated firms but is not defined for untreated firms for which a treatment year is not available. Hence, in standard statistical packages for propensity score matching estimation (such as psmatch2 in Stata) the years when treatment firms are matched with control firms are disregarded, which may be problematic if calendar time matters for performance.

The main approach that we use to deal with this issue is to rely on weighted-least squares (WLS) regressions following Hirano, Imbens, and Ridder (2003) (henceforth HIR). The weights are a function of the estimated propensity scores and create a balance in covariates across treated and control firms to allow for an unbiased regression-based estimator of the effect of a program.[22] To be more specific, let $\hat{p}_i$ be the estimated propensity score of firm i and $\hat{r}_i = \hat{p}_i / (1 - \hat{p}_i)$ its estimated odds ratio. HIR propose to estimate a weighted least squares regression of an outcome on a treatment indicator and other covariates giving a weight of 1 to the observations of treated firms and a weight of $\hat{r}_i$ to the observations of control firms. The advantage of using a regression framework is the possibility of taking into account the actual year of treatment and of controlling for firm covariates. Since our combined dataset is a panel of firms over the 2004-2010 period, a regression framework is attractive in that it allows to control for cyclical trends through year fixed effects. Our baseline equation, to be estimated by WLS, explains changes in firm outcomes and can thus be thought of as a difference-in differences (DID) equation:

$$\Delta y_{it} = \alpha + \beta I_{it} + \overline{X}_i \gamma + \delta_t + u_{it} \tag{5}$$

where $\overline{X}_i$ is a vector of time-invariant firm covariates and the indicator variable for FAMEX participation could in principle be defined as:

distance, treated firm i is ignored. Nearest-neighbor matching can be extended to any fixed number of matches. An alternative method – that we will use - is kernel matching which uses a weighted average of *all* control-group firms as a match for *each* treated firm i . See Caliendo and Kopeinig (2008) for additional details on matching.

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

$$I_{it} = \begin{cases} 1 & \text{if } i \in T \text{ and } t = t(i) \\ 0 & \text{otherwise.} \end{cases} \tag{6}$$

One concern with the definition in Eq. (6) is that it allows treated firms after their treatment year to be part of the control group. To address this concern we set I_{it} to a missing value for $t > t(i)$ and also for all $t < t(i) - 1$. Thus, in the baseline regression we consider only the treatment year and the year just prior to treatment which means that we are left with a two-period panel for each firm. Since the dependent variable is the log-difference in the outcome variable defined in Eq. (4), the sample structure degenerates into a cross-section of firms in which calendar time comes in only through the year of enrolment into FAMEX. This is controlled for by time effects $\delta_{t(i)}$ which indicate whether firm *i* enrolled in FAMEX in 2005, 2006, or any subsequent year up to 2009. The baseline WLS regression equation is thus given by:

$$\Delta y_i = \alpha + \beta I_i + \overline{X}_i \gamma + \delta_{t(i)} + u_{it}\,. \tag{7}$$

Our WLS regressions will be estimated for the treated and control firms that are part of the common support identified by the propensity score matching. We will estimate variants of Eq. (7) considering one-year log-differences in outcomes in the period subsequent to FAMEX enrollment, that is, where the dependent variable is defined as below, for *k* ranging from 1 to 5:

$$\Delta y_i^{TY+k} = y_{i,TY(i)+k} - y_{i,TY(i)+k-1} \tag{8}$$

We will also estimate other variants of Eq. (7) considering progressively longer time differences relative to the year of FAMEX enrollment, that is, where the dependent variable is defined as below, for *k* ranging from 1 to 5:

$$\Delta y_i^{TY+k} = y_{i,TY(i)+k} - y_{i,TY(i)-1} \tag{9}$$

Finally, as an alternative approach to deal with the differences in treatment year across firms we will obtain as a robustness check a PSM-DID estimator that restricts control firms matched to treated firm *i* to be taken in $t(i)$ following Todo (2011).[23]

While Glazerman, Levy, and Myers (2003) show that the PSM method can reduce bias in program impacts, particularly when combined with DID or with WLS regression as in our case, and the use of such methods is now pervasive in the evaluation of public programs, these estimators have limitations. In particular, the resulting estimates may be biased if unobserved

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

time-varying firm characteristics affect both participation and outcomes. In a non-experimental study such as ours, selection bias on unobservables can never by fully ruled out.

5. FAMEX Treatment Effects: Short-Lived or Persistent?

5.1. Propensity Score Matching Estimates

A key ingredient for both the WLS regression estimators and the PSM-DID estimator are propensity scores which are obtained from a cross-sectional probit regression explaining the probability of Tunisian firms receiving a FAMEX grant in any year between 2005 and 2009 estimated using the full sample.[24] The explanatory variables are a rich set of firm covariates: firm age in levels and squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial pre-FAMEX total exports, number of exported products and number of destinations served.[25] The probit estimates show that firms with larger exports and those exporting 100% of their output in 2004 are significantly less likely to receive a FAMEX grant thereafter, whereas smaller exporters and those exporting more products and shipping to more destinations are more likely (see Appendix Table A.1). Exporters located in Tunis are significantly more likely to receive a FAMEX grant. The insignificance of the sector fixed effects suggests that FAMEX grants did not target particular sectors.

Propensity scores for each Tunisian firm are retrieved from the probit regression as the predicted probabilities of getting treated and their densities for treated and control groups. The relatively similar shapes of the densities and the seemingly very large common support imply that most FAMEX firms can, in principle, be matched to one or more control firms based on the closeness of propensity scores (see Appendix Figure B.1).[26] However, the imperfect overlap in the densities suggests that it is important to use matching and re-weighting to select control firms that are more comparable to FAMEX firms. We perform propensity score matching using a

[24] While we use data on export outcomes in 2009 and 2010, we exclude from the sample the firms that received the FAMEX grant in 2009 since we are unable to assess the effect of the grant for such firms.

[25] The initial export-related variables refer to year 2004. As Bernard et al. (2010) show, more productive firms tend to export more products to more destinations, therefore controlling for the initial 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).

[26] The importance of a large common support and similarity in the distribution of covariates/propensity scores across treated and untreated groups for unbiased matching DID estimators is shown by Heckman, Ishimura and Todd (1997).

kernel estimator, which is a non-parametric estimator that uses a weighted average of all control firms to match each treated firm, where the weights depend on the distance between the propensity score of a control firm and the treated firm.

Imposing the common support condition for this matching (as in assumption (ii) above for PSM-DID estimators) implies dropping treated firms whose propensity score is higher than the maximum or lower than the minimum score for the control group. In our case, this results in no loss of treated firms. Our common support includes 401 FAMEX beneficiaries and 2,346 control firms. To assess the quality of the matching we implement tests for the balancing hypothesis proposed by Rosenbaum and Rubin (1985), Dehejia and Wahba (2002) and Smith and Todd (2005). The rationale behind the tests is to assess whether the matching is able to balance the distribution of covariates in the treatment and control groups (Caliendo and Kopeining, 2008). Overall, the balancing tests suggest that our matching procedure generates sufficiently 'similar' control firms to match to each treated firm in the common support (see Appendix B).

5.2. PSM-DID and WLS Regression Results

Table 4 presents results from a PSM-DID estimator as well as from WLS regressions using the weighting scheme based on the propensity scores just described for three firm-level outcome variables: total exports (in Tunisian dinars), the number of destinations, and the number of exported products, all in log-differences. As a benchmark, column (1a) shows the PSM-DID estimates of FAMEX effects in the year of enrolment based on Eq. (5) with w_{ij} being determined by a kernel matching.[27] Column (1b) shows the estimated impact of FAMEX assistance in the year of enrolment (henceforth designated as 'TY') based on the WLS regression in Eq. (7). Columns (2)-(6) show the year-to-year estimated impact of FAMEX after enrollment by modifying the dependent variable in Eq. (7) to be the log-difference given by Eq. (8). As further clarification to the estimates shown in Table 4, consider the example of a firm enrolled in FAMEX in 2005 and the outcome total exports. The dependent variable in the WLS regression for such firm is: the change in log-total exports between 2004 and 2005 in column (1b); the change in log-total exports between 2005 and 2006 in column (2); the change in log-total exports between 2006 and 2007 in column (3); and so on. The indicator variable for treatment included

[27] The standard errors reported for the PSM-DID estimator are based on the estimator of the asymptotic variance, as bootstrapped standard errors can be invalid for this estimator because their asymptotic properties are not known (Abadie and Imbens, 2006). But we obtain qualitatively similar findings when using bootstrapped standard errors.

in Eq. (7) for such firm is: 0 until 2004, 1 in 2005, and missing thereafter in column (1b); 0 until 2005, 1 in 2006, and missing thereafter in column (2); 0 until 2006, 1 in 2007, and missing thereafter in column (3); and so on. This indicator variable definition ensures that treated firms are not included in the control group in the years post-treatment (which would happen if the indicator variable switched back to zero after enrolment). To estimate the specifications in Table 4 we pool across all Tunisian firms receiving FAMEX treatment in different years and all control firms in the common support. All regressions use robust White-corrected *t*-statistics.

