What Do Market-Access Subsidies Do? Experimental Evidence from Tunisia*

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Abstract

Many countries seek to promote exports by subsidizing market access, but evidence on such efforts has been mixed. We present the first randomized evaluation of a government financial-support program explicitly targeting exports, the Tasdir+ program in Tunisia. The program offered matching grants for fixed market-access costs but not variable costs. Tracking outcomes in administrative data, we find positive effects on exports on average. We find limited impacts on the numbers of destinations or exported products, which were stated policy targets. The finding that fixed-cost subsidies significantly expanded exports on the intensive margin but not the extensive margins of destinations or products stands in contrast to the predictions of several workhorse trade models.

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1 Introduction

There is growing evidence that exporting has salutary effects on firms, especially in developing countries. It can improve product quality (Verhoogen, 2008; Atkin et al., 2017; Bastos et al., 2018; Hansman et al., 2020; Demir et al., 2024), raise productivity (De Loecker, 2007; Atkin et al., 2017; Garcia-Marin and Voigtländer, 2019), induce technology adoption (Lileeva and Trefler, 2010; Bustos, 2011), increase wages (Verhoogen, 2008; Brambilla et al., 2012; Frías et al., 2024), and improve working conditions (Tanaka, 2020), among other outcomes.

It is less clear whether government interventions to promote exports are effective. Governments dedicate substantial resources to various strategies to facilitate market access, including tax rebates, duty drawbacks, foreign-country trade missions, high-level delegation visits, and direct subsidies. Between 2009 and 2020, more than 2,500 export-related measures and nearly 500 initiatives providing financial assistance in foreign markets were implemented globally (Juhász et al., 2023b). But the evidence on such interventions, reviewed briefly below, is mixed. Moreover, the evaluations to date have almost uniformly relied on non-experimental methods such as matching and difference-in-difference estimators, which are subject to concerns about unobserved differences between beneficiary and non-beneficiary firms.

This paper presents the first randomized evaluation of a government financial-support program explicitly targeting exports, focusing on the Tasdir+ program in Tunisia. The program provided matching grants to offset the costs of accessing export markets. The stated goals of the program were to increase the scale of exports and to diversify exports toward higher-value-added products and new markets. A distinctive aspect of the program is that the grants were limited to fixed costs (i.e. costs that did not depend on the number of units produced) such as marketing expenditures, participation in trade fairs, and establishment of offices abroad. Variable costs such as salaries, materials, and transport costs were not eligible for reimbursement. The program thus offers an opportunity to study the effects of reductions specifically in fixed costs of export market access, which are commonly thought to be crucial for firms' entry into new destinations and products.

To help organize our thinking about the effects of market-access subsidies, we present a simple model of product-scope and market-penetration decisions by heterogeneous,

multi-product firms, along the lines of one version of the model of Arkolakis et al. (2021), which incorporates market-penetration costs (as in Arkolakis (2010)) and endogenous product scope (as in Eckel and Neary (2010), Bernard et al. (2011), and Mayer et al. (2014)) into a Melitz (2003)-type framework. The model generates two key implications: first, product-scope and destination-entry decisions may be fairly insensitive to changes in fixed market-access costs over some ranges; and second, even without extensive-margin responses, sales within existing destinations and products are expected to respond to such changes.

Eligibility for the matching grants was randomized in five application rounds in 2018-2019. Pooling rounds, the randomization sample included 487 firms. The grants offered reimbursement of 50% of eligible expenditures incurred within one year as part of an approved business plan, with a cap of USD 50,000 (TND 150,000) per firm. The budget for the randomized grants was approximately USD 14 million (TND 42 million).

We are able to track firms in data from several sources. We observe sales and exports from corporate tax records and employment and wages from social security records, all of which are collected in the *Repertoire National des Entreprises (RNE)* [National Repertory of Firms], a firm-level database. Importantly, this database includes exports for service firms as well as non-service firms. We also have access to transaction-level customs records, as well as administrative records from the Tasdir+ program. In addition, we conducted baseline and endline surveys.

To analyze exports, which contain many zeros, our preferred specification is a simple two-part ANCOVA with separate regressions for exporter status and for log exports among continuing exporters. We also present a Poisson Pseudo Maximum Likelihood (PPML) specification that combines the two margins. Using the linked customs records, we use similar specifications to examine the effects of the program on the numbers of destinations served and products exported. The study period spans the COVID-19 crisis, which had a major impact in Tunisia as elsewhere. We focus primarily on outcomes in 2021, the most recent year we observe in the RNE data.

We find that the program had a positive, statistically and economically significant impact on exports. The estimates range from a 27% increase in the level of exports in the PPML specification to a 39 log point (48%) increase among continuing exporters in the ANCOVA specification. These estimates are large, but not out of line with the existing literature, as discussed below. We find positive point estimates on the extensive margin of being

an exporter, but these are not statistically significant. Using the customs records, we find little evidence that the numbers of destinations or products changed differentially between the treatment and control groups. Overall, it appears that the program achieved the goal of increasing the scale of exports, but had limited success in encouraging firms to expand the range of destinations or products exported.

Using the rich combination of datasets we have collected, we examine the mechanisms through which the program had these effects on exports. From our surveys, we find the strongest program impacts on actions to establish contracts with agents and/or distributors and to establish a foreign presence through affiliates or representatives abroad. Possibly related to this increase in foreign presence, we find that firms were more likely to become importers, although we see little increase in the value of imports conditional on being a continuing importer. Perhaps surprisingly, we do not see significant increases in employment, earnings per employee, or wage bills of permanent employees in response to the program, possibly due to binding collective bargaining agreements; we discuss below.

We also examine heterogeneity in the effects of the program by pre-program characteristics. Perhaps the most salient dimension is between "totally exporting" firms — which administratively are classified in a free trade zone, outside of the Tunisian customs area — and "non-totally exporting" firms. The increase in exports we find is driven entirely by the non-totally exporting firms. We also find significantly greater treatment effects among firms with at least one quality certification at baseline, consistent with the idea that the ability to upgrade quality is an important determinant of export success (Verhoogen, 2023). There is suggestive evidence that the benefits of the program were greater for smaller and more credit-constrained firms, although these estimates are not significant in all specifications.

Although the reduced-form patterns are broadly consistent with the main predictions of our model, a question remains about whether the theoretical mechanism we highlight can plausibly explain the estimated magnitudes, both the large intensive-margin response and the small extensive-margin responses. To address this issue, we conduct a simple back-of-the-envelope calibration exercise using reasonable values for the relevant parameters, some taken from the literature and some inferred from our data. While a full structural estimation is beyond the scope of the paper, this exercise is instructive in that it shows that estimates in the ballpark of the magnitudes we find can

be generated by a quantitatively plausible version of our theoretical model.

Our findings that the subsidy for fixed market-access costs led to statistically significant increases on the intensive margin of export sales but not the extensive margins of destinations or products stand in contrast with the predictions of workhorse trade models in the tradition of Melitz (2003) and multi-product variants such as Bernard et al. (2011). In models of this type, fixed costs typically matter for entry, either into destinations or (when multi-product firms are considered) into products, but not for sales conditional on entry. Our results point to the importance of market-penetration costs such as those modeled by Arkolakis (2010), in which marketing expenditures in a given destination increase the share of consumers reached. More generally, we contribute to a growing literature suggesting that the costs of entering a destination are not well represented by one-time sunk costs or per-period fixed costs of accessing entire destination markets, reviewed by Alessandria et al. (2021).

Beyond the studies cited above, our paper is related to several strands of literature. It is most closely related to two non-experimental studies of an earlier matching-grants-for-exports program in Tunisia known as FAMEX II. Relying on matching methods, Gourdon et al. (2011) find positive effects on exports but no impacts on employment or total sales, and Cadot et al. (2015) find short-term effects on exports, the number of destinations, and the number of products, but no persistence in these effects past two years after the program. Relative to these studies, the key advantage of the current evaluation is that program eligibility was based on a lottery rather than the judgment of program staff. Our results differ both in that we find more persistent effects on exports and that we do not find significant effects on the number of destinations or products.

Our paper speaks to the broader literature on government export-promotion efforts. This literature has also had to rely on non-experimental methods such as difference-in-differences and matching estimators.[1] Among the leading studies, Volpe Martincus and Carballo (2008) find that beneficiaries of Peruvian export-promotion programs over the 2001-2005 period saw increases on the extensive margins of products and destinations but not in total exports. Álvarez and Crespi (2000) find that three export-promotion instruments in Chile (exporter committees, presence in international fairs, and utilization of business information systems) had positive effects on

[1] Volpe Martincus (2010), Van Biesebroeck et al. (2016), and Srhoj et al. (2023) provide overviews of this literature.

technological innovation but not on the number of products exported. Other notable contributions include Bernard and Jensen (2004), Görg et al. (2008), Girma et al. (2009), Lederman et al. (2010), Head and Ries (2010), Volpe Martincus and Carballo (2010a,b,c, 2012), Volpe Martincus et al. (2012), Chandra and Long (2013), Helmers and Trofimenko (2013), Van Biesebroeck et al. (2015, 2016), Broocks and Van Biesebroeck (2017), Munch and Schaur (2018), Srhoj and Walde (2020), Defever et al. (2020, 2025), Chávez et al. (2020), Olarreaga et al. (2020), Girma et al. (2020), Buus et al. (2025a), and Matray et al. (2026).[2] Although a few papers have found significant intensive-margin effects (Görg et al., 2008; Van Biesebroeck et al., 2015; Buus et al., 2025a), the preponderance of evidence suggests that the effects of export-promotion programs are primarily on the extensive margin of destinations.[3] But while the literature has generated many important insights, in the absence of randomization it is difficult to rule out the possibility that the patterns are due to unobserved differences between treated and untreated firms.

We are aware of eight experimental studies related to exporting. In collaboration with an international buyer, Atkin et al. (2017) randomized initial export contracts to Egyptian rug producers and find effects on rug quality and firm productivity, but are not able to speak to the effectiveness of government interventions. Iacovone et al. (forthcoming) mainly focus on a novel method of Bayesian estimation but at the same time evaluate a program that provided consulting about management practices and find little evidence of an impact on exports. Kim et al. (2018) find that informational seminars in Vietnam on average had no effect on export participation. Breinlich et al. (2017) find that informational brochures sent to firms by the UK export-promotion agency perversely made non-exporters more pessimistic about exporting. Cusolito et al. (2023) evaluate a business-training program in the Western Balkans that did not specifically target exports but that had an effect of increasing exports. The contemporaneous projects by Carvalho et al. (2024) in Brazil and Gonzalez et al. (2024) in Argentina are evaluating programs that offer information and/or consulting. Another contemporaneous project by Münch et al. (2025) randomized female entrepreneurs into consortia and examines the effects on export performance. Relative to these papers, the

[2] Relatedly, Barteska and Lee (2023) analyze the rotation of bureaucrats within the South Korean export-promotion agency and find that individual bureaucrats matter for the effectiveness of export-promotion programs.

[3] Summing up the literature as of 2015, Van Biesebroeck et al. (2015, p. 1483) write, "Virtually all papers in the literature agree that the dimension of export performance most affected by these programs is the extensive margin." Van Biesebroeck et al. (2015) is one of the few studies that finds a significant intensive-margin effect, which the authors note could be due to Arkolakis (2010)-type market-penetration costs.

distinctive aspect of our study is the focus on government financial support aimed explicitly at increasing exports.

Another distinctive aspect of the Tasdir+ program is worth emphasizing. Relative to the many export-promotion programs evaluated in the literature, the Tasdir+ program provides arguably the cleanest source of variation specifically in the fixed costs of market access. Although a number of evaluated programs have included subsidies for export-related expenses (Görg et al., 2008; Girma et al., 2009; Van Biesebroeck et al., 2016; Broocks and Van Biesebroeck, 2017; Helmers and Trofimenko, 2013; Defever et al., 2020; Srhoj and Walde, 2020; Girma et al., 2020), the interpretation of their effects is made difficult by the facts that the subsidies were bundled with other components and/or that they covered variable as well as fixed costs. Beyond the earlier matching-grant-for-exports program in Tunisia (FAMEX II), which was designed similarly, we are not aware of programs that are as readily interpretable as subsidies for the fixed costs of exporting as they typically appear in trade models. Our setting is particularly apt for analyzing the effects of variation in such costs.