Several observations stand out from Table 4. The impact effects in columns (1a) and (1b) are significant at the 1% level for all outcome variables and the estimates are in fact very close. The magnitude of the effects is large: 50% more growth in total exports (in logarithms of values) and 15% more growth in export destinations and products (in logarithms of counts) for FAMEX beneficiaries, compared to the control group. However, growth effects vanish after the treatment year, remaining significant in the second year only for the number of destinations. Oddly, destination and product growth show positive and significant effects five years after treatment. This finding is based only on firms that enrolled in FAMEX in 2005, and it means that they enjoyed in 2010 a resumption in destination and product growth, compared to control firms.

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

Difference Estimator	TY-(TY-1) PSM-DID (1a)	TY-(TY-1) WLS reg. (1b)	(TY+1)-TY WLS reg. (2)	(TY+2)-(TY+1) WLS reg. (3)	(TY+3)-(TY+2) WLS reg. (4)	(TY+4)-(TY+3) WLS reg. (5)	(TY+5)-(TY+4) WLS reg. (6)
<u>Outcome</u>							
Total exports	0.562	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	0.113	0.150	0.086	0.052	0.021	0.036	0.059
	(4.33)**	(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	0.11	0.147	0.071	0.049	0.008	0.060	0.097
	(5.59)***	(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 parentheses; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated and control firms in the common support. PSM-DID estimates are based on propensity scores obtained using kernel matching. The WLS regressions include treatment year fixed effects, firm age and age squared, location and sector fixed effects, a dummy variable for firm size (based on employment), a dummy variable for firms exporting 100% of their output, firm initial exports, number of exported products and number of destinations served.

In Table 5, we take a different angle on persistence and report cumulative effects of FAMEX, which show for how long the impact effect remains perceptible. Thus, we modify the dependent variable in Eq. (7) to be defined as the log-difference given by Eq. (9). Those log-differences are taken over increasingly longer time intervals and are always relative to the year before FAMEX treatment. To estimate the specifications in Table 5 we pool across all Tunisian firms receiving the FAMEX treatment in different years and all controls in the common support. The effects on total exports do not vanish one year after the enrolment in FAMEX because the positive impact effect of the treatment year carries over. But after three years, no effect remains on total exports of FAMEX beneficiaries relative to those of control firms, as shown by columns (4)-(6). The estimates in Table 5 show, however, long-lasting positive effects on the numbers of destinations and products of FAMEX beneficiaries relative to control firms.

Table 5. Cumulative Effects of FAMEX on Export Outcomes

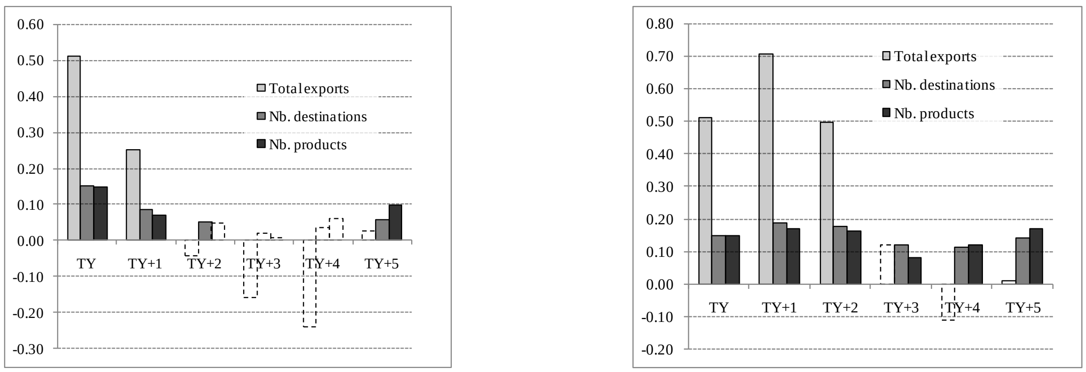
Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Outcome</u>						
Total exports	0.511	0.707	0.499	0.120	-0.113	0.008
	(3.08)***	(3.53)***	(2.27)**	(0.45)	(-0.34)	(0.02)
R-squared	*0.17*	*0.22*	*0.22*	*0.22*	*0.23*	*0.25*
Nb. destinations	0.150	0.187	0.178	0.122	0.113	0.141
	(6.10)***	(6.81)***	(5.60)***	(3.43)***	(2.45)**	(2.65)***
R-squared	*0.15*	*0.19*	*0.20*	*0.22*	*0.27*	*0.28*
Nb. products	0.147	0.171	0.163	0.081	0.119	0.170
	(4.68)***	(4.62)***	(4.09)***	(1.77)*	(2.06)**	(2.67)***
R-squared	*0.15*	*0.19*	*0.23*	*0.25*	*0.25*	*0.28*
Observations	12,263	12,263	9,915	7,526	5,087	2,656

Notes: T-statistics based on robust standard errors in parentheses; *: 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, firm initial exports, number of exported products and number of destinations served.

To sum up, according to Table 4, one year after FAMEX enrolment, the growth rates in total exports stop differing between the treatment and control groups; according to Table 5 after three years, even the levels of total exports have converged, as the cumulative log-change is no longer significantly different across FAMEX beneficiaries and control firms. Figure 1 summarizes the findings. Whatever the econometric approach we find the same patterns, FAMEX had a positive impact on export performance in the year of treatment, while considering a longer time period of up to give years post-treatment, FAMEX had positive impacts only on the extensive margins on trade in terms of products and destinations.

Figure 1. Year-to-Year and Cumulative Effects

Year-to-Year Effects (Table 4) | Cumulative Effects (Table 5)



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

5.3 Robustness Checks

We perform several checks to show the robustness of the findings in Tables 4 and 5. First, we address the fact that censoring progressively reduces the sample size across the columns in those tables.[28] The reason for this sample shrinkage is that firms that enrolled in FAMEX in later years drop from the sample as the time differences grow larger: e.g., for a firm enrolled in FAMEX in 2008, the observations to compute its TY*(i)*+3-TY*(i)*+2 difference are not available. Since the WLS regression conditions on the year of treatment through year fixed effects, this is not a major problem. However, we do re-estimate the specifications in Tables 4 and 5 using a restricted sample that includes only firms enrolled in FAMEX in 2005 and control firms in the common support and show that the results are qualitatively maintained (see Appendix Table C.1).

Second, the FAMEX effects shown in Tables 4 and 5 are based on a sample which includes in the control group 126 firms that were accepted into the FAMEX program but subsequently dropped out of the program. If those dropouts are more similar to FAMEX recipients than other control firms, their inclusion in the control group should improve the quality of the matching and

[28] We are grateful to Beata Javorcik for attracting our attention to this issue.

subsequently the accuracy of the estimates of the treatment effect.[29] However, we check the robustness of our findings by re-estimating Eq. (7) with the dependent variable defined by Eq. (9) using alternative samples: (A) eliminating the dropouts from the sample or (B) including the dropouts in the treatment group (see Appendix Table C.2). Relative to the baseline effects, the estimates show that when dropouts are excluded from the sample a slightly larger effect from FAMEX assistance is obtained, whereas the opposite is true when dropouts are included in the treated group. Hence, dropout firms performed better than other control firms but performed worse than FAMEX firms that took their export business plan to full completion.

Third, we modify the matching method used to obtain the propensity scores for our PSM-DID estimators to a five-nearest neighbours estimator and to a radius estimator with caliper equal to 0.1. The results, available upon request, are qualitatively maintained.

Finally, we address a potential bias in the PSM-DID estimates in Table 4 caused by the fact that they rely on a procedure allowing a firm enrolled in FAMEX in year *t(i)* to be matched to a combination of control firms drawn in year *t(i)* but also in years other than *t(i)*, while performing the matching based on time-invariant firm characteristics. A firm j which is a good match for firm i (in the sense of having a similar propensity score) may be very different from firm i in terms of outcomes if firm i is observed in year *t(i)* while firm j is observed in another year. For instance, if *t(i)* is a 'bad' year and the control firm is observed in a 'good' year, then the treatment effect will be underestimated. In our WLS regressions, year fixed effects control for macroeconomic shocks, but such controls are not used in the PSM-DID estimator. For better comparability with WLS estimates, the PSM-DID estimates need therefore to be adjusted using a procedure that pairs each treated firm with only with control firms in the year *t(i)* when the firm enrolls into FAMEX, as in Todo (2011).[30] This matching procedure generates for each treated firm in year *t(i)* a 'fictitious' control firm which is a weighted average of control firms with close propensity scores all observed in year *t(i)* and computes an outcome for that 'fictitious' control

[29] An ideal specification would consider only drop-outs as the control group. However, such specification is impossible to estimate given the small number of dropouts relative to the number of FAMEX firms.