There is a small experimental literature on matching grants unrelated to exporting. Several experimental evaluations have failed because of political pressures or low take-up (Campos et al., 2014). Among the few successes, McKenzie et al. (2017) find positive short-term effects of matching grants for business services in Yemen on product innovation, marketing, and adoption of accounting systems, but are not able to look at longer-term impacts because of political instability. Bruhn et al. (2018) find that a program offering subsidized consulting services to micro, small, and medium enterprises in Mexico had positive effects on sales, profits, productivity, and long-run employment. Grimm et al. (2024) find that matching grants for technical and management training and consulting services in Burkina Faso have no effects on profits, employment, and sales, but have positive effects on firms' survival and business practices. Relative to this literature, we believe that there is value in focusing on matching grants to promote exports, widely viewed as a key driver of upgrading by developing-country firms (Verhoogen, 2023).

Our paper is related to work in international trade on entry costs and the responsiveness of firms on extensive and intensive margins, outside of the context of export-promotion programs. An influential study by Das et al. (2007) estimates a dynamic model of export entry by Colombian firms and infers that the sunk costs of

export entry are large and that subsidizing them is unlikely to be effective in increasing exports.[4] One possible reading of our results, consistent with the first inference of Das et al. (2007), is that the matching grants were small relative to the sunk costs of entry into a new destination; that might explain the lack of response on the destination margin. But our finding that the market-access subsidies had a significant effect on the intensive margin of exports runs counter to the second inference. Related work on the question of whether exports expand primarily on the extensive margins of destinations and products or on the intensive margin of sales within particular destinations and products includes Kehoe and Ruhl (2013), Albornoz et al. (2023), Fitzgerald et al. (2024) and Erhardt and Gupta (2024). Relative to this literature, our study has the advantage of a clean source of exogenous variation and, specifically, variation in fixed costs as opposed to variable costs.

Finally, our paper is related to a growing literature on empirical evaluations of industrial-policy interventions, reviewed by Juhász et al. (2023a), Juhász and Steinwender (2023), Lane (2020), and Reed (2024). Relative to other forms of industrial policy, matching grants for exports have several attractive aspects: firms are required to have "skin in the game" and hence are arguably more likely to select promising investments; the subsidies are broad ("horizontal" in the terminology of Crespi et al. (2014) and others) and do not require governments to choose specific sectors or firms to support; and, as mentioned above, evidence is mounting that exporting can drive various forms of upgrading, which in turn generate positive externalities for other firms. As Reed (2024) and Juhász et al. (2023b) emphasize, export promotion has been the principal form of industrial policy in many developing countries. As one of the first randomized evaluations of an industrial-policy intervention, our paper is helping to strengthen the evidence base in this important policy area.

The paper is organized as follows. The next section presents our theoretical framework. Section 3 provides an overview of the program context and roll-out. Section 4 describes the data and estimation sample. Section 5 discusses our empirical strategy and Section 6 reports the reduced-form results. Section 7 presents our calibration exercise and Section 8 concludes.

[4] Das et al. (2007) argue that this is because "exporters that need a subsidy to get into export markets are almost always marginal suppliers" that face relatively high entry costs, and "large incumbent exporters, who account for most of the industry's foreign sales, are unaffected by entry subsidies" (p. 868). See also Cherkashin et al. (2015) and Arkolakis et al. (2021) and the review by Alessandria et al. (2021).

2 Conceptual Framework

To guide our empirical analysis, this section presents a partial-equilibrium model of market-penetration and product-scope decisions by heterogeneous, multi-product firms. It is essentially a simplified version of the framework of Arkolakis et al. (2021) Appendix S2, which introduces market-penetration costs (à la Arkolakis (2010)) and endogenous product scope (à la Eckel and Neary (2010), Bernard et al. (2011), and Mayer et al. (2014)) into a Melitz (2003)-type framework. We depart slightly from Arkolakis et al. (2021) in imposing approximations in order to derive explicit analytical solutions for product scope and market penetration, but our approach is broadly consistent with theirs.

2.1 Supply Side

In each source country s, there is an exogenously given continuum of potential producers of measure 1. Each has a productivity parameter, ϕ, and a set of firm-destination-specific fixed market-access costs, c_d, as in Eaton et al. (2011), where d indexes destinations. We order a firm's products in a destination ($g = 1, 2, 3, ...G_{sd}$) by increasing distance from the firm's core competence. Following Arkolakis et al. (2021), we assume that a firm's efficiency in producing a product declines in g according to $\phi_g = \frac{\phi}{g^\alpha}$, where $\alpha \geq 0$. We normalize labor cost to 1 and allow for a destination-specific iceberg trade cost, τ_{sd}. Marginal cost is then:

$$mc_{sdg} = \frac{\tau_{sd}}{\phi_g} = \frac{\tau_{sd} g^\alpha}{\phi} \tag{1}$$

Let n_{sd} be the share of consumers in destination d that are reached by firm ϕ from source country s. Again following Arkolakis et al. (2021), we assume that n_{sd} is the same for all products of a firm in a given destination. We further assume that the total costs of market access are given by:

$$F_{sd}(G_{sd}, n_{sd}; c_d) = c_d \sum_{g=1}^{G_{sd}} f_{sd}(g, n_{sd}) \tag{2}$$

where for each product:

$$f_{sd}(g, n_{sd}) = \frac{g^{\delta_{sd}}}{\psi} \ln\left(\frac{1}{1 - n_{sd}}\right) \tag{3}$$

Here δ_{sd} governs how market-access costs vary with distance from core competence.

The parameter ψ captures the cost of marketing, for instance, the cost of posting an advertisement, defined (following Arkolakis (2010)) such that a higher ψ corresponds to lower cost; we assume $\psi > 0$. Arkolakis (2010) provides a microfoundation for this specification based on a model of the visibility of advertisements.[5] The key properties are that the costs of access increase in market penetration ($\frac{\partial f_{sd}}{\partial n_{sd}} > 0$), at an increasing rate ($\frac{\partial^2 f_{sd}}{\partial n_{sd}^2} > 0$), and approach infinity as penetration approaches 1 ($\lim_{n\to 1} f_{sd} = \infty$), which guarantees less-than-full market penetration in equilibrium. Although F_{sd} varies with market penetration and product scope, it does not depend directly on firm output; in this sense, it can be thought of as representing fixed market-access costs.

2.2 Demand Side

In each destination country, there is a continuum of consumers of measure 1. Each consumer, ℓ, faces a potentially different set of firms from each source, Ω_{sd}^{ℓ}, and has constant-elasticity-of-substitution (CES) preferences over bundles of varieties offered by firms:

$$U_d = \left(\sum_{s=1}^{N} \int_{\phi \in \Omega_{sd}^{\ell}} X_{sd}(\phi)^{\frac{\sigma-1}{\sigma}} d\phi \right)^{\frac{\sigma}{\sigma-1}} \tag{4}$$

where the firm-specific bundle $X_{sd}(\phi)$ is itself a CES combination of varieties:

$$X_{sd}(\phi) = \left(\sum_{g=1}^{G_{sd}(\phi)} x_{sdg}(\phi)^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \tag{5}$$

Here $x_{sdg}(\phi)$ is consumption of variety g and $G_{sd}(\phi)$ is the set of varieties offered by firm ϕ in destination d. Following Arkolakis et al. (2021), we assume that the elasticities of substitution at the variety and firm level are both equal to σ; setting the elasticities equal in the two nests simplifies the algebra but is not crucial for our predictions.[6]

Assuming large numbers of firms and consumers in each country, the demand for each product in each destination is:

$$x_{sdg}(\phi) = p_{sdg}(\phi)^{-\sigma} n_{sd}(\phi) T_d P_d^{\sigma-1} \tag{6}$$

[5] Arkolakis (2010) allows the rate at which returns to advertising diminish, captured by his parameter β, to vary. Here we focus on the case where $\beta = 1$ in his model. This greatly simplifies the exposition without (in our view) sacrificing insights that are important in our setting. Note that a minus sign is missing in the second part of equation (2) of Arkolakis (2010), which corresponds to (3) above.

[6] To keep the framework as simple as possible, we do not introduce an explicit term capturing product quality; note that $x_{sdg}(\phi)$ can be considered to be consumption in quality-adjusted units. See e.g. Appendix D of Kugler and Verhoogen (2012) for a discussion.

where $p_{sdg}(\phi)$ is the price of variety g offered by firm ϕ; $n_{sd}(\phi)$ is the firm's probability of reaching a given consumer; T_d is total consumer expenditure, and P_d is the aggregate CES price index corresponding to (4)-(5).[7]

2.3 Optimal Product Scope and Market Penetration

We can now consider the firm's choices of product scope and market penetration. To reduce clutter, we focus on the decisions of a single firm, with productivity ϕ, from a single origin, s, selling to a single destination, d, and suppress the sd subscripts and the dependence on ϕ. Similar relationships hold for each firm-source-destination combination.

Given the CES demand structure, the firm's markup over costs is constant and the optimal price is:

$$p_g = \left(\frac{\sigma}{\sigma-1}\right) mc_g = \frac{\sigma}{\sigma-1}\frac{\tau g^{\alpha}}{\phi} \tag{7}$$

Using (1), (2), (3), (6) and (7), the profit of the firm can then be written as a function of product scope, G, market penetration, n, and variables that the firm takes as exogenous:

$$\pi(G,n) = \sum_{g=1}^{G}(p_g - mc_g)x_g - c_d\sum_{g=1}^{G} f(g,n) = \Theta n\left(\sum_{g=1}^{G} g^{-\alpha(\sigma-1)}\right) - \frac{c_d}{\psi}\ln\left(\frac{1}{1-n}\right)\sum_{g=1}^{G} g^{\delta} \tag{8}$$

where $\Theta \coloneqq \left(\frac{\sigma-1}{\sigma}\frac{\phi}{\tau}\right)^{\sigma-1}\frac{T_d {P_d}^{\sigma-1}}{\sigma}$.

As in Arkolakis et al. (2021), we need to impose a restriction on the parameters to ensure that the firm's product-scope choice problem is well defined:

$$\delta + \alpha(\sigma-1) > 0 \tag{9}$$

This condition ensures that the incremental costs from adding a new product will increase more quickly than incremental revenues and hence that we will have an interior solution in the choice of product scope.[8]

In this context, Tasdir+-type matching grants can be interpreted either as reducing

[7] That is,

$$P_d = \left(\sum_{s=1}^{N}\int_0^{+\infty}\sum_{g=1}^{G_{sd}(\phi)} p_{sdg}(\phi)^{1-\sigma} n_{sd}(\phi) h_s(\phi) d\phi\right)^{\frac{1}{1-\sigma}}$$

where $h_s(\phi)$ is the probability density of productivities in s. See Arkolakis (2010) and Arkolakis et al. (2021).

[8] This condition is an analogue in our context of the assumption in Arkolakis et al. (2021) that combined incremental scope costs are strictly increasing (their Assumption 1).

destination-level market-access costs, c_d, or as lowering marketing costs within a given destination, here captured by an increase in ψ. Given our functional-form assumptions, reductions in c_d and increases in ψ are isomorphic; only the ratio c_d/ψ appears in (8). Let $a \coloneqq c_d/\psi$ represent combined market-access costs.