[30] We are grateful to Yasusuki Todo for sharing his matching routine. The routine restricts treated firm *i*'s matches to be exclusively control firms taken at *t(i)*. This substantially reduces the number of possible matches and is thus feasible only with a sufficiently large initial control group. See also Arnold and Javorcik (2009) on the importance of ensuring that matched observations are assigned from the same year and sector to eliminate the possibility that differences in unobservable dimensions of firm performance across different year-sector combinations exert influence on the estimated PSM-DID effects. Due to the relatively small number of exporters in each sector shown in Table 1, we are unable to consider sector in this type of PSM.

firm in year *t(i)*.[31] This results in a new cross-sectional dataset per year where each treated firm is matched with a unique 'fictitious' control firm, whose size is by construction twice the number of firms for which that year was the year of FAMEX enrollment. We pool across all treatment years and estimate by OLS different DID regressions where the dependent variables are progressively longer time-differences in export outcomes on a FAMEX treatment indicator (see Appendix Table C.3). The estimates reveal a strong positive effect of FAMEX on export growth both at the intensive and the extensive margins in the year of FAMEX enrolment. But in most years after enrolment the treatment effects are positive only for the number of destinations and products and are short-lived for total exports, confirming the findings in Table 5.

6. FAMEX and Export Survival

The vanishing effects of FAMEX on total exports shown in Section 5 may mask the effects of the program on export survival. Recently, studies have shown the importance of longer export survival for export growth in developing countries. As such a positive effect of FAMEX on export survival could be viewed as a clear benefit from the program. However, conceptually there is no clear prior on the expected effect of the FAMEX program on export survival. On the one hand, if FAMEX provides continued assistance, then it should raise export survival rates (whether in terms of destinations or products). On the other hand, if FAMEX reduces the sunk costs of entry into export markets (whether in terms of destinations or products), the hysteresis literature would suggest that it would reduce hysteresis and thus reduce survival.

To examine this issue, we construct, for each firm, destination survival rates, product survival rates, and destination-product survival rates in its export portfolio over progressively longer time periods. Focusing on destinations –an analogous procedure is employed for products– the survival rate between year *A* and year *B* is calculated as the ratio between the number of surviving destinations for a firm between year *A* and year *B* (and in all intervening years between *A* and *B*) and the sum of the number of surviving destinations and the number of dropped destinations between year *A* and year *B*. We re-estimate Eq. (7) with the dependent variable being the firm-level destination, product, or destination-product survival rates and the results are shown in Table 6. Since the dependent variable is restricted to vary between 0 and 1, WLS

[31] This matching procedure is based on the probit regression in Appendix Table A.1 and the corresponding balancing tests are satisfactory.

estimation would result in biased coefficient estimates in the case of a large variance of the error term in Eq. (7). To preclude this possibility we use weighted Tobit estimation.

Table 6. Effects of FAMEX on Export Survival

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					
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
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
Destination-product survival rate	0.024	0.008	0.002	-0.01	0.081
	(0.72)	(0.23)	(0.04)	(-0.18)	(1.31)
R-squared	*0.016*	*0.025*	*0.033*	*0.045*	*0.054*
Observations	6,625	5,440	4,188	2,833	1,504

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

The estimates in Table 6 show very clearly that FAMEX had positive and significant effects on survival rates of beneficiary firms only for destination survival rates. The positive effects are verified up to five years after FAMEX treatment. This finding suggests that FAMEX assistance seems to be most successful at mitigating asymmetric information about export market destinations.

7. Did FAMEX Assistance Encourage Risk-Taking?

The experimental economics literature has long noticed that individuals tend to be more willing to take risks out of windfall gains than out of regular earnings, a phenomenon called the "house money effect" (a term borrowed from gambling). For instance, Thaler and Johnson (1990) asked two groups of individuals to choose between two lotteries with the same expected value, one riskier than the other. The treatment group was given a prior endowment (the windfall) while the control group was given no endowment. They report that 77% of the treatment group's individuals chose the risky lottery against only 44% for the control group. We noted in Table 3 that FAMEX firms performed worse in terms of export growth than control firms during the onset of the global financial crisis (2007-2009). Although FAMEX was a matching-grant program rather than a pure subsidy—precisely in order to limit moral hazard—could it be nevertheless that export promotion encouraged beneficiary firms to take more risk?

Before exploring this question, we provide further evidence of the diversification brought about by FAMEX. Growth in the number of destinations and products exported is shown to be persistently higher for FAMEX firms relative to control firms in Tables 4 and 5. To combine the two extensive margin dimensions, we construct for each Tunisian firm two standard measures of concentration—a Herfindahl index and a Theil index—of their export shares across product-destination cells (below designated as product-destination *markets*) in each year. We re-estimate Eq. (7) by WLS, using each of the concentration measures as the dependent variable with time differences defined as in Eq. (9). Table 7 presents the results which show a clear increase in product-destination diversification upon treatment and over time for FAMEX firms relative to control firms.

Table 7. Effects of FAMEX on Concentration and Volatility

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)	Before-After 2005
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome							
Herfindahl index	-0.131	-0.108	-0.142	-0.153	-0.144	-0.267	
	(-4.34)***	(-2.73)***	(-3.42)***	(-3.34)***	(-2.57)**	(-3.91)***	
R-squared	*0.08*	*0.11*	*0.09*	*0.10*	*0.10*	*0.13*	
Theil index	-0.021	-0.020	-0.025	-0.025	-0.026	-0.046	
	(-4.83)***	(-3.42)***	(-4.13)***	(-3.72)***	(-3.17)***	(-4.38)***	
R-squared	*0.10*	*0.13*	*0.12*	*0.12*	*0.14*	*0.15*	
Observations	7,743	7,403	5,794	4,244	2,774	1,398	
Coeff. variation of exports							0.023
							(0.36)
R-squared							*0.17*
Observations							320

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes treated firms 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, firm initial exports, number of exported products and number of destinations served.

To address the possibility of increased risk-taking linked to product and destination diversification by FAMEX beneficiaries, we examine whether the volatility in total export sales of Tunisian firms was affected by the participation in the FAMEX program. Export volatility is measured by the coefficient of variation in total exports for each firm in two sub-periods, one before and one after FAMEX treatment. To have sufficient time variation post-FAMEX treatment for a meaningful coefficient of variation calculation, we consider only firms that enrolled in FAMEX in 2005 (for which five data points are observed). For those firms as well as for control firms part of the sample in 2005 we obtained additional customs data going back to 1999 to calculate the coefficient of variation prior to 2005. We re-estimate Eq. (7) by WLS using as dependent variable a before-after 2005 difference in the coefficient of variation. The results are shown in column (7) of Table 7.[32] Despite its diversification effects, the evidence does not show any benefits from the FAMEX program in reducing export volatility for beneficiary firms relative to control firms. If anything that volatility increases, though the effect is insignificant. A possible reason for why export volatility did not decrease for FAMEX firms although they diversified their portfolios could be that they diversified into riskier product or destination markets.

8. Externalities

Estimated treatment effects can be biased by the presence of general equilibrium effects - which not very likely in the case of an assistance program of limited scale like FAMEX - or by externalities "polluting" the outcomes of the control group.[33] For instance, it might be that FAMEX beneficiaries' actions such as the participation in trade fairs or the hiring of export-marketing consultants were visible and easily imitable by other firms in their sector or location. It might even be that FAMEX beneficiaries shared information voluntarily, as exporters from the same country do not necessarily see themselves as competitors.[34]

The benefits of FAMEX could be underestimated if the control group's export outcomes improved as a result of spillovers from FAMEX beneficiaries. Importantly, the presence of

[32] The sample used for that regression is small because it includes only firms that exported uninterruptedly from 1999 to 2010 divided into 135 FAMEX firms that received treatment in 2005 and 185 control firms.

[33] Formally, treatment effects are measured under the assumption of "stable unit treatment value assumption" (Rubin 1961), which means that a treated individual's outcome is independent of the treatment's mode of administration and of the status of other individuals (treated or not).