The fact that g and G take on only integer values makes the optimization problem non-standard — in particular, a mixed integer programming problem. Such problems are often difficult to solve analytically (see e.g. Boyd et al. (2007, Sec. 9.2)), and this case is no exception. For the purpose of guiding the interpretation of our experiment, we feel that it is useful to derive explicit analytical results. To do so, we approximate the summations in (8) by Riemann integrals:[9]

$$\sum_{g=1}^{G} g^{\delta} \approx \frac{G^{1+\delta}}{1+\delta}, \qquad \sum_{g=1}^{G} g^{-\alpha(\sigma-1)} \approx \frac{G^{1-\alpha(\sigma-1)}}{1-\alpha(\sigma-1)} \tag{10}$$

These integrals are well defined only if the exponents in the summations are greater than -1,[10] i.e.

$$\delta > -1, \qquad \alpha(\sigma-1) < 1 \tag{11}$$

We impose these restrictions hereafter.

Using an envelope theorem with an arbitrary choice set from Milgrom and Segal (2002), optimal market penetration can be written as a function of product scope, G:

$$n^*(G) = 1 - \frac{a\Lambda}{\Theta} G^{\delta+\alpha(\sigma-1)} \tag{12}$$

where $\Lambda \coloneqq \frac{1-\alpha(\sigma-1)}{1+\delta}$ and $0 < \Lambda < 1$ by (9) and (11).[11] See Appendix A.1.

To characterize the optimal choice of product scope, we first consider a relaxation of the problem in which product scope is treated as a continuous variable, call it $\widetilde{G}$. In Appendix

[9] These approximations are increasingly good as G grows large. To see this, let $z_g = g/G$ and note that $z_1, z_2, ..., z_G$ form a regular partition of the interval $[0,1]$. Then:

$$\lim_{G\to\infty} \frac{\sum_{g=1}^{G} g^{\delta}}{G^{1+\delta}} = \lim_{G\to\infty} \sum_{g=1}^{G} z_g^{\delta} \triangle z_g = \frac{\int_0^1 z^{\delta} dz}{G^{1+\delta}} = \frac{1}{1+\delta}$$

where $\triangle z_g = z_g - z_{g-1}$. A similar argument holds for $\sum_{g=1}^{G} g^{-\alpha(\sigma-1)}$.

[10] See e.g. Hunter (2014, ex. 12.25).

[11] This equation holds for $n^*(\cdot) \in [0,1)$, which we will show to be the case.

A.2, we derive an analytical solution for the optimal $\widetilde{G}$:

$$\widetilde{G}^* = \left[\frac{\Theta}{a\,|W_{-1}\left(-\Lambda e^{-\Lambda}\right)|}\right]^{\frac{1}{\delta+\alpha(\sigma-1)}} \tag{13}$$

where $W_{-1}(\cdot)$ is one branch of the Lambert W function (Corless et al., 1996).[12] Note that optimal product scope is declining in market-access costs ($\frac{\partial \widetilde{G}^*}{\partial a} < 0$). Plugging (13) into (12) gives the optimal market penetration in the relaxed case — call it $\widetilde{n}^*$ — which does not vary with market-access costs ($\frac{\partial \widetilde{n}^*}{\partial a} = 0$).

Turning back to the integer-constrained problem, we show in Appendix A.3 that optimal *integer* product scope, G^*, is a decreasing step function of a, defined by the following cutoffs:

$$\widehat{a}_k = \frac{\Theta H}{\Lambda |W_{-1}(-He^J)|} \tag{14}$$

where $H \coloneqq \frac{\left((k+1)^{1-\alpha(\sigma-1)} - k^{1-\alpha(\sigma-1)}\right)}{\left((k+1)^{1+\delta} - k^{1+\delta}\right)}$, $J \coloneqq (\delta+\alpha(\sigma-1))\,\frac{\left((k+1)^{1+\delta}\ln(k+1) - k^{1+\delta}\ln k\right)}{(k+1)^{1+\delta}-k^{1+\delta}} - 1$, and k indexes the steps. It follows from (12) that optimal market penetration in the integer-constrained problem, $n^*(a)$, is declining in a within each range $(\widehat{a}_{k+1}, \widehat{a}_k)$ and that the optimal market penetration increases discontinuously at each cutoff (i.e. $\lim_{a\to\widehat{a}_k^-} n^*(a) < \lim_{a\to\widehat{a}_k^+} n^*(a)$). It follows from (14) that the cutoff for introducing the first product in a destination is $\widehat{a}_0 = \frac{\Theta}{\Lambda}$.

The key patterns are illustrated in Figures 1 and 2. In Figure 1, the green curve represents the optimal (continuous) $\widetilde{G}^*(a)$, which declines in a; the $\{a_k\}$ are the values of a at which $\widetilde{G}^*(a)$ happens to take on integer values (i.e. at which $\widetilde{G}^*(a_k) = k$ for $k = 1, 2, 3, 4, ...$). The blue step function, with cutoffs $..., \widehat{a}_4, \widehat{a}_3, \widehat{a}_2, ...$, represents the optimal product scope under the integer constraint, $G^*(a)$. In Figure 2, optimal market penetration in the relaxed problem is given by the red dotted horizontal line at $\widetilde{n}^*$. The purple sawtooth curve represents $n^*(a)$, the optimal market penetration under the integer constraint, which declines in market-access costs between consecutive cutoffs $\widehat{a}_{k+1}$ and $\widehat{a}_k$.

[12] The Lambert W function is also sometimes referred to as a product logarithm, since if (and only if) $ye^y = x$ then $y = W_j(x)$; the function has two branches, indicated by $j \in \{-1, 0\}$. For $x \geq 0$, $y = W_0(x)$; for $-\frac{1}{e} \leq x < 0$, $y = W_0(x)$ or $W_{-1}(x)$. We show in Appendix A.2 that $W_{-1}(\cdot)$ is the relevant branch in (13). It will be useful below to note that $W_{-1}(x) \leq -1$ for $-\frac{1}{e} \leq x < 0$.

2.4 How Product Scope, Market Penetration, and Exports Respond to Market-Access Subsidies

Market-access subsidies of the type provided by the Tasdir+ program in our experiment can be thought of as reducing market-access costs, a. What impacts would we expect on product scope, market penetration, and total exports? We draw two main implications from the model.

First, product scope may be fairly insensitive to changes in market-access costs. As is evident in Figure 1, G is weakly decreasing in a (and hence weakly increasing in reductions in a), but, crucially, a reduction in a will only affect G if the change shifts a across one of the cutoffs, $\widehat{a}_k$. Depending on the values of the various parameters, the ranges over which destination entry and destination-specific product scope remain unchanged may be wide. If a firm is initially just to the right of a cutoff, then even a small reduction in a could bump it to another step (and increase product scope) but this effect may be small on average.

Second, market penetration and export sales can be expected to respond even in the absence of changes in product scope. Even when they do not shift a across one of the cutoffs, reductions in a have an impact on the intensive margin of market penetration within a given destination-product. As can be seen in Figure 2, between any two thresholds, $\widehat{a}_{k+1}$ and $\widehat{a}_k$, a reduction in a increases n. As a consequence, a firm's exports can be expected to increase with the reduction in market-access costs, even when there is no change in product scope. Using (6), (7), and (12), we can write total firm export revenues from a destination, call them E, as follows (refer to Appendix A.4):

$$E = \sum_{g=1}^{G} px_g = \left[\Theta - a\Lambda G^{\delta+\alpha(\sigma-1)}\right] \frac{\sigma G^{1-\alpha(\sigma-1)}}{1-\alpha(\sigma-1)} \tag{15}$$

It is evident from (15) that for a given product scope, G, exports are declining in market-access costs, a. In the empirical work below, we will consider proportional changes in exports in response to reductions in market-access costs. If product scope is unchanged, then the proportional change in exports can be written as:

$$\frac{dE/da}{E} = -\frac{\Lambda G^{\delta+\alpha(\sigma-1)}}{\Theta - a\Lambda G^{\delta+\alpha(\sigma-1)}} < 0 \tag{16}$$

If the reduction in a induces changes in product scope, then the proportional change in

exports will be a complicated mix of intensive- and extensive-margin changes. But we will see below that the case in which there is no change in product scope appears to be the more empirically relevant one. In this case, we have an unambiguous prediction that exports increase proportionally in response to a reduction in market-access costs. As noted above, the implication that exports might respond on the intensive margin to reductions in the fixed costs of market access, even in the absence of changes in product scope, stands in contrast to the predictions of now-standard heterogeneous-firm models of trade such as Melitz (2003) and Bernard et al. (2011), in which fixed market-access costs matter for firms' entry into destinations and products but not for market penetration or sales conditional on entry.

Although we consider them to be of lower importance, our framework also carries implications for heterogeneous treatment effects in response to the subsidies. In particular, the response of exports can be expected to vary with the level of market-access costs that a firm faces, other things equal. From (16), if there is no change in product scope, it follows that:

$$\frac{\partial}{\partial a}\left(\frac{dE/da}{E}\right) < 0 \tag{17}$$

That is, lower market-access costs dampen the responsiveness of exports to reductions in market-access costs (i.e. $\frac{dE/da}{E}$ is less negative for firms facing lower a). Similarly, it can be shown that the responsiveness of exports in proportional terms is lower for higher-productivity (and hence larger) firms. Recalling from the definition of Θ following equation (8) that Θ is increasing in firm productivity, ϕ, we have:

$$\frac{\partial}{\partial \phi}\left(\frac{dE/da}{E}\right) > 0 \tag{18}$$

That is, for a given product scope and level of market-access costs, higher productivity dampens the responsiveness of exports to reductions in market-access costs (i.e. $\frac{dE/da}{E}$ is less negative).

3 Context and Experimental Design

A key motivation for Tunisian export-promotion programs has been limited diversification of the country's export destinations and products (Lopez-Calix et al.,

2010). Approximately 75% of Tunisia's exports go to Western Europe — 58.1% to France, Italy, and Germany alone. Exports are concentrated in a small number of sectors, including machinery and transport equipment, textiles, and agricultural products.[13]

The Tunisian government has devoted substantial resources to a series of export-promotion programs, administered by its export-promotion agency, CEPEX. In 1999, it launched a matching-grants-for-exports program known as FAMEX I with a budget of USD 24 million (over multiple years). This was followed in 2006 by FAMEX II — the subject of Gourdon et al. (2011) and Cadot et al. (2015) — with a budget of USD 37.6 million. Since 1989, CEPEX has also managed a large program to defray transportation costs for Tunisian exporters, known as FOPRODEX, with a recent budget of USD 17-25 million per year.

The Tasdir+ program, known officially as the Fund for Competitiveness and Export Development Support [*Fonds d'Appui à la Compétitivité et au Développement des Exportations*], was created in 2014 with a budget of USD 23.5 million. The stated objectives were to increase exports in a sustained manner and to promote diversification towards higher-value-added exports and new markets. The program funded non-randomized waves of matching grants in 2015 and 2017.

Given the large expenditures on export promotion, the Tunisian government has been keenly interested in the cost-effectiveness of its subsidies. It decided in 2018, with encouragement from the World Bank, to randomize grants in the third and fourth waves of the Tasdir+ program.[14] The budget for the randomization sample was USD 14 million. There were four calls for applications as part of the third wave and one call as part of the fourth wave. We refer to these calls as "rounds" and focus on these five rounds hereafter.

As part of the application, firms were required to submit a "business plan" listing expenditures to be subsidized by the grant. CEPEX staff reviewed the applications to determine eligibility. To be eligible, a firm had to fulfill the following criteria: (1) be privately owned; (2) be legally based in Tunisia; (3) not be a retailer or wholesaler;[15] (4) not be an artisanal firm; (5a) (for non-agricultural firms) have a liquidity ratio, defined as assets over liabilities averaged for the three calendar years preceding the program

[13] These figures are from our own calculations using World Integrated Trade System (WITS) data.

[14] In parallel to the randomization, a smaller number of firms received support in the third and fourth waves without being included in the randomization sample; see Appendix B for details.

[15] An exception to this rule was made for import-export firms, known locally as "trading firms."

year, greater than or equal to 1,[16] or (5b) (for agricultural firms) have five or more permanent employees or at least one export operation during the three calendar years preceding the program year; and (6) be established prior to Jan. 1, 2015 for Rounds 1-4 or Jan. 1, 2017 for Round 5. Further details on the eligibility rules are in Appendix B.