[34] Cadot, Iacovone, Pierola, and Rauch (2011) show that for African exporters their expected survival rises with the number of firms from the same country exporting the same product to the same destination.

externalities indicates the non-appropriability of knowledge about export markets and thus would be needed to justify the intervention by the Tunisian government and the World Bank.

Interestingly, an interpretation for our main findings of vanishing effects of FAMEX assistance after two years could be that control firms catch up, indicating the presence of externalities. The difficulty in investigating this issue explicitly is that the measurement of spillovers is elusive, especially when one does not know what their transmission channel is. In their study of a child de-worming program in Kenya, Miguel and Kremer (2004) estimate program externalities across schools by including in the outcome regressions the number of pupils in treated schools within a certain distance, which is exogenous due to randomized school assignment.

We construct a proxy for the exposure to treated firms following the logic of Miguel and Kremer (2004). That is, we assume that externalities are more likely to exist between firms in the same sector and region. Although the case for transmission is less clear-cut in our case, we assume that firm managers are more likely to know each other if they produce similar goods in the same region. Thus, we construct a count variable equal to the number of FAMEX beneficiaries in each sector-region-year cell.

The use of such variable is well grounded theoretically and empirically. Krautheim (2011) develops a trade model with heterogeneous firms including a spillover effect from the number of exporters to the fixed costs of exporting. Empirical studies on export spillovers such as Aitken et al. (1997), Bernard and Jensen (2004), or Kneller and Pisu (2007) use the presence of exporters in an industry and location to capture spillovers. Recent studies relying on customs data such as Koenig, Mayneris, and Poncet (2010) and Mayneris and Poncet (2010) also test for the presence of export spillovers using the numbers of exporters in the same region exporting similar products and/or to similar destinations.

Our reduced form approach to capture the effect of spillovers consists of estimating regressions that explain export-related outcomes based on the exposure to FAMEX beneficiaries. We begin by considering a sample including *only* control fir*ms*. The variable measuring the exposure to FAMEX beneficiaries is a time-variant characteristic that is available for each firm in the control group. Since the year of enrolment *t(i)* is not defined for control firms, we consider regressions of first-differences in their outcomes in a panel framework given by:

$$\Delta y_{jsrt} = \alpha + \sum_k \beta_k n_{jsr,t-k} + \delta_j + \delta_{st} + \delta_{rt} + \nu_{jsrt} \tag{14}$$

where s and r designate, respectively, sectors and regions, $n_{jsr,t-k}$ is the number of FAMEX beneficiaries in control firm j's sector-region in year $t-k$. Eq. (14) includes sector-year interaction fixed effects δ_{st} and region-year interaction fixed effects δ_{rt} to control for shocks in a sector or region which could affect both outcomes and the number of firms receiving FAMEX support in that sector or region. The exposure variable enters with various lags to deal to mitigate any potential endogeneity and more importantly to allow for the slow diffusion of externalities. Eq. (14) includes also firm fixed effects δ_j to account for unobserved firm heterogeneity.

Table 8 shows the results from estimating Eq. (14).[35] The estimates fail to suggest any positive externalities. On the contrary, the results suggest negative effects of a larger presence of FAMEX beneficiaries on all export outcomes of control firms, though only the four-year effect in column (12) is significant. While the evidence of negative spillovers is weak, it raises the possibility that FAMEX firms use their cash to poach talented managers and workers from control firms and serves as a reminder that externalities can go either way. We re-estimated a variant of Eq. (14) considering a sample including all firms, both FAMEX beneficiaries and control firms. The estimates again provide no evidence of externalities, just (see Appendix Table C.4).

Table 8. Effect of Exposure to FAMEX Firms on Control Firms' Export Outcomes

Estimator	Firm Fixed Effects (Within)				Firm Fixed Effects (Within)				Firm Fixed Effects (Within)			
Outcome	First diff. of total exports for sample of control firms				First diff. of nb. destinations for sample of control firms				First diff. of nb. products for sample of control firms			
	(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 parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes only control firms in the common support or outside.

[35] The results from OLS estimation without firm fixed effects are qualitatively similar.

Finally, one could argue that spillovers to control firms are more likely to emerge from FAMEX firms whose objective was to expand into new destinations or to export new products than from FAMEX firms that are just becoming more substantive exporters. If that were the case, the spillovers from all FAMEX firms in Table 8 could have been under-estimated. To address this possibility we re-estimate Eq. (14) including a variant of the count variable equal to the number of FAMEX firms in each sector-region-year cell whose objective was to reach more export destinations or export more products. The estimates indicate that not even these types of FAMEX firms generated externalities to the control firms (see Appendix Table C.4).

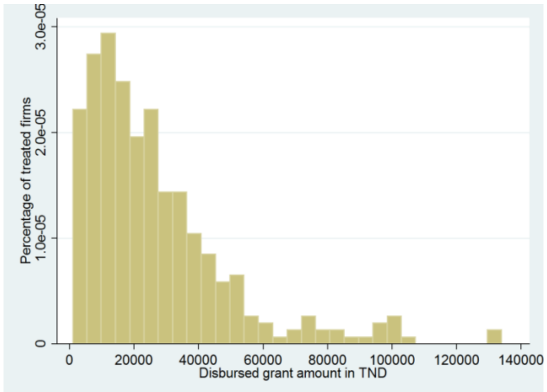
9. Extensions

9.1 Grant Size

Given the matching-grant nature of the FAMEX program it is of great interest to explore whether treatment effects for Tunisian firms differ systematically as a function of the size of the grants disbursed. Due to limited data access in previous studies, this analysis is novel in the export promotion literature. We use the Generalized Propensity Score (GPS) methodology developed by Imbens (2000) and Hirano and Imbens (2004) to construct a "dose-response" function showing the estimated causal effect of all "dosages" of the treatment on the various export-related outcomes.[36] The different grant amounts disbursed by FAMEX across firms constitute different "dosages" of the treatment. The implementation of the GPS methodology involves three stages, described in Fryges (2008), Fryges and Wagner (2008), and Bia, Flores and Mattei (2011).[37] The first stage estimates the conditional density of the treatment variable given observable firm covariates or GPS. In our case, grants are officially capped at TND 100,000. The distribution of treated firms across grant amounts is shown in Figure 2.[38]

[36] Görg, Henry and Strobl (2008) use information on grant amounts per plant in their study of the effect of subsidies on exports in Ireland. However, their approach suffers from a caveat in that they group grant amounts into a small number of categories instead of using a GPS methodology with data on continuous grant amounts as we do.
[37] We follow Helmut Fryges' treatment of the GPS methodology.
[38] Out of the 345 treated firms for which information on grants disbursed is available, 5 grants obtained were in excess of this ceiling and are excluded from the GPS estimation.

Figure 2. Distribution of Disbursed Grant Amounts among Treated Firms



Note: the density is based on 345 FAMEX beneficiaries for which disbursed grant amounts are available.

In what follows, we consider the treatment variable to be distributed between zero and TND 100,000. We express treatment "dosage" or "intensity" as a continuous variable defined on the unit interval $D_i(\beta) = z_i / 100{,}000$ where z_i is the size of the grant received by firm *i* in Tunisian Dinars. The distribution of the treatment intensity over the entire sample (treatment and control) has a large mass at zero corresponding to the control group's observations and cannot be considered normal even after algebraic transformation (Bia and Mattei, 2008). Accordingly, we assume that the treatment intensity follows a logistic distribution and we estimate a special case of the fractional-logit model of Papke and Wooldridge (1996), which consists in maximizing the Bernoulli likelihood function:

$$L_i(\beta) = \left[\Lambda\left(\overline{X}_i\beta\right)\right]^{Di}\left[1 - \Lambda\left(\overline{X}_i\beta\right)\right]^{1-D_i} \tag{10}$$

where $\Lambda(.)$ is the logistic distribution's cumulative distribution function, i.e., $\Lambda\left(\overline{X}_i\beta\right) = \exp\left(\overline{X}_i\beta\right) / \left[1 + \exp\left(\overline{X}_i\beta\right)\right]$ and $\overline{X}_i$ is a vector of firm covariates. The estimation of Eq. (15) yields parameter estimates $\hat{\beta}$, which are used to construct, for each observation, the estimated conditional density at the treatment intensity actually received, i.e., the GPS or continuous-treatment equivalent of the estimated propensity score:[39]

$$\hat{L}_i = L_i\left(\hat{\beta}\right) = \left[\Lambda\left(\overline{X}_i\hat{\beta}\right)\right]^{Di}\left[1 - \Lambda\left(\overline{X}_i\hat{\beta}\right)\right]^{1-D_i} \tag{10}$$

[39] Eq. (15) is estimated using the generalized linear models framework of McCullagh and Nelder (1989).