In their applications, firms could propose a budget of up to TND 300,000 (USD 100,000),[17] of which the matching grant would cover 50%. Business plans could be of two types: support for exporting (*appui a l'export*) or setting up an affiliate abroad (*implantation a l'étranger*). Approximately 75% of firms chose the former. For this type of plan, eligible expenses included visits to expositions and trade fairs; market research expenditures; marketing expenditures; creation of websites and other forms of online marketing; and certification and regulatory compliance expenditures for products. For the second type of plan, eligible expenses included rent and other costs of maintaining the foreign office; travel abroad to monitor the foreign office; costs of trademark registration in the foreign country; and technical assistance required to set up or maintain the foreign office. A full list of eligible expenditure categories appears in Table 1. Importantly for our study, variable costs such as wages and material input expenditures were not eligible for support, nor were capital investments such as purchase or installation of equipment. In their applications, firms were required to list 2-3 target export destinations, of which at least half had to be new destinations. Firms' business plans had to be approved by CEPEX staff in order for the firms to be included in the randomization sample. Once the business plans were approved, firms were also required to respond to our baseline survey to be included in the randomization.

Figure 3 summarizes the timeline of the experiment. The first call for applications to the experimental phase was launched in July of 2018. Randomization for the five rounds was carried out in public meetings in Sept. 2018, Nov. 2018, Feb. 2019, May 2019, and Dec. 2019. Following each randomization, the Tasdir+ steering committee formally approved the grants for selected firms and the firms were officially enrolled; this process typically took 2-3 months. Firms then had 12 months in which to incur the expenses in their business plans. Note that part of the Round 4 spending period and almost all of the Round 5 spending period coincided with the COVID-19 pandemic; we return to this issue below. Control firms were excluded from re-applying to the program for at least 12

[16] For round 5, the liquidity threshold was lowered to 0.9.

[17] The average exchange rate over our study period was approximately 3 TND/USD, and we use that rate throughout the paper.

months.[18] Although we primarily rely on administrative data to track outcomes, we conducted a follow-up survey with Rounds 1-2 in July-December 2020 and with Rounds 3-5 in March-August 2021. When referring to pre-program information, we follow Tasdir+ practice in using 2017 as the reference year for Rounds 1-4 and 2018 for Round 5.

Within each round, randomization was stratified by sector, size, and type of business plan. Firms were classified into 6 sectors: agriculture/fishing (5% of randomization sample), trading (10%), food processing (7%), non-food manufacturing (34%), information and communication technology (ICT) services (14%), and non-ICT services (30%).[19] Size categories for each sector (small, medium, large) were calculated based on the revenue distribution of previous beneficiaries of Tasdir+. In some rounds, because of prohibitively small numbers of firms in some strata, we pooled strata prior to randomization; see Appendix B for details. At the most disaggregated level, there were 28 strata.

The experiment faced a key difficulty in implementation. The original design included a "pay for performance" treatment arm in which firms that succeeded in increasing exports would receive an additional rebate, up to 40% of eligible expenses, beyond the 50% matching grant (i.e. up to 90% together). To conform to World Trade Organization (WTO) rules, these performance rebates were limited to firms selling food and agricultural goods. See Appendix B.2 for details. Approximately midway through our study, an external government auditor prohibited the Tasdir+ program from reimbursing more than 50% of expenses, effectively eliminating the rebates. In total, of the 41 firms selected to be eligible for rebates, only 5 submitted rebate requests and none received a rebate. Thus, in practice, the "Matching Grant + Rebate" arm was effectively the same as the "Matching Grant Only" arm and we treat them as a single treatment in our analysis.

Pooling rounds, 487 firms were included in the randomization sample. In Rounds 1-4, two-thirds of firms in the rebate-eligible sectors and one-half of other firms were assigned to receive matching grants. In Round 5, two-thirds of all firms were assigned to receive matching grants. Overall, among the 487 firms in the randomization sample, 281

[18] Four control firms from Round 1 re-applied in Round 5; these firms were not included in the Round 5 randomization sample.

[19] When applying, firms self-classified into these six sectors. Tasdir+ consultants verified sectoral affiliation against firms' commercial register.

were assigned to receive grants and 206 were assigned to control.

Two additional features of the institutional context are particularly relevant. First, a significant share of Tunisian firms (about 30% of our randomization sample) are classified legally as "totally exporting." Administratively, these firms are in a free trade zone, outside of the Tunisian customs area. Firms in this regime sell most of their production abroad, but are allowed to sell part of their output (usually up to 30% of sales) on the domestic market.[20] Second, wages in Tunisia are in large part determined by sectoral bargaining agreements (World Bank, 2014; Angel-Urdinola et al., 2015). According to the International Labour Organization, these agreements covered around 63% of private- and public-sector workers in 2019 (ILO, 2023). The agreements stipulate base salary grids that are binding for many firms (Ali and Trang, 2025).

4 Data

This section reviews the data sources we use and the estimation samples we construct. Additional details are in Appendices C and D.

4.1 Sources

Several sources of administrative records are collected in the *Repertoire National des Entreprises (RNE)* [National Repertory of Firms], a database of registered firms maintained by the Tunisian national statistical agency, the *Institut National des Statistiques (INS)*. In particular, the RNE contains annual domestic sales and exports from corporate tax declarations to the *Direction Générale des Impôts (DGI)* [General Tax Authority] and firm-level employment and wages from the *Caisse Nationale de Sécurité Sociale (CNSS)* [National Social Security Fund]. We have been able to link all 487 firms from the Tasdir+ randomization sample to the RNE, although not all variables are available for all firms, as discussed below. Importantly, the RNE data contain information on exports for service firms as well as non-service firms (from the corporate tax records). We use the RNE from 2015 to 2021, the latest year to which we have access.

Through an agreement between two government ministries that we helped to facilitate, we have also been able to access data on firms' international transactions from

[20] Totally exporting firms have benefited historically from an array of advantages such as VAT exemption, a reduced 10% corporate tax rate, import tax exemptions on inputs, and hiring incentives.

2017-2022 from the Tunisian customs authority. In these data, we observe transaction-level export records at the level of destination country, date of shipment, and 11-digit product category.[21] A shortcoming of the customs data is that firms are required to submit declarations only when they ship physical goods; service firms typically are not required to submit declarations for exported services. For this reason, we primarily rely on the RNE for information on exports, although we use the customs data when focusing on the number of destinations or products.

We also have access to administrative data from the Tasdir+ program itself, for all firms in the randomization sample. Data from firms' applications include sector, quality certifications, number of employees, sales, targeted countries, and expected effects. We also observe reimbursement requests and amounts disbursed for treated firms.

Finally, we conducted baseline and follow-up surveys. We asked about the actions firms took to increase exports and about the amounts spent on innovative activities, among other variables. Since responding to the baseline survey was a condition for eligibility for the matching grant, we have complete coverage at baseline. But response rates to the endline survey were much lower, in part due to the COVID-19 pandemic and in part to the fact that CEPEX had little leverage to oblige firms to respond.[22] For this reason, we rely primarily on the information from administrative records to evaluate the impact of the program, although we present some results from the survey below.

4.2 Samples

Given the incomplete information in the RNE, customs and survey datasets, we must use different samples when analyzing different outcomes. The five samples we use are the following.

1. The *Randomization Sample* contains information from the Tasdir+ applications for all 487 firms included in the randomization.

2. The *RNE Export Sample* includes the 377 firms for which sales and exports from the DGI (tax authority) are available in the RNE in both the reference year (2017 for

[21] The 11-digit categories are from the *Nomenclature de Dédouanement des Produits (NDP)* [Customs Clearance Product Nomenclature], Tunisia's most detailed product classification; the first 6 digits are harmonized with the international Harmonized System (HS) trade categories.

[22] Lower-than-usual responses during the pandemic were documented even in established surveys like the Current Population Survey in the US (Rothbaum and Bee, 2021).

Rounds 1-4, 2018 for Round 5) and in 2021 (and for which sales are observed in the application data at baseline); the reported exports may be zero.

3. The *RNE Employment Sample* includes the 327 firms in the *RNE Export Sample* for which the RNE also reports employment and wages from the CNSS (social security agency) in both the reference year and in 2021.

4. The *Customs Sample* contains the 210 non-service firms from the RNE Export Sample. If a non-service firm from the RNE Export Sample has no reported exports in the customs data in a given year, we impute zero exports, and similarly for imports. Although service firms occasionally show up in the customs data as exporters or importers of physical goods, the customs data do not give a true picture of their engagement in international markets and we exclude them.

5. The *Survey Sample* contains the 204 firms for which we observe information on actions and spending in both the baseline and endline surveys.

Our working data contain at most the original 487 firms part of the experiment and not the full population of Tunisian firms present in the various registries.

4.3 Descriptive Statistics

Table 2 reports balance between the treatment and control groups for the Randomization Sample. Appendix Tables A1-A4 present similar balance tables for the other samples. To facilitate comparison, we include five key variables from the administrative data for each sample — export regime, firm age, domestic capital share, employment, and sales — in addition to variables specific to each sample. In these balance tables, as well as in the regressions below, monetary variables have been winsorized at the 3%/97% level to reduce the influence of outliers. We observe balance between the treatment and control groups, conditional on round and stratum fixed effects, in the Randomization, RNE Export, RNE Employment, and Customs Samples. At the same time, it is important to note that the patterns of missing data create differences across samples (without generating imbalance between treatment and control); in particular, firms in the RNE and Customs Samples tend to be older and larger than firms in the full Randomization Sample. Also, there is some indication of imbalance in the Survey Sample; in particular, it appears that larger (i.e. higher-sales) treated firms were more likely to respond to the endline than larger control firms. Although we do not

reject the joint null of no treatment-control differences (conditional on round and stratum effects) in this sample, below we show that the results are robust to controlling for the baseline characteristics that display a lack of balance.

5 Empirical Strategy

Given that we have a randomized experiment, the empirical analysis is straightforward. The main complication is that the outcome of primary interest is exports, a variable with a significant number of zeros. There is an active econometric debate about the best way to analyze outcomes in such cases (Chen and Roth, 2024; Mullahy and Norton, 2024). The literature has not converged on a consensus solution. Our approach is to present estimates using what we perceive to be the two leading approaches and show that the results are robust.

Our preferred specification is a simple two-part ANCOVA specification, where we run separate regressions with (binary) exporter status and log exports as outcomes and control for values of the dependent variable in the reference year (pre-program). The basic specification is:

$$Y_{i,2021} = \beta Treated_i + \alpha Y_{i,refyear} + \gamma_r + \delta_s + \epsilon_i \tag{19}$$

where $Y_{i,2021}$ denotes outcome Y for firm i in year 2021 (e.g. either exporter status or log exports), $Y_{i,refyear}$ denotes outcome Y for firm i in the reference year (2017 for rounds 1-4, 2018 for round 5), $Treated_i$ is an indicator for being *assigned* to receive the matching grant in the randomization (we discuss take-up of the program in Section 6.1 below), γ_r are round fixed effects, δ_s are strata fixed effects, and ϵ_i is an error term. The approach of controlling for baseline values of the outcome has been shown to have higher power than standard difference-in-differences when autocorrelation is low (McKenzie, 2012). Note that the regression with log exports as the outcome can only be implemented among continuous exporters, and only captures the intensive margin of exports, conditional on exporting. When analyzing outcomes without zeros (for example, sales and employment), we simply estimate (19) with the log of the outcome as the dependent variable.

For outcomes with many zeros, we also present estimates from a Poisson Pseudo

Maximum Likelihood (PPML) estimator (Gourieroux et al., 1984; Santos Silva and Tenreyro, 2006; Wooldridge, 2010, section 18.2; Chen and Roth, 2024; Mullahy and Norton, 2024), where we include firm fixed effects to increase precision. The model is:

$$Y_{it} = \exp(\beta Treated_i \times Post_t + \gamma_i + \lambda_t)\nu_{it} \tag{20}$$

where Y_{it} denotes the level of the outcome (e.g. exports) for firm i in the reference year (2017 for rounds 1-4, 2018 for round 5) or 2021, $Treated_i$ is an indicator for being assigned to receive the matching grant in the randomization, $Post_t$ is an indicator for the year 2021, γ_i are firm fixed effects, λ_t are year fixed effects, and ν_{it} is an error term (with mean 1) clustered by firm. In this context, $e^{\beta} - 1$ is equal to the population-average treatment effect as a proportion of the control mean. Note that this estimand combines effects on the extensive and intensive margins (e.g. of entry into exporter status and exports conditional on entry).