As in our earlier case of a binary treatment propensity score, proceeding this way eliminates selection bias in the estimated treatment effect due to differences in pre-treatment covariates at different treatment intensities (Hirano and Imbens, 2004).

In the second stage, we estimate the conditional means of an outcome variable (say, total export growth) as a function of the treatment intensity and the estimated GPS based on a flexible linear specification. Following Hirano and Imbens (2004), we use OLS to estimate the coefficients on a second-degree polynomial in the treatment intensity and the estimated GPS given by:

$$E\left(\Delta y_i / D_i, \hat{L}_i\right) = \alpha_0 + \alpha_1 D_i + \alpha_2 D_i^{\,2} + \alpha_3 \hat{L}_i + \alpha_4 \hat{L}_i^{\,2} + \alpha_5 D_i \hat{L}_i \qquad (10)$$

The third stage consists of estimating and plotting a "dose-response" function mapping each treatment intensity into expected outcome realizations.[40] Following Newey (1994) and Bia, Flores and Mattei (2011), the dose-response function is constructed by averaging the conditional means in Eq. (14) over the distribution of the estimated GPS for each treatment intensity *d* as in:

$$\varphi(d) = \frac{1}{N} \sum_i \left[\alpha_0 + \alpha_1 d + \alpha_2 d^2 + \alpha_3 \hat{L}_i(d) + \alpha_4 \hat{L}_i(d)^2 + \alpha_5 d \hat{L}_i(d)\right] \qquad (16)$$

where N is the total sample size. This is a highly non-linear function of d; indeed, one of the advantages of this methodology that it does not impose any restriction on the relationship between the continuous treatment intensity and the outcome (Fryges and Wagner, 2008). In practice, Eq. (16) is estimated numerically for each centile of the distribution of d in the $[0,1]$ interval. Confidence intervals for the dose-response functions are calculated by bootstrapping, re-estimating the GPS at each bootstrap to take into account the uncertainty in its estimation. Since the procedure controls for differences in pre-treatment variables, the estimated difference between the average potential outcome at two different treatment intensities—each based on Eq. (15)—is called a "pairwise treatment effect" and can be interpreted as the effect of varying treatment intensity.

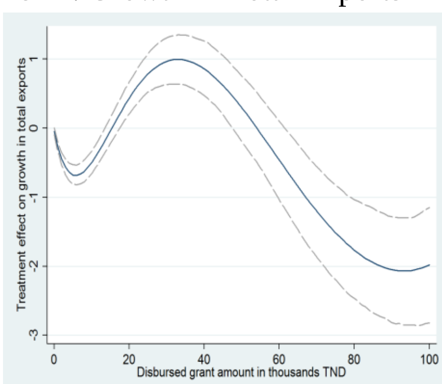
The dose-response functions for the three export-related outcomes are shown in Figure 3 together with 90% confidence intervals. We focus on the impact in the year of treatment (as in column (1b) of Table 4). In each of the panels note that the horizontal axis measures treatment

[40] Hirano and Imbens (2004) argue that the regression function in Eq. (14) does not have a causal interpretation. It represents the average potential outcome for the strata defined by the GPS (the conditional density of receiving a particular treatment level) but does not allow us to compare directly outcome values for different treatment levels and obtain the causal difference in outcomes from receiving a treatment level versus another (Heinrich, Maffioli, and Vazquez, 2010). Hence, there is a need for the third stage in the GPS methodology.

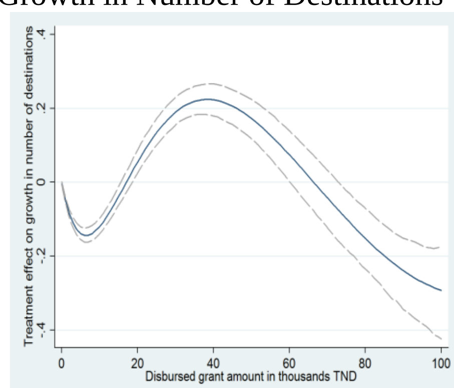
intensity centiles—*not* centiles of the distribution of firms by grant amount—so they can be read directly as grant amounts in thousands of TND. Also note that the shape of the function near the upper bound of the treatment should be interpreted with caution, as relatively few observations are available in that region. Be that as it may, Panel A shows that for total export growth diminishing returns seem to set in at around TND 38,000 (USD 26,000), i.e. well before the grant ceiling, and treatment effects are positive and significant only between about TND 18,000 (USD 12,300) and TND 56,000 (USD 38,200). The shapes of the dose-response functions using destination growth and product growth shown in Panels B and C have similar shapes, although the estimated effects at the peak are smaller (in the vicinity of 20% for both destinations and products, against 100% for total export growth, which is consistent with the binary-treatment results of Table 4). In all cases, grants below TND 180,000 seem to serve little purpose.

Figure 3. Dose-Response Functions: Growth in Export-Related Outcomes as a Function of Grant Size

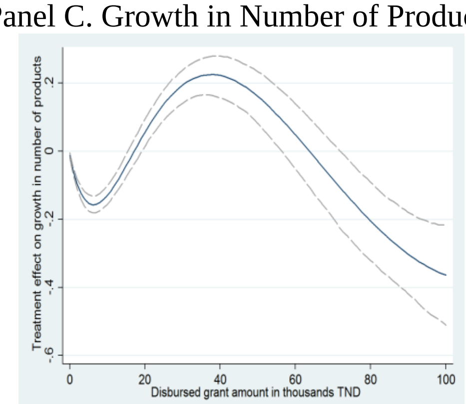
Panel A. Growth in Total Exports



Panel B. Growth in Number of Destinations



Panel C. Growth in Number of Products



Notes: in each of the panels the solid lines show the estimated conditional expectation of firms' one-year growth in an export-related outcome (for FAMEX firms between the year before receiving support and the first year when support is received) given the FAMEX grant amount and the estimated GPS. The dashed lines represent the 90% confidence intervals based on the bootstrap distribution.

9.2 Heterogeneity Results

Ravallion (2009, p. 37) argues that practitioners should "never be happy with an evaluation that assumes common (homogeneous) impact". Given the detailed information available on the FAMEX program, we can exploit two dimensions potentially affecting the treatment effect: the objective of the individual project that FAMEX supported and the specific activities supported by the FAMEX grant. To our knowledge, this type of analysis is novel in the export promotion literature. Moreover, we also consider heterogeneity across firm characteristics namely size and the presence of an in-house export unit.

Tunisian firms had to state an objective when applying for FAMEX assistance, whether they wanted to: (i) become a significant exporter, (ii) export to a new destination, or (iii) export a new product. Given the way the FAMEX application packages were structured, firms could state only *one* of these three objectives. Hence the project objectives partition the 401 treated firms into three non-overlapping groups: 95 came to FAMEX to start exporting, 194 came to FAMEX to export to a new market, and 112 came to FAMEX to export a new product. We re-estimate Eq. (7) by WLS with the dependent variable defined by Eq. (9), allowing the treatment effect to differ across objectives. The results indicate that the largest treatment effects on all outcome variables are generally experienced by FAMEX firms whose objective was to export to new markets, while the smallest treatment effects are experienced often by firms whose objective was to export new products (see Appendix Table D.1). These findings are somewhat surprising as one would expect stronger effects on total export growth for firms wanting to start exporting, on destination growth for firms looking for new markets, and on product growth for firms wanting to export new products.

The FAMEX program financed the various types of activities listed in Table 1. To analyze the effect of the different activities, we re-estimate Eq. (7) by WLS with the dependent variable defined by Eq. (9), replacing 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.[41] The results show that market prospection activities and promotion activities have a significant effect on all outcome variables up to four years after treatment, with both activities exhibiting similar marginal returns on the dinar (see Appendix Table D.2). Firm development has a significant positive effect on all outcomes but only four or five years after treatment, which may be due to the longer gestation period needed for such activities bring export benefits. By contrast, the other two types of activities have insignificant returns. A rationale for the lack of effects of product development activities could be that the FAMEX administration was bad at identifying the right partners for product development and at advising on organizational issues, which require management experience that bureaucrats typically lack.

We also examine whether firm size affects the magnitude of the impact of FAMEX assistance. To do so we classify firms into two broad size categories: below 50 workers (196 firms) and above 50 workers (205 firms). We re-estimate Eq. (7) by WLS with the dependent variable defined by Eq. (9), allowing the FAMEX effect to differ across size categories. The estimates suggest that FAMEX has a strong effect in the year of treatment for both small and large firms, and the magnitudes of the effects are quite similar across size categories for all export outcomes (see Appendix Table D.3).