Our choice of outcome year merits some discussion. The RNE is an annual dataset, covering calendar years. Three of our five randomization rounds occurred in 2019 (refer to Figure 3). The first year of the COVID-19 outbreak, 2020, saw a sharp contraction of economic activity in Tunisia, as elsewhere, and events in that year are difficult to interpret. We therefore focus on 2021, the latest year to which we have access in the RNE, to study post-intervention outcomes.

6 Results

6.1 Take-Up

We begin with a brief discussion of firms' take-up of the program. Appendix Table A5 presents basic statistics from Tasdir+ administrative data. Defining take-up as enrolling in the Tasdir+ program (which required signing a legal agreement with CEPEX) or as submitting at least one reimbursement request, take-up was high. Of the 281 firms assigned to treatment, 269 (96%) signed a contract and officially enrolled. For the enrolled firms, the average approved matching grant was USD 21,339 (TND 64,016), based on an average business-plan budget of USD 42,678. Of the 269 enrolled firms, 187 (70%) eventually submitted at least one reimbursement request. These take-up rates are higher than in other matching-grant studies in the literature. For instance, in the Bruhn

et al. (2018) study of subsidized consulting services in Mexico, just 80 of 150 firms offered subsidies (53%) took them up. In McKenzie et al. (2017)'s analysis of matching grants in Yemen, 58% (74/128) or 44% (74/170) of firms took up the grants, depending on the base of eligible firms used.

Although take-up was high in the sense of enrollment or submitting at least one reimbursement request, realization rates — the shares of matching grants that were claimed by firms — remained modest. Among the 187 firms that submitted at least one reimbursement request, the average realization rate was just 22%. This figure is not out of line with estimates from similar programs. For instance, Campos et al. (2014) present an example (in their Figure 3) in which only 31% of firms with approved projects actually completed the projects. Nevertheless, it is natural to wonder why the realization rate was so low. Our conversations with firms suggest three possible explanations, all related to administrative costs as perceived by firms. One was that firms were worried about administrative delays or non-payment by CEPEX. Given that firms had to incur expenses up front, the risks of delays or non-payment (either because expenses were deemed ineligible or because of changes in CEPEX policy) appear to have weighed heavily in entrepreneurs' minds. An indication that this perceived risk was real is that 33 firms that submitted reimbursement requests (of the 187 that submitted at least one request) were denied because CEPEX staff considered the expenses to be ineligible or not in line with the approved budget. A second issue was that there were frictions in the process of changing business plans. Although in principle firms were able to modify their plans with CEPEX approval, firms reported that in practice the process was difficult. Especially in view of the fact that the COVID-19 pandemic occurred during our study period, this lack of flexibility may have led firms to reduce spending on the items in their business plan. A third issue was that CEPEX placed restrictions on the timing of expenditures, requiring firms to undertake at least one action from their business plan within three months and to spend 30% of their budgets within six months. Enforcement of these rules tightened over time, and was binding on a majority of firms by the final round (during COVID-19).

What did firms spend money on? CEPEX only observed spending for treatment firms, which makes it difficult to draw causal inferences about the effects of different types of spending from the administrative data. (Below we present survey evidence with treatment-control comparisons of spending for a subsample of firms.) But the patterns

are illustrative. Appendix Figure A1 and Appendix Table A6 report spending breakdowns by broad categories (the categories listed in Table 1). In Appendix Figure A1, we see that the categories with the highest spending shares were promotion (which includes many marketing activities), and prospecting (which includes attending trade fairs and sales missions), and that the actual share of spending (as indicated by approved reimbursements) on promotion exceeded the proposed share, with the opposite pattern for prospecting. Appendix Table A6 reports a further breakdown for totally exporting vs. non-totally exporting firms, which will be relevant below. Non-totally exporting firms spent a larger share on promotion and a smaller share on prospecting than totally exporting firms.

Our preferred indicator of take-up is enrollment in the program, rather than submission of reimbursement requests or share of approved grants that were used. The reason is that having the option of submitting expenses for reimbursement may have had an effect on behavior even of firms that never submitted a reimbursement request. In this sense, firms can be considered treated as soon as they have the option of requesting reimbursement, which was true once they signed their contracts with CEPEX. Because take-up defined in this way was so high (96%) the intent-to-treat estimates using assignment to treatment (from our equations (19) and (20) above) are nearly the same as the treatment-on-the-treated estimates we would obtain using assignment to treatment as an instrument for enrollment. Below we focus on the intent-to-treat estimates.

6.2 Export Outcomes

We now turn to the effects of the matching grants on exports, our main outcome of interest. Panel A of Table 3 reports two-part ANCOVA estimates of (19) in the RNE Export Sample of 377 firms. Column 1 reports a linear probability model with a 0/1 exporter indicator as the outcome. Although the point estimate is positive, indicating an increase of 5% in the probability of having positive exports, on a baseline control mean of 71% (see Table 2), we are not able to reject the null of no effect at conventional levels of confidence. When we focus in Column 2 on continuous exporters (i.e. the 244 firms with positive exports in both the reference year and 2021), we find a significant positive effect of 39 log points (48%).

Panel B of Table 3 reports PPML estimates of (20) for exporter status (Column 1) and the level of exports (Column 2). Although the point estimate is positive, the estimate of the

treatment effect on exporter status is not significant. In Column 2, we find a marginally significant positive estimate of 0.24, equivalent to a proportional average treatment effect of (exp(0.24)-1)=27%. Given that this PPML estimate combines the intensive margin of exports with the extensive margin of entry into exporting, on which there is relatively little adjustment, it is not surprising that the magnitude is smaller in percentage terms than in Column 2 of Panel A.

Whichever specification we focus on, the estimated average effect on exports is large in economic terms. Average exports in the reference year were approximately USD 720,000 (TND 2.15 million). In percentage terms, our estimates range from 27% to 48% over 3-4 years. The increase in exports over the period is thus estimated to be on the order of USD 193,500-344,000. Recall that the program offered reimbursement of up to USD 50,000, but the average payout for firms that submitted at least one reimbursement request was approximately USD 4,440 per firm (Appendix Table A5). Our estimates thus suggest that the program generated an increase of USD 44-78 in exports on average for every dollar spent. This effect is large, but not out of line with the existing literature. For instance, using a non-experimental approach, it has been estimated that a USD 1 increase in the budget of trade promotion organizations is associated with an increase of approximately USD 100 in exports (ITC, 2016; Olarreaga et al., 2020; Olarreaga, 2024). Volpe Martincus and Carballo (2008) find that participation in a Peruvian export-promotion program was associated with an increase in exports of 16-36 log points (17-43%), over a shorter time period than we consider here.

While the large effects we estimate are consistent with previous results in the literature, they raise the question of how they can be reconciled with the fact that many firms did not take full advantage of the matching grants, i.e. that realization rates were low. We believe that two factors played an important role. First, being selected for a matching grant may have had an impact even with no money changing hands, either because the option value of the support allowed for greater risk-taking by firms or because advice from CEPEX staff was helpful for firms. In Appendix Table A7 we provide some descriptive evidence. The table presents our basic two-part ANCOVA specification (similar to Table 3, Panel A), but where we have interacted the Treated variable with an indicator for whether a firm submitted at least one reimbursement request (Panel A) or received at least one reimbursement (Panel B). (Appendix Table A8 presents means for the subsets of treated firms in these categories; on observable dimensions, the subsets

are largely similar.) Caution is warranted in interpreting these results, since whether firms decided to submit a request or receive reimbursement is clearly endogenous, but the results are suggestive. The fact that the point estimate is positive even on the main (uninteracted) Treated term, which corresponds to firms that never submitted a request (0.12, Panel A, Column 2) or never received reimbursement (0.22, Panel B, Column 2), suggests that being assigned to a matching grant itself may have had a positive impact on exporting. Second, as noted above, many firms perceived the administrative costs of the program to be high, because of uncertainty in whether reimbursements would actually be forthcoming, because of inflexibility in adjusting business plans, or because of rules on the timing of expenditures. Given the high perceived administrative costs, it is plausible that firms would only incur them if they perceived the returns to doing so to be high. That dynamic could itself explain the coincidence of low realization rates but large estimated impacts on exporting.

While the program appears to have been quite successful in realizing one of its primary aims, to increase Tunisian exports overall, how successful was it in realizing its other main aim, of helping firms diversify their export destinations and products? To investigate, we turn to the Customs Sample, in which we see destinations of exports and export volumes at the 11-digit product level. Table 4 reports estimates of the effect of the program on the numbers of export destinations and products. Panel A reports our ANCOVA specification, (19), and Panel B reports the PPML specification, (20). For comparison purposes, we include exporter status and exports as outcomes in Columns 1-2, which are directly comparable to Table 3, and the estimates are similar, with slightly larger standard errors (as would be expected given the reduced sample). The key message of the table is that in Columns 3-4, we cannot reject the null of no effect on the numbers of destinations or products. Indeed, the point estimates in Panel A and in Column 4 of Panel B are negative.

Another way to evaluate the success of the matching grants in diversifying exports is to look at the effect on the numbers of *new* destinations or products. If the matching grants led firms both to add new destinations and products and drop old ones, we might see no effect in total numbers of destinations and products but positive effects on new destinations and products. We consider a destination or product to be new if it did not appear in the customs data in the reference year for a given firm. Recall that firms had to identify 2-3 targeted destinations in their Tasdir+ applications, of which at least half had

to be new destinations, so we can also examine whether firms added new targeted destinations. Table 5 presents simple regressions with different measures of new destinations or products as outcomes. With the outcomes defined in this way, there are no baseline values to control for; we present OLS estimates in Panel A and PPML estimates in Panel B, and we only include data from 2021. There are no statistically significant coefficients; the estimates reinforce the above observation that there is little evidence of an effect on the destination or product margins.

Overall, it appears that the matching grants did not significantly increase the diversification of treated firms' exports. Because of imprecision in the estimates, we cannot rule out modest positive effects on the order of 0.46 more destinations or 1.7 new products in response to the program (using ANCOVA estimates from Table 4 Panel A). But with the standard models of Melitz (2003) and Bernard et al. (2011) in mind, one would likely have expected a larger response on these margins, especially given the large increase in exports overall. In contrast, the non-results for destinations and products accord quite naturally with the model presented above. In particular, the results are consistent with the two main implications highlighted in Section 2.4, that product scope may be relatively insensitive to subsidies to market-access costs (first implication), even as market penetration and exports rise with such subsidies (second implication).

Although a full meta-analysis of the export-promotion literature is beyond the scope of this paper, in Appendix E we briefly compare our findings to those of the most closely related non-experimental study, the Cadot et al. (2015) analysis of the previous-generation export-promotion program in Tunisia, called FAMEX II, and we discuss possible reasons for the differences in our results.

6.3 Other Outcomes

The rich combination of datasets we have collected allows us to explore the mechanisms underlying the reduced-form relationship between the matching grants and exports documented in Table 3. In this subsection, we consider the impacts of the matching grants on a variety of other outcomes. When the frequency of zeros is not an issue, we focus on the ANCOVA specification (equation (19)).