A large number of FAMEX firms - 200 - had an in-house export unit prior to the start of FAMEX assistance. It is natural to expect such firms to exhibit stronger performance since their in-house dedicated export units may enable them to make better use of the FAMEX assistance. To address this possibility we re-estimate Eq. (7) by WLS with the dependent variable defined by Eq. (9), allowing 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, relative to FAMEX beneficiaries without such an export unit, suggesting that prior stronger export orientation by a firm revealed by the set-up of a dedicated export unit helps to benefit more from

[41] 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. Note that for each activity the amount entering in the WLS regressions is the amount co-financed at 50% by FAMEX. The total amount spent by the firm in that activity is actually twice as large. Also, note that in this exercise we control for selection into the program through the usual propensity score matching weighting scheme, but we do not control for selection into particular levels of support for each activity.

the program (see Appendix Table D.3). Oddly, the estimates also show lower growth in the number of products exported for FAMEX firms with an in-house export unit.

9.3. Economic Magnitude of the FAMEX effects

Our baseline results suggest that the benefits of the FAMEX program were a positive - though relatively short-lived - effect on total exports of treated firms. But to address the question of whether the FAMEX program was worth it, we also need to take into account the cost side and provide an indication of the economic benefits of FAMEX. Next, we describe a simple cost-benefit calculation used to estimate the rate of return of the FAMEX program *per firm* laying out clearly at each step the assumptions made.[42] We first consider the benefits from the FAMEX program 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 a 66.7% higher export growth than control firms. Since the average annual total export growth for control firms in the 2004-2008 period was 8.35%, the estimated annual total export growth for a FAMEX beneficiary in the year of treatment is 13.9%.[43] What does this number imply in terms of additional total exports? Given average total exports per firm in 2004 (prior to FAMEX) of 2308 thousand TND, the growth rates above imply that, in the year of treatment, exports for a typical FAMEX beneficiary would have increased to 2629 thousand TND while exports for a typical control firm would have increased to 2501 thousand TND. Thus, the implied difference in total exports due to FAMEX would be of 128.5 thousand TND. The average grant amount disbursed by the FAMEX program per firm was 21.7 thousand TND and given the matching grant nature this implies that the average total cost of an export business plan was twice that amount, i.e., 43.4 thousand TND. Overall, for a 21.7 thousand TND typical FAMEX grant, a beneficiary firm generated 128.5 additional exports, which suggests a 5.9 to 1 ratio. Cost-benefit calculations for longer time differences are also shown in Appendix D. While the rates of return in the first three years after the FAMEX treatment appear quite high, it is important to note that they are an upper bound since they are made based on the grant-component cost of FAMEX but do not take into account the overhead administrative costs of the FAMEX program for which we have no

[42] The details of the rate of return calculations are shown in Appendix D.
[43] We pick average growth over the 2004-2008 instead of the 2004-2010 period to avoid the global financial crisis years post-2008. This number is obtained as the product of 8.35% and (1+66.7%).

information. Appendix D also plots the total export levels for a typical FAMEX beneficiary and a typical control firm over time after the FAMEX intervention.

10. Concluding Remarks

Our study estimates a stronger impact of the export promotion program –FAMEX– at the extensive margin than at the intensive margin in terms of markets and products. The effect on total export growth rates for program beneficiaries is short-lived. However, our results on the long-term impact of export promotion must be interpreted cautiously given that the later years of our sample period are special in that they were characterized by the collapse of world trade, which may not have affected all firms equally. In particular treated firms may have ventured into riskier markets. Our evidence also shows a benefit from FAMEX in terms of survival in destination markets and a clear increase in diversification in terms of markets and products for treated firms. However, this increased diversification did not result in reduced export volatility.

In terms of policy implications, the quasi-experimental approach suffers from a fundamental drawback, namely, that treatment effects, which are generally construed as favorable to policy intervention when they are significant, only indicate appropriable benefits. They give no indication on the presence of a market failure. Indeed, if anything, it is the absence of treatment effect that is consistent with the non-appropriability of benefits. We attempt to go around this problem by testing directly for the presence of externalities. We show no evidence of externalities from treated firms to control firms, which is very different from the strong positive externalities that could justify government intervention. Thus, even though we do find significant treatment effects, the policy implications of our findings are not unambiguously favorable to public funding of export promotion.

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Appendix

Appendix A: Propensity Score Estimation

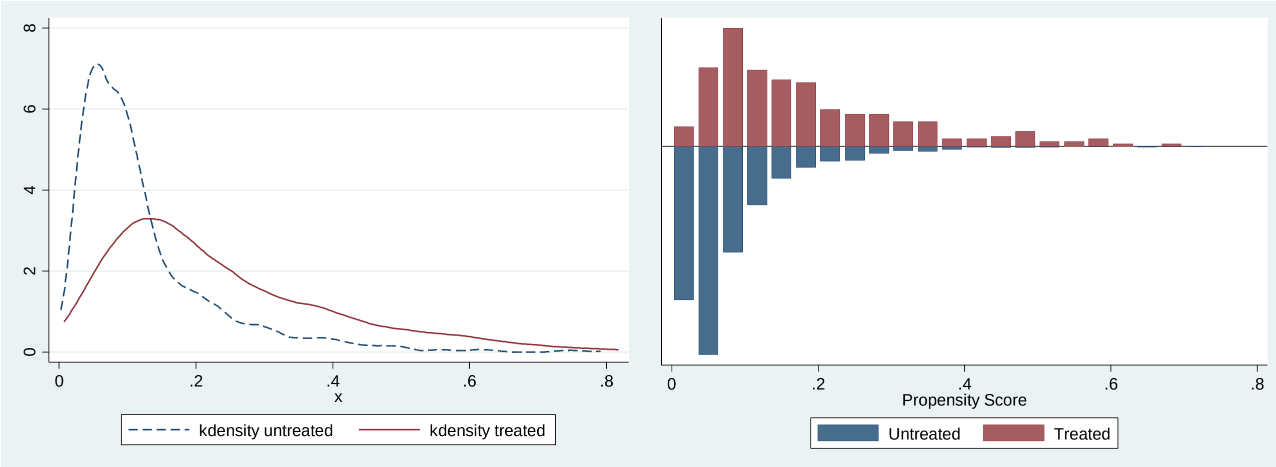
Table A.1 Probit Regression for the Propensity to receive FAMEX treatment

	FAMEX treatment status
Age	0.355
	(0.354)
Age squared	-0.098
	(0.065)
Total exports in 2004	-0.038
	(0.009)***
Number of exported products in 2004	0.158
	(0.046)***
Number of export destinations in 2004	0.497
	(0.058)***
100% exporter	-0.341
	(0.070)***
10-19 employees	-0.491
	(0.116)***
20-49 employees	-0.359
	(0.099)***
50-99 employees	-0.393
	(0.106)***
100-199 employees	-0.385
	(0.113)***
More than 200 employees	-0.411
	(0.122)***
Textiles and apparels	-0.067
	(0.088)
Paper, wood, and furniture	0.019
	(0.102)
Chemicals	-0.041
	(0.100)
Metals	-0.021
	(0.116)
Machine and equipment	0.017
	(0.099)
Electric	-0.111
	(0.122)
Grand Tunis	-0.352
	(0.072)***
Central Sea	-0.950
	(0.157)***
Rest of Tunisia	-0.447
	(0.077)***
Year fixed effects	Yes
Observations	12,263

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

Appendix B: Propensity Score Matching

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



Source: Authors' calculations.

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

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

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

The fourth test is based on the comparison of the pseudo-R-squared of the propensity score estimated on the full sample versus on the matched sample, which explains how well the covariates explain the propensity to participate in the program. With a high quality matching, the pseudo-R-squared should be very low after matching because there should be no differences in the distribution of the covariates that can explain the propensity to participate in the program. Indeed, our pseudo-R-squared is 0.086 before matching and 0.003 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 316.85 with a p-value of 0 before matching and 3.64 with a p-value of 1 after matching.