We first consider the effects of the program on domestic and total sales. In the presence of capacity constraints, an increase in exports induced by the program might have been

expected to reduce domestic sales — the flip side of the dynamic highlighted by Almunia et al. (2021), in which firms reacted to a domestic slump by selling more abroad. But it does not appear that this was the case. Table 6 reports both ANCOVA (equation (19)) and PPML (equation (20)) results for domestic and total sales in the RNE Export Sample. (Because some firms, particularly "totally exporting" firms, have zero domestic sales, the sample size drops in Column 2 of Panel A, when log domestic sales is the outcome.) There is little evidence of increased exports crowding out domestic sales; the point estimates for domestic sales are positive. There is some suggestive evidence of a positive effect on total sales, in particular a marginally significant estimate in the PPML results in Column 3 of Panel B, although given the noisiness of the data and the size of the standard errors, it is difficult to make definitive statements.[23]

The grants appear not to have led firms to increase employment or wages. Table 7 considers employment and average earnings per worker at the firm level using the RNE Employment Sample. For comparison purposes, Columns 1-2 report estimates for a 0/1 exporter indicator and log exports; the coefficients are similar to those in Table 3. The main new information is in Columns 3-5: we find no effects on log employment, log quarterly earnings per employee, or log total wage bills; the point estimates are -0.01, 0.01, and 0.00 respectively. Given the imprecision in our estimates, we cannot rule out modest positive effects. But we nevertheless view the lack of effects on employment and earnings as surprising, given the substantial effects on exporting. Studies have typically found positive effects of exporting on wages (see e.g. Verhoogen (2008); Brambilla et al. (2012); Hummels et al. (2014); Frías et al. (2024)). One possible explanation relates to Tunisian labor-market institutions. As noted above, sectoral bargaining agreements are prevalent and often binding at the firm level, which may explain the lack of wage effects. Given high costs of hiring and firing in Tunisia, many firms prefer to hire workers informally or on short-term contracts (Rijkers et al., 2014; Angel-Urdinola et al., 2015). These may not show up, or may show up only partially, in the RNE employment numbers.[24] Another possible explanation is that the grants simply led to reduced slack,

[23] Note that the estimates for exports, domestic sales, and total sales are consistent with one another — i.e. an adding-up constraint applies. To see this, consider the PPML estimates from Tables 3 and 6, which use the same 377-firm sample (the RNE Export Sample) in all columns. Total sales rise by 12% (Table 6, col. 3) or TND 919,200 (.12 times the control mean of TND 7.66 million), which is approximately USD 306,400. Domestic sales go up by 5% (Table 6, col. 2) or TND 259,500 (.05 times TND 5.19 million), approximately USD 86,500. Exports increase by 27% (Table 3, col. 2) or TND 667,000 (.27 times TND 2.47 million), approximately USD 222,300. The increase in exports plus the increase in domestic sales is very close to the increase in total sales.

[24] Informal employment is not recorded in the RNE, our primary dataset, and short-term employment is

in line with recent research finding that many developing-country firms can increase output without significant increases in input purchases (Egger et al., 2022; Walker et al., 2024).

Rather than increasing employment, it appears that firms' main response to the matching grants was to expand their presence abroad. In our surveys, we asked a series of questions about the actions and expenditures undertaken by firms. Table 8 reports results for several survey outcomes for the Survey Sample: an indicator for whether the firm established a new contract with a foreign distributor, agent, or partner following randomization (Column 1); an indicator for whether the firm established a new foreign affiliate or representative following randomization (Column 2); an indicator for whether the firm participated in an international fair after randomization (Column 3); and indicators for whether the firm had positive spending on certifications, new technology, travel or consulting in the previous calendar year (Columns 4-7). We simply regress the outcomes on an indicator for treatment, strata dummies and round dummies.[25] The statistically significant coefficients are in Columns 1-2; it appears that the main effect of the matching grants was to induce firms to strengthen their ties to destination markets through contracting relationships or through subsidiaries. There are suggestive positive estimates for participating in international fairs, travel and consulting, but these are not statistically significant at conventional levels. As noted above, there is some reason for concern that differential response rates to our survey between treated and control firms led to a lack of balance. To explore the robustness of the patterns, Appendix Table A10 reports specifications similar to Table 8 but where we also control for baseline sales, exporter status, and exports; the results are very similar to Table 8.

The greater presence abroad may in part be responsible for the pattern observed in Panel A of Table 9, namely that treated firms are more likely to be importers. Having a foreign partner or affiliate in a destination may also facilitate search for input suppliers from that market. However, we do not see an effect on the intensive margin of imports (Column 2 of Panel A), nor do we see an effect on the importer margin in the PPML specification (Column 1 of Panel B).

One would expect the program to have had positive effects on firms' profits. Accounting

recorded only if it involves a formal contract and payments to the social security agency.

[25] The fact that the Column 1-3 outcomes are about new actions leads us to prefer this simple specification to the ANCOVA specification (equation (19)), but we also report the latter in Appendix Table A9. The results are very similar.

profits appear in the RNE data, and results for profits as an outcome are reported in Table 10, using the ANCOVA specification of equation (19). In Column 1, the outcome is the level of profits, in millions of 2015 dinars. In Column 2, the outcome is profits as a share of total sales. Profits are notoriously difficult to measure and noisy, and the estimates are not statistically significant. But the point estimates are positive for both measures.

Overall, the results in this subsection support the interpretation that the subsidies for fixed costs of accessing foreign markets led firms to expand their marketing and customer search efforts primarily in destinations to which they were already exporting.

6.4 Heterogeneous Impacts

In this subsection, we briefly explore heterogeneity of program impacts by pre-program characteristics of firms. We focus on the main exporting outcomes using the RNE Export Sample and ANCOVA specification as in Panel A of Table 3. Here we pursue a simple split-sample approach, focusing on dimensions that are suggested by our theoretical framework or that seem particularly salient. In Appendix F, we supplement this simple approach with a data-driven approach using two state-of-the-art machine-learning (ML) methods: Generalized Random Forest (GRF) framework of Athey et al. (2019), and the Generic Machine Learning (GenericML) approach of Chernozhukov et al. (2025).

As noted above, a key institutional feature in the Tunisian context is the difference between "totally exporting" firms, which are administratively outside the Tunisian customs area, and non-totally exporting firms. In the context of our theoretical model, totally exporting firms can be viewed as facing low market-access costs (i.e. low a); the theory then predicts (refer to (17)) that we will see a smaller proportional export response for this group. Panel A of Table 11 reports separate results for non-totally exporting (Columns 1-2) and totally exporting (Columns 3-4) firms. For non-totally exporting firms, we see a marginally significant response along the extensive margin of exporting and a very strong response on the intensive margin. By contrast, the point estimate for the intensive margin for totally exporting firms is zero.[26] That is, the average program effects documented above are entirely driven by the non-totally exporting firms. It appears that the matching grants were of most help to firms that were not already fully integrated into world markets.

[26] Given that the totally exporting firms are all exporters, there is no variation in the 0/1 exporter indicator for this group.

Another implication of our framework is that we would expect exports of smaller, lower-productivity firms to respond more to the subsidies, in proportional terms, than larger firms. (Refer to (18).) Panel B of Table 11 splits the RNE Export Sample by employment, using 50 employees as a cutoff. Effects on the extensive margin (for the 0/1 exporter variable in Columns 1 and 3) are similar. The point estimate of the effect on the intensive margin is larger for smaller firms (Column 2 vs. Column 4), although this difference is not statistically significant. Given the lack of statistical significance, this pattern should not be over-interpreted, but it is worth noting that the difference in point estimates is in the direction predicted by the theory.

Although our theoretical model does not feature explicit quality choices, previous work has suggested that the ability to upgrade quality is an important determinant of export success (Verhoogen, 2023). It is natural to ask whether the effects of the market-access subsidies differ along this dimension. As a proxy for the potential for quality upgrading, we use an indicator for whether a firm has any quality certification at baseline. Panel C of Table 11 presents split-sample results using this indicator. We find little evidence of an effect of the subsidy on firms without a quality certification at baseline — indeed, the point estimate is negative — and strong evidence of an effect on firms with a quality certification.

In Table 12, we use the richness of our data to explore heterogeneity along other salient dimensions. As a matching grant, the Tasdir+ program may alleviate constraints on spending especially for cash-strapped firms, allowing them to undertake profitable investments that would otherwise be outside their budget set. To explore heterogeneity on this dimension, we calculate firms' assets-to-liabilities ratios, using information provided in their Tasdir+ applications. In Panel A of Table 12, we split the RNE Export Sample by whether firms are above or below the median of this ratio. The fact that the intensive-margin coefficient in Column 2 is larger than in Column 4 is suggestive evidence in favor of this hypothesis, although the difference across the subsamples is not statistically significant. Another variable plausibly related to credit constraints is foreign capital presence; it is often thought that firms with foreign capital are less credit constrained than those without. In Panel B of Table 12, we present split-sample evidence for firms with zero vs. non-zero foreign ownership. Approximately 90% of the firms in our sample have zero foreign capital, so the number of firms in Columns 3-4 is small and the standard errors are large. But consistent with the results for the

assets-to-liabilities ratios, we find larger intensive-margin impacts for firms without foreign capital (p-value 0.06). Another suggestive piece of evidence comes from comparing firms that had plans to introduce new products and/or tailor existing products to export destinations to those that had none, in Panel C. The difference across the samples is not statistically significant, but the results suggest that firms that had plans to innovate were better able to take advantage of the matching grants.

The results from the GRF and GenericML approaches presented in Appendix F are somewhat inconclusive, due in part to the fact that our sample size (487 firms, with only 264 firms having positive exports in the post period) is smaller than is typically needed for such applications. Although the GRF approach yields strong evidence of heterogeneity, we are not able to detect overall heterogeneity using the more conservative GenericML approach. But in both approaches we find some evidence consistent with the discussion above. In particular, we find significant heterogeneity along the totally exporting/non-totally exporting and no quality certification/has quality certification dimensions. We also find some evidence that larger firms (measured either by employment or baseline exports) responded less to the subsidies.

7 Calibration

In this section, we undertake a simple calibration exercise to determine whether the magnitudes of our empirical estimates are quantitatively consistent with the theoretical framework we presented in Section 2.[27] We impose plausible parameterizations and draw some measures from our data. We show that large intensive-margin effects (in the neighborhood of 15-40%) and modest extensive-margin effects can be generated under reasonable parameterizations in our model. We present a baseline calibration with specific parameter values and also explore the robustness of findings to alternative choices of parameters. In the interests of simplicity, we abstract from destination choice and treat all destinations as part of a single export market.

For the elasticity of product substitution, σ, we use a baseline value of 6.75, which is in the range typically considered in the literature (Broda and Weinstein, 2006; Simonovska and Waugh, 2014; Fontagné et al., 2022). We allow a wider range of σ in robustness checks, but we note that our model is better able to replicate observed patterns for values of σ

[27] We thank our editors and referees for suggesting such an exercise.

on the higher side of the range typically considered. For the parameter δ governing how market-access costs increase with distance from core competence, we set $\delta = 0.9$ in our baseline calibration. It is convenient to define $\eta \coloneqq \delta + \alpha(\sigma - 1)$ to capture the curvature of market penetration with respect to product scope (refer to (12)) and of product scope with respect to market-access costs in the continuous (non-integer-constrained) case (refer to (13)). We maintain assumption (9), i.e. that $\eta = \delta + \alpha(\sigma - 1) > 0$. For given values of σ and δ, we face a decision about whether to choose a value of η, which pins down α, or to choose α, which pins down η. We opt for the former, which allows us to consider the same product-scope curvature for different values of σ. We set $\eta = 0.925$ in our baseline calibration.

We use the observed information on product scope, G, and exports, E, for each firm to assign values of market-access costs, a, to firms and to pin down the market-level parameter Θ and the cutoffs for different values of product scope, $\{\widehat{a}_k\}$, as given by (14), where $G = k$ for firms with $a \in [\widehat{a}_k, \widehat{a}_{k-1})$. To begin, suppose first that we have values for Θ and $\{\widehat{a}_k\}$; below we show how these values are determined. Given these values, we assign firm-specific market-access costs, a_i, based on the firm's value of exports relative to other firms in the same product-scope bin. Let E_k^{max} be the observed exports of the firm with the highest export revenues among firms with $G = k$ and let n_k be the number of firms in the bin, i.e., $n_k \coloneqq \sum_i \mathbf{1}[G_i = k]$. Then for firm i with exports E_i, we set:

$$a_i = \hat{a}_{k-1} - \left(\hat{a}_{k-1} - \hat{a}_k\right)\left(\frac{E_i}{E_k^{max}}\right)\left(1 - \frac{0.5}{n_k}\right). \tag{21}$$

The $\left(1 - \frac{0.5}{n_k}\right)$ term serves to "jitter" the highest-export observation away from the $\hat{a}_k$ cutoff, so that no firm sits exactly at that boundary; if there is just one observation in a bin, then a_i is set at the midpoint of the $[\widehat{a}_k, \widehat{a}_{k-1})$ range.