Table B.1 Balancing Tests

Covariates	Mean in Matched Sample Treatment	Control	T-test T-statistic	P-value	Percentage Bias	Percentage Bias Reduction
Age	2.720	2.709	0.26	0.796	1.7	86.3
Age squared	7.716	7.663	0.24	0.809	1.7	83.9
Total exports in 2004	10.343	10.086	0.63	0.531	4.3	77.3
Number of exported products in 2004	1.322	1.276	0.69	0.492	5.1	84.6
Number of export destinations in 2004	1.151	1.087	1.08	0.28	8.6	83.5
100% exporter	1.340	1.362	-0.65	0.516	-4.5	84.5
10-19 employees	0.095	0.101	-0.29	0.771	-1.9	82.6
20-49 employees	0.288	0.285	0.08	0.936	0.6	80.5
50-99 employees	0.193	0.204	-0.39	0.694	-2.7	51.1
100-199 employees	0.163	0.158	0.19	0.851	1.3	-206.2
More than 200 employees	0.158	0.150	0.28	0.777	2.1	84.1
Textiles and apparels	0.315	0.330	-0.46	0.645	-3.2	85.5
Paper, wood, and furniture	0.128	0.119	0.36	0.719	2.7	73.7
Chemicals	0.125	0.132	-0.29	0.771	-2.1	27.2
Metals	0.083	0.079	0.19	0.851	1.4	81.4
Machine and equipment	0.138	0.132	0.21	0.835	1.5	83.4
Electric	0.065	0.064	0.06	0.953	0.4	89.3
Grand Tunis	0.475	0.476	-0.02	0.987	-0.1	97.6
Central Sea	0.020	0.031	-1	0.316	-5.3	80.2
Rest of Tunisia	0.283	0.287	-0.14	0.887	-1	94.6

Appendix C: Robustness of Main Results and of Spillover Results

Table C.1 FAMEX Effects for Firms Treated in 2005

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

Difference	TY-(TY-1)	TY-(TY-1)	(TY+1)-TY	(TY+2)-(TY+1)	(TY+3)-(TY+2)	(TY+4)-(TY+3)	(TY+5)-(TY+4)
Estimator	PSM-DID (1a)	WLS reg. (1b)	WLS reg. (2)	WLS reg. (3)	WLS reg. (4)	WLS reg. (5)	WLS reg. (6)
Outcome							
Total exports	0.439	0.485**	0.360*	-0.026	-0.17	-0.264	0.025
	(1.67)*	[2.32]	[1.77]	[-0.12]	[-0.81]	[-1.13]	(0.11)
R-squared		*0.16*	*0.152*	*0.112*	*0.088*	*0.113*	*0.11*
Nb. destinations	0.113	0.156***	0.092***	0.059*	0.022	0.044	0.059
	(4.33)**	[4.81]	[2.93]	[1.70]	[0.68]	[1.24]	(2.07)**
R-squared		*0.142*	*0.117*	*0.077*	*0.134*	*0.143*	*0.08*
Nb. products	0.11	0.156***	0.071	0.057	0.003	0.069*	0.097
	(5.59)***	[3.72]	[1.60]	[1.37]	[0.08]	[1.69]	(2.58)***
R-squared		*0.162*	*0.169*	*0.114*	*0.11*	*0.134*	*0.13*
Observations		2,524	2,553	2,579	2,606	2,606	2,607

Panel B. Cumulative Effects of FAMEX on Export Outcomes

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg. (1)	WLS reg. (2)	WLS reg. (3)	WLS reg. (4)	WLS reg. (5)	WLS reg. (6)
Outcome						
Total exports	0.485**	0.838***	0.636**	0.386	0.023	0.2
	[2.32]	[3.40]	[2.29]	[1.22]	[0.06]	[0.52]
R-squared	*0.16*	*0.213*	*0.209*	*0.209*	*0.222*	*0.246*
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	*0.142*	*0.191*	*0.197*	*0.257*	*0.298*	*0.303*
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	*0.162*	*0.21*	*0.218*	*0.235*	*0.263*	*0.299*
Observations	2,524	2,502	2,502	2,524	2,524	2,524

Notes: T-statistics based on robust standard errors in parentheses; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated firms in 2005 and control firms in the common support. The PSM-DID estimates are estimated based on propensity scores obtained using kernel matching. 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, firm initial exports, number of exported products and number of destinations served.

Table C.2 Effects of FAMEX on Export Outcomes using Year-by-Year Matching

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

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

Table C.3 Effects of FAMEX on Export Outcomes – Alternatives for Dropouts

Panel A. Excluding Dropouts from the Sample

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.548	0.725	0.517	0.098	-0.138	0.033
	(3.26)***	(3.59)***	(2.32)**	(0.36)	(-0.40)	(0.08)
Nb. destinations	0.154	0.186	0.179	0.113	0.108	0.141
	(6.18)***	(6.73)***	(5.54)***	(3.15)***	(2.30)**	(2.60)***
Nb. products	0.150	0.175	0.167	0.079	0.120	0.176
	(4.74)***	(4.68)***	(4.14)***	(1.69)*	(2.02)**	(2.67)***
Observations	11,645	11,645	9,423	7,160	4,846	2,537

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	0.590	0.273	0.091	-0.238	-0.007
	(3.32)***	(3.15)***	(1.32)	(0.37)	(-0.75)	(-0.20)
Nb. destinations	0.139	0.143	0.140	0.091	0.090	0.134
	(6.15)***	(5.63)***	(4.81)***	(2.80)***	(2.07)**	(2.70)***
Nb. products	0.116	0.140	0.124	0.069	0.088	0.173
	(4.17)***	(4.31)***	(3.46)***	(1.67)*	(1.63)	(2.88)***
Observations	11,950	11,950	9,719	7,421	5,038	2,656

Notes: T-statistics based on robust standard errors in parentheses;* 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, firm initial exports, number of exported products and number of destinations served. In Panel A the sample includes 401 treated firms and 2220 control firms while in Panel B the sample includes 526 treated firms and 2220 control firms.

Table C.4 Further Effects of Exposure to FAMEX Firms on Export Outcomes

Panel A. Spillovers for Sample of All Firms

Estimator	Firm Fixed Effects (Within)				Firm Fixed Effects (Within)				Firm Fixed Effects (Within)			
Outcome	First diff. of total exports				First diff. of nb. destinations				for sample of control 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	Firm Fixed Effects (Within)				Firm Fixed Effects (Within)				Firm Fixed Effects (Within)			
Outcome	First diff. of total exports				First diff. of nb. destinations				for sample of control firms			
	(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 parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. In Panel A, the sample includes all firms, both FAMEX firms and control firms in the common support or outside. In Panel B, the sample includes only control firms inside the common support or outside that are in sector-location cells where FAMEX firms that required assistance with the objective of increasing their destinations or their exported products were present.

Appendix D: Extensions

Table D.1 Effect of Treatment Interacted with Project Objective

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Objective						
	Start exporting	0.467	0.867	0.497	-0.247	-1.842	-0.93
		(1.15)	(1.62)	(0.94)	(-0.39)	(-2.18)**	(-1.06)
Total exports	New destinations	0.563	0.502	0.148	-0.026	0.01	0.133
		(2.48)**	(1.97)**	(0.48)	(-0.07)	(0.02)	(0.29)
	New products	0.184	0.645	0.734	0.855	0.739	0.46
		(0.78)	(2.28)**	(2.48)**	(2.20)**	(1.39)	(0.73)
	R-squared	*0.17*	*0.22*	*0.23*	*0.22*	*0.24*	*0.25*
Outcome	Objective						
	Start exporting	0.144	0.183	0.165	0.068	-0.097	0.024
		(2.66)***	(2.69)***	(2.33)**	(0.88)	(-0.94)	(0.21)
Nb. Destinations	New destinations	0.171	0.186	0.165	0.155	0.144	0.189
		(4.95)***	(5.11)***	(3.77)***	(3.21)***	(2.44)**	(2.83)***
	New products	0.085	0.164	0.16	0.085	0.171	0.119
		(2.42)**	(3.94)***	(3.31)***	(1.52)	(2.38)**	(1.45)
	R-squared	*0.16*	*0.20*	*0.20*	*0.23*	*0.28*	*0.29*
Outcome	Objective						
	Start exporting	0.130	0.220	0.236	0.07	-0.051	0.019
		(1.89)*	(2.54)**	(2.66)***	(0.73)	(-0.42)	(0.16)
Nb. Products	New destinations	0.156	0.144	0.078	0.075	0.127	0.173
		(3.43)***	(3.00)***	(1.45)	(1.22)	(1.81)*	(2.23)**
	New products	0.082	0.148	0.203	0.137	0.189	0.246
		(1.72)*	(2.35)**	(3.17)***	(1.78)*	(1.83)*	(2.11)**
	R-squared	*0.15*	*0.19*	*0.23*	*0.25*	*0.26*	*0.29*
	Observations	12,263	12,263	9,915	7,526	5,087	2,656

Notes: T-statistics based on robust standard errors in parentheses;* 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, firm initial exports, number of exported products and number of destinations served.

Table D.2 Effects of FAMEX Program Components

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Activity (amounts in TND)						
Total exports	Market prospection	0.039	0.045	0.072	0.098	0.018	0.024
		(2.03)**	(1.79)*	(2.57)**	(2.97)***	(0.37)	(0.47)
	Promotion	0.028	0.039	0.03	0.008	0.029	0.005
		(3.06)***	(3.08)***	(2.38)**	(0.49)	(1.35)	(0.16)
	Product development	-0.014	-0.013	0.003	-0.044	-0.023	-0.024
		(-0.96)	(-0.55)	(0.21)	(-1.41)	(-0.69)	(-0.67)
	Firm development	-0.022	0.004	-0.002	0.042	0.081***	0.091**
		(1.12)	(0.20)	(-0.07)	(1.64)	(2.66)	-2.15
	Foreign subs. creation	-0.003	0.025*	-0.017	-0.005	-0.043	-0.057*
		(-0.15)	(1.86)	(-0.59)	(-0.18)	(-1.47)	(-1.73)
	R-squared	*0.21*	*0.257*	*0.265*	*0.244*	*0.252*	*0.263*
Outcome	Activity (amounts in TND)						
Nb. Destinations	Market prospection	0.007	0.012	0.012	0.009	0.01	0.008
		(2.07)**	(3.24)***	(2.98)***	(1.79)*	(1.55)	(1.17)
	Promotion	0.006	0.006	0.011	0.006	0.003	0.005
		(3.20)***	(3.67)***	(4.95)***	(2.55)**	(0.70)	(0.96)
	Product development	0.000	-0.001	0.003	-0.003	-0.003	-0.002
		(0.06)	(-0.23)	(1.00)	(-0.75)	(-0.69)	(-0.46)
	Firm development	0.001	0.004	0.007	0.009	0.015	0.018
		(0.27)	(1.02)	(1.36)	(1.46)	(2.77)***	(2.56)**
	Foreign subs. creation	-0.000	0.001	-0.004	-0.004	0.000	-0.008
		(-0.00)	(0.63)	(-0.97)	(-1.07)	(-0.06)	(-1.35)
	R-squared	*0.17*	*0.211*	*0.235*	*0.239*	*0.295*	*0.301*
Outcome	Activity (amounts in TND)						
Nb. Products	Market prospection	0.009	0.009*	0.015	0.013	0.012	0.011
		(2.05)**	(1.70)*	(3.00)***	(2.25)**	(1.39)	(1.22)
	Promotion	0.004	0.006**	0.005	0.003	0.002	0.000
		(1.11)	(2.07)	(1.56)	(0.77)	(0.62)	(0.03)
	Product development	-0.003	0.004	0.005	-0.002	0.001	0.006
		(-1.39)	(0.93)	(1.19)	(-0.43)	(0.23)	(1.03)
	Firm development	-0.002	0.000	0.002	0.012	0.023	0.022
		(-0.41)	(0.02)	(0.42)	(1.66)*	(3.21)***	(2.43)**
	Foreign subs. creation	0.002	0.004	-0.003	-0.006	-0.005	-0.008
		(0.49)	(1.10)	(-0.64)	(-1.21)	(-0.68)	(-1.20)
	R-squared	*0.16*	*0.215*	*0.251*	*0.257*	*0.275*	*0.298*
	Observations	12157	12157	9,841	7,476	5,056	2,628

Notes: T-statistics based on robust standard errors in parentheses;* 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, firm initial exports, number of exported products and number of destinations served.

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

Panel A. Firm Size

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Firm size						
Total exports	Less than 50 workers	0.512	0.987	0.563	0.138	-0.025	0.343
		(1.84)*	(3.08)***	(1.61)	(0.35)	(-0.05)	(0.62)
	More than 50 workers	0.511	0.44	0.439	0.102	-0.183	-0.236
		(2.82)***	(1.82)*	(1.62)	(0.29)	(-0.40)	(-0.46)
	R-squared	*0.17*	*0.23*	*0.23*	*0.22*	*0.23*	*0.25*
Outcome	Firm size						
Nb. Destinations	Less than 50 workers	0.151	0.215	0.162	0.102	0.055	0.125
		(3.98)***	(5.23)***	(3.41)***	(2.02)**	(0.86)	(1.76)*
	More than 50 workers	0.149	0.161	0.193***	0.141	0.159	0.153
		(4.69)***	(4.42)***	(4.54)	(2.83)***	(2.42)**	(2.02)**
	R-squared	*0.15*	*0.19*	*0.20*	*0.23*	*0.27*	*0.29*
Outcome	Firm size						
Nb. Products	Less than 50 workers	0.179	0.21	0.188	0.086	0.108	0.112
		(3.58)***	(3.76)***	(3.14)***	(1.31)	(1.31)	(1.29)
	More than 50 workers	0.115	0.134	0.139	0.076	0.127	0.212
		(2.97)***	(2.73)***	(2.62)***	(1.19)	(1.60)	(2.40)**
	R-squared	*0.15*	*0.19*	*0.23*	*0.25*	*0.25*	*0.28*
	Observations	12,263	12,263	9,915	7,526	5,087	2,656

Panel B. In-House Export Unit

	Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
	Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
		(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Export Unit Status						
Total exports	Has in-house export unit	0.532	0.880	0.694	0.404	-0.063	-0.022
		(2.94)***	(3.92)***	(2.72)***	(1.16)	(-0.14)	(-0.04)
	No in-house export unit	0.491	0.536	0.306	-0.159	-0.167	0.041
		(1.93)*	(1.72)*	(0.91)	(-0.42)	(-0.37)	(0.08)
	R-squared	*0.17*	*0.23*	*0.23*	*0.22*	*0.23*	*0.25*
Outcome	Objective						
Nb. Destinations	Has in-house export unit	0.162	0.221	0.236	0.179	0.166	0.186
		(4.96)***	(6.25)***	(5.77)***	(3.61)***	(2.59)***	(2.59)*
	No in-house export unit	0.138	0.154	0.121	0.066	0.054	0.091
		(3.95)***	(3.88)***	(2.68)***	(1.40)	(0.92)	(1.36)
	R-squared	*0.15*	*0.20*	*0.20*	*0.23*	*0.28*	*0.29*
Outcome	Objective						
Nb. Products	Has in-house export unit	0.134	0.197	0.178	0.097	0.172	0.239
		(3.42)***	(4.07)***	(3.47)***	(1.57)	(2.15)**	(2.71)***
	No in-house export unit	0.159	0.145	0.148	0.065	0.06	0.093
		(3.47)***	(2.77)***	(2.62)***	(1.04)	(0.81)	(1.16)
	R-squared	*0.15*	*0.19*	*0.23*	*0.25*	*0.25*	*0.29*
	Observations	12,263	12,263	9,915	7,526	5,087	2,656

Notes: T-statistics based on robust standard errors in parentheses;* 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, firm initial exports, number of exported products and number of destinations served.

Appendix E: Economic Magnitude of FAMEX Effects

Table E.1 Rates of Return on the FAMEX Program

		Benefits and costs in year					
		TY	TY+1	TY+2	TY+3	TY+4	TY+5
		1	2	3	4	5	6
A	β coefficient on growth in total exports	0.511	0.707	0.499	0.12	-0.113	0.008
B= Exp(A-1)*100	Change in export growth	66.7	102.8	64.7	12.7	-10.7	0.8
C=1+B/100	In rate of growth terms	1.67	2.03	1.65	1.13	0.89	1.01
D	Average annual growth in total exports for control firms	8.35	17.40	27.20	37.82	49.33	61.80
E=C*D	Annual growth for FAMEX beneficiaires	13.92	35.28	44.80	42.64	44.06	62.29
F=1+D/100	Annual rate of growth for control firms	1.08	1.17	1.27	1.38	1.49	1.62
G=1+E/100	Annual rate of growth for FAMEX beneficiaries	1.14	1.35	1.45	1.43	1.44	1.62
H	Average total exports per firm in 2004	2308	2308	2308	2308	2308	2308
I=H*F	Total exports for typical control firm	2501	2710	2936	3181	3447	3734
J=H*G	Total exports for typical FAMEX beneficiary	2629	3122	3342	3292	3325	3746
K=J-I	Difference in total exports due to FAMEX	128.5	412.7	406.2	111.3	-121.6	11.5
L	Average FAMEX disbursed amount per firm	21.7	21.7	21.7	21.7	21.7	21.7
M=K/L	Return on public investment	5.92	19.02	18.72	5.13	-5.61	0.53
N= L*2	Average total cost of export business plan per firm	43.4	43.4	43.4	43.4	43.4	43.4
O=K/N	Return on total investment	2.96	9.51	9.36	2.56	-2.80	0.26

Note: In row D, growth shown in column 2 is obtained as growth in column 1 (8.35) multiplied by growth in column 1 row F divided by the number of years since TY-1 (2 years).

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