Now consider the determination of Θ and $\{\widehat{a}_k\}$. Recall that $\hat{a}_0 = \Theta/\Lambda$ (where Λ is a function of α, σ and δ (or η)) is the cutoff for entering a market at all, and the cutoffs $\hat{a}_k$ are defined in (14). All of these cutoffs scale linearly with Θ. It follows that a_i as assigned in (21) scales linearly with Θ as well. Hence (15) implies that exports at the firm level scale linearly with Θ too. As a consequence, we can pin down Θ by requiring observed average exports to be equal to the theoretically predicted average exports given the

assignment of market access costs described above:

$$\overline{E} = \frac{1}{N}\sum_{i=1}^{N}\left[\Theta - a_i \Lambda G_i^{\delta+\alpha(\sigma-1)}\right]\frac{\sigma G_i^{1-\alpha(\sigma-1)}}{1-\alpha(\sigma-1)}. \tag{22}$$

where N is the number of firms used in the calibration. By the above arguments, the right-hand side is linearly increasing in Θ and this equation pins down the value of Θ and hence the $\{\hat{a}_k\}$ and $\{a_i\}$.

When exploring robustness, we allow σ to range from 3 to 12 in steps of 0.25, δ to vary from -0.1 to 0.9 in steps of 0.1, and η to vary from 0.15 to 0.95 in steps of 0.025. We maintain the restrictions for our Riemann approximation to be well defined, $\delta > -1$ and $\alpha(\sigma-1) < 1$ (from (11)), and the restriction that $\alpha > 0$.[28] When considering robustness, we drop combinations of $(\sigma, \delta, \eta, \alpha)$ that would violate these restrictions.

We implement the calibration with the "always exporters" from the Customs Sample (141 firms, used in Column 2 of Table 4 Panel A). We focus on these firms in part to avoid difficulties that would be inherent in assigning a value of a for non-exporters.[29] Focusing on firms already engaged in international markets tends to lead the calibration to find larger extensive-margin effects on product scope; the choice is conservative in this sense. In this subsample, the average baseline export level is USD 1.6 million. We take the subsidy under the Tasdir+ program to be the average matching grant disbursed for treated firms in this sample of USD 3,083.[30]

We model the program as a proportional reduction in market-access costs, a, such that the average reduction is equal to the average disbursement observed in the data. That is, under the program, we set $a' = a(1-s)$ for treatment firms ($i \in T$), where $s \coloneqq \frac{\sum_{i\in T} 3{,}083}{\sum_{i\in T} a_i}$.

Each firm's post-subsidy scope G' is then recovered by placing a' within the calibrated cutoff sequence. We then calculate pre- and post-subsidy exports for each firm from our expression for total exports, (15), evaluated at (G, a) and (G', a') respectively and given the parameters σ, δ, η and Θ.

With these simulated data in hand, we implement our ANCOVA (equation (19)) and

[28] We impose $\alpha > 0$ as opposed to $\alpha \geq 0$ as in model because boundary points are typically not suitable for simulations.

[29] The lower cutoff for non-exporters is $\widehat{a}_0$ but it would be unclear how to choose the upper cutoff.

[30] We assign zero to firms that did not submit a reimbursement request. The results would be qualitatively unchanged if we used instead the average matching grant disbursed for submitting firms of USD 4,440, reported in Appendix Table A5.

PPML (equation (20)) specifications. We report β_E^{ANCOVA}, β_E^{PPML}, β_G^{ANCOVA}, and β_G^{PPML}, the ANCOVA and PPML coefficients for exports and product scope, respectively. Recall that control firms would have no response to the subsidy by construction.

Appendix Table A12 reports the first 20 parameterizations from this exercise, sorted by distance from our "target" values. In the case of β_E^{ANCOVA} and β_E^{PPML}, the targets are the estimated values for the "always exporters" from the Customs Sample, reported in Appendix Table A13 Column 1: $\beta_E^{ANCOVA} = 0.42$ (s.e. 0.26) and $\beta_E^{PPML} = 0.22$ (s.e. 0.17). We set the targets β_G^{ANCOVA} and β_G^{PPML} equal to zero. Because there is no standard error for the targets for product scope, they do not enter the distance formula.[31] The distance is then calculated as the sum of the squares of standard-error-weighted differences for β_E^{ANCOVA} and β_E^{PPML}: $\left(\frac{\beta_E^{ANCOVA}-0.412}{0.259}\right)^2 + \left(\frac{\beta_E^{PPML}-0.218}{0.169}\right)^2$. For readability, in Appendix Table A12 we only show the 20 parameterizations closest to the targets, which satisfy model restrictions, and for which $\sigma \in [3,7]$, intensive margin effects are at least 50% of the targets, and scope effects are smaller than 1 product in the ANCOVA and 0.2 log points for the PPML. The full set of implemented (admissible) parameterizations is presented in Appendix Figure A6, discussed below.

The first row of Appendix Table A12 corresponds to our baseline calibration. We find an ANCOVA intensive margin on exports of 0.304 log points, and a PPML intensive margin effect of 0.148 or 16%, well within the confidence intervals of our estimated effects for the "always exporters" from the Customs Sample (Appendix Table A13) as well as for the RNE Export Sample (Table 3). For product scope, we find an increase of 0.964 with ANCOVA and 0.134 log points with PPML. Given that average product scope for this sample is 6.6, this represents a change in product scope of about 15%. While the estimates from the simulated data vary, the key point is that there are many combinations of reasonable parameter values that yield estimates of β_E^{ANCOVA} and β_E^{PPML} of similar magnitude as those from the real data, together with modest (if not zero) estimates for product scope, β_G^{ANCOVA} and β_G^{PPML}.

Another way to see this point is to plot the simulated intensive margin effects for all admissible parameterizations (σ, δ, η) in our large grid of 8,288 admissible parameterizations. In Appendix Figure A6, we plot the intensive margin simulated

[31] The estimated effects in our regressions for these 141 firms are -1.01 (s.e. 1.74) for ANCOVA and -0.13 (s.e. 0.21) for PPML (Appendix Table A13 Column 3), but our calibration mechanically cannot produce any negative value.

effects for ANCOVA and PPML specifications against potential values of σ.[32] The figure shows that there are a large number of combinations of our fundamental parameters which produce simulated effects in the ballpark of our empirical estimates.

It is important to emphasize that the simulation does not suggest that the extensive-margin effects are completely absent. The simulated estimates are generally within the confidence intervals of our estimates from the real data, but they suggest modest positive effects of the program on product scope. The reason can be understood in the context of Figure 1: although there are regions of insensitivity of product scope within each step, there may be firms near enough to cutoffs of each step that they are bumped by the program to a different step; this is likely to be especially true for high-product-scope firms. The simulation suggests caution in interpreting the product-scope estimates in the observed data; we interpret our calibration findings as suggesting that the product-scope effects of the matching grants were probably modestly positive, not literally zero (or negative).

How do our quantitative estimates compare to those in the literature? Probably the leading estimate of fixed costs of exporting is from Das et al. (2007). Their approach differs in several ways from ours: their model is dynamic and they estimate one-time sunk costs of entering export markets, while the estimate that comes out of our static model can be considered fixed costs per period (i.e. per year). The comparison is nonetheless instructive. Depending on the sector, they estimate export-entry costs of USD 344,000-430,000 per firm in 1986 dollars (approximately USD 750,000-935,000 in 2015 dollars). In our case, combining (2), (3) and (12) and using the Riemann approximation (10), we have:

$$F \approx a \ln\left(\frac{\Theta}{a\Lambda G^{\eta}}\right)\frac{G^{1+\delta}}{1+\delta} \tag{23}$$

Plugging in the parameter values that we use for our baseline calibration (Table A12 row 1), observed product scope G, and the inferred values of Θ and a_i, we can construct a firm-level estimate of these fixed costs, F_i. We arrive at a median of USD 77,475 with a wide range of variation, with the 10th percentile at approximately USD 9,120 and the 90th at approximately USD 315,474 (see Appendix Table A14). While caution is warranted in comparing numbers across the studies, we note that at an interest rate of

[32] The choice of σ for the horizontal axis is not crucial. Figures with another parameter on the x-axis would convey the same story.

10% the amortized per-period equivalent of the Das et al. (2007) sunk cost of USD 750,000-935,000 would be USD 68,000-85,000 — similar to our median estimate, which we consider to be reassuring. Both sets of numbers highlight the fact that fixed market-access costs of exporting are substantial.

8 Conclusion

We have presented what we believe is the first successful randomized evaluation of a government financial-support program aimed explicitly at promoting exports. A distinctive aspect of the program is that the grants could be used only for fixed market-access costs, not variable costs. Trade theories in the tradition of Melitz (2003) and Bernard et al. (2011) predict that such subsidies will affect the extensive margins of entry into destinations and products but not the intensive margin of sales conditional on entry. Our results contrast with these predictions. We find positive effects of the subsidies on the intensive margin of exports but little evidence of impacts on the extensive margins of destinations or products. These patterns are consistent with the theoretical model we present, which embeds the Arkolakis (2010) idea that fixed costs are required to reach more customers within a given destination. Because of an integer constraint on the number of products sold in a market, product scope and destination entry may be fairly insensitive to shocks to fixed market-access costs over some ranges, but the shocks are still expected to affect market penetration for existing products and destinations.

Given the large effects on export revenues we observe, a natural question is why a government program was necessary to subsidize the export-promotion activities. Why did non-subsidized firms not undertake the same activities on their own? A first point to note is that gains in export revenues do not necessarily translate into gains in profits; exporting may also have incurred higher costs on dimensions that we do not observe. Although our point estimates of the effect of the program on profits are positive, the estimates are noisy and we are not able to reject the null of no difference in profits between treated and control firms. Hence one reason the unsubsidized firms did not engage in exporting to the same extent may simply be that it was not privately optimal to do so. Second, although our statistical power to distinguish among different mechanisms is limited, the patterns of heterogeneity we have presented are consistent

with the hypothesis that more credit-constrained firms saw greater program impacts than less constrained firms. Such constraints could explain why some unsubsidized firms engaged less in exporting than would be privately optimal if they were better-financed. Third, although we do not observe risk attitudes of firms, we note that exporting is an inherently risky activity and risk-aversion on the part of firms (as for instance documented in another African context by Killeen (2025)) may also have limited firms' engagement in exporting, even if the expected return to such engagement was positive.

What are the implications for policy? Relative to the stated goals of the Tasdir+ program — to increase exports overall and to diversify the products sold and the destinations served by Tunisian exporters — our message is mixed. We find large effects on exports overall, but little evidence of diversification on either the product or destination margins. We also find little evidence of impacts on employment, wages, or profits (with the caveat that the latter are noisily measured). The mixed conclusion raises the question of whether the Tunisian government should implement the same or a similar program in the future. While a complete welfare accounting would require structural estimation of a fully specified general-equilibrium model, which is beyond the scope of the paper, three thoughts seem important to keep in mind. First, a lack of diversification effects is not necessarily an argument against the policy. A key motivation for this and similar programs is that exporting may generate positive externalities for other firms, but the externalities from increased intensive-margin exports may be as large as those from entry into new products or markets (Fernandes and Tang, 2014; Buus et al., 2025b). To the extent that increased tax revenue from affected firms is a policy goal, an increase in intensive-margin exports seems again as beneficial as increased entry. Second, our theoretical framework, the experimental results, and our calibration exercise are all consistent with the hypothesis that the Tasdir+ matching grants were simply not large enough to generate much action on the diversification margins, given the substantial costs of market entry that have been estimated in the literature (Roberts and Tybout, 1997; Das et al., 2007) and that we infer from our calibration. Here again, more evidence is needed from studies that exploit greater variation in the sizes of subsidies, especially at the larger end. Third, we have presented clear evidence that the impact of the program was larger for non-totally exporting firms and suggestive evidence that it was larger for smaller and more credit-constrained firms. This in turn suggests that governments may want to target such firms in the future, especially to the extent that

smaller, more credit-constrained firms are prioritized in government preferences.

The foregoing discussion also raises the important question of how generalizable our findings are to other contexts. The non-experimental literature on export promotion has generally found stronger effects on the extensive margin of destinations than on the intensive margin of sales within destinations. What accounts for the differing results in this case? One possibility is that the randomization is better at purging the estimates of unobservable differences between beneficiary and non-beneficiary firms than the matching and difference-in-difference estimators that have typically been employed. Another possibility is that the COVID-19 pandemic made it difficult to explore new markets and encouraged firms to focus more on established sales channels than they would have otherwise. A third possibility is that the specific features of the Tasdir+ program were responsible for the different impacts. In particular, to be eligible for the matching grants, firms had to submit a business plan and have it approved by Tasdir+ staff. Although in principle the plans could be changed, in practice there were frictions in doing so. Firms may have been more comfortable in developing plans for destinations and products that they were familiar with. The question of external validity is a common issue in empirical work and cannot be definitely answered by one study. One hopes that further research (especially more randomized evaluations) will soon allow us to make stronger statements about which aspects of our findings are robust across contexts. But at the least, we interpret our results as providing strong evidence that fixed market-access costs *can* affect the intensive margin of market penetration, and that modeling of the behavior of firms in international markets should take this into account.

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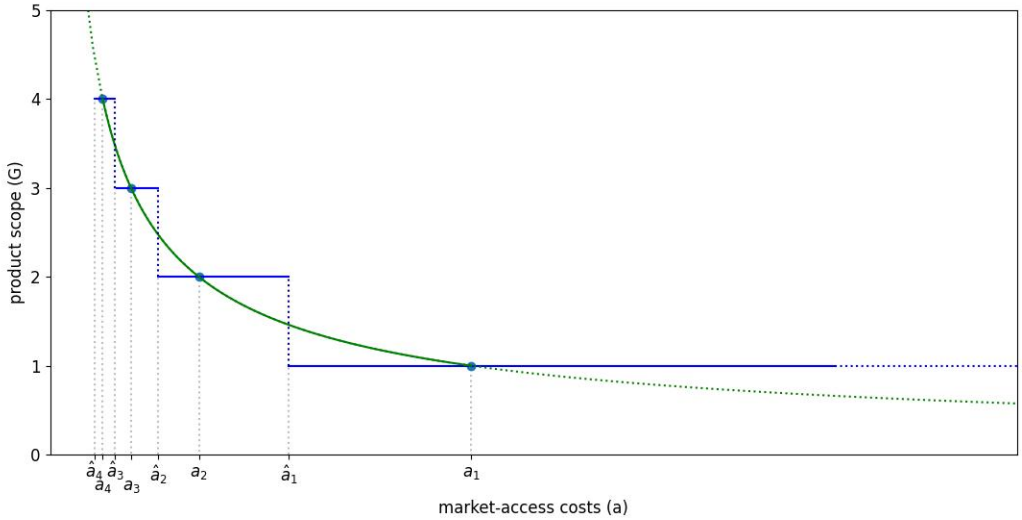
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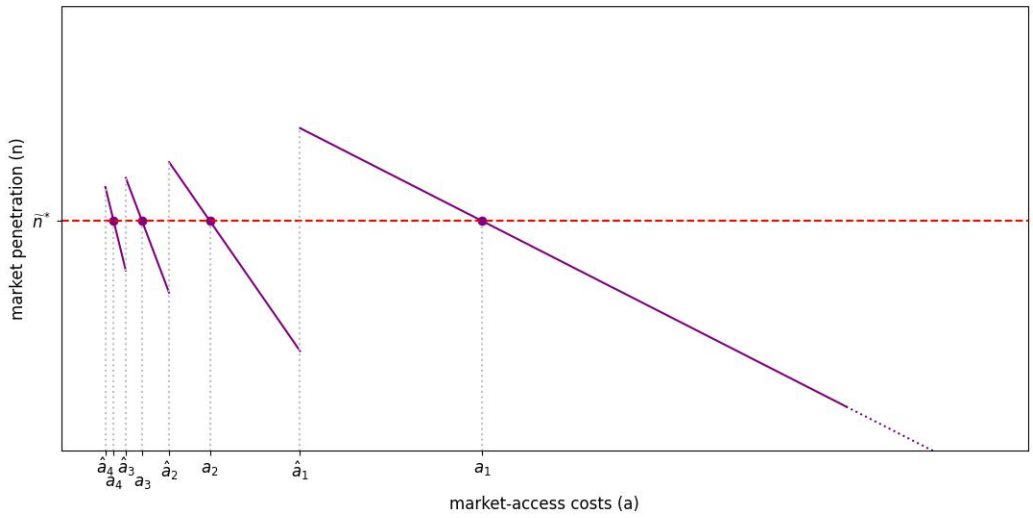
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Figure 1. **Product Scope vs. Market-Access Costs**



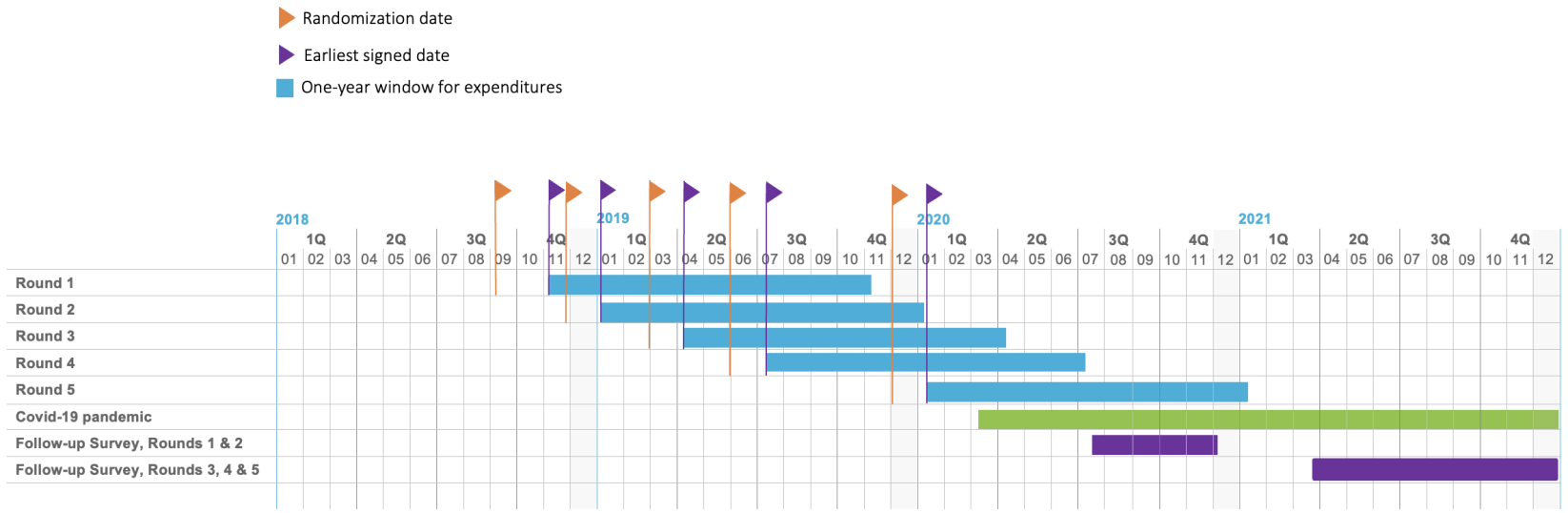
Notes: The figure is generated by a calibration of our model with the following parameter values: $\delta = 1$, $\alpha = 0.5$, $\sigma = 2$, $\Theta = 1$ (and hence $\Lambda = 0.25$). The green curve is $\widetilde{G}^*(a)$, optimal product scope in the relaxed problem (no integer constraint). The blue step function is $G^*(a)$, optimal product scope under the integer constraint. The $\{a_k\}$ are the values of a at which $\widetilde{G}^*(a)$ takes on integer values. As $a \to 0$, $k \to \infty$; for visual clarity, we omit values of $k > 4$ from the graph. The $\widetilde{G}^*(a)$ and $G^*(a)$ curves continue to the right, to the point where $a = \Theta/\Lambda$; again for visual clarity we truncate the graph.

Figure 2. **Market Penetration vs. Market-Access Costs**



Notes: The figure is generated by the same calibration as in Figure 1; see the notes to that figure. The red horizontal dotted line is the optimal market penetration in the relaxed problem (no integer constraint), $\widetilde{n}^*$, which takes on the value 0.90 in this calibration. (The y-axis extends from 0.8 to 1.0.) The sawtooth purple curve is optimal market penetration under the integer constraint, $n^*(a)$. For visual clarity, we truncate the graph as in Figure 1.

Figure 3. **Experiment Timeline**



Notes: The dates of the public randomization meetings were Sept. 6, 2018, Nov. 29, 2018, Feb. 28, 2019, May 30, 2019, and Dec. 5, 2019. The figure reports the earliest date of enrollment for a firm from the corresponding randomization round; there was variation in the dates on which enrollment agreements were signed by firms from a given round.

Table 1. **Eligible Expenditures**

Category	Action
Distribution	Marketing/sales activities at points of sale
	Search costs in distribution channels
Firm development	Acquisition of export development software tools
	Certification fees
	Consulting in the creation of the export business plan
	Consulting in the implementation of the export business plan
	Development of skills related to exports
	Other actions related to export development
	Support for setting up an export unit
Foreign office set-up	Consulting in the creation of the export business plan
	Consulting in the implementation of the export business plan
	Incorporation costs (lawyer fees, publication, etc.)
	Missions abroad to follow-up on the office set-up
	Missions abroad to prepare the office set-up
	Promotional actions linked to establishment abroad
	Rent for office abroad
	Technical and/or commercial assistance linked to implementation
Product development	Acquisition of brands
	Adaptation of packaging to the target market(s)
	Analysis, control and testing of products intended for export
	Certification and approval of products (Halal, CE marking, etc.)
	Creation of brands
	Design/adaptation of products for target markets (prototyping, new collections)
	Registration of brands
Promotion	Creation of promotional materials
	Design and implementation of digital actions
	Organization of events, open houses etc.
	Participation in exhibits, fairs, and B2B meetings
	Website creation
	Written, audiovisual and web advertising actions
Prospecting	Broker commissions
	Development and/or acquisition of market studies
	Invitation/hosting of buyers and contractors
	Participation in calls for tenders
	Prospecting missions
	Subscription to reviews and websites
	Visits to exhibits and fairs

Notes: Translated by authors. Source is Tasdir+ internal documents, also publicized on Tasdir+ website (url: http://www.cepex.nat.tn/article/article.php?id=233).

Table 2. **Balance, Randomization Sample**

	(1) Control Mean/SD	(2) Treatment Mean/SD	(3) P-value
Totally Exporting firm	0.28	0.31	0.45
	(0.45)	(0.46)	
Age of firm (as of randomization)	14.59	14.54	0.96
	(10.78)	(10.66)	
Domestic capital share	96.89	96.66	0.90
	(13.04)	(12.70)	
Employment	39.98	44.52	0.56
	(70.52)	(114.31)	
Sales (millions 2015 dinars)	6.97	6.28	0.51
	(15.23)	(12.60)	
Exporter	0.71	0.73	0.56
	(0.46)	(0.45)	
Exports (millions 2015 dinars)	2.28	2.07	0.86
	(7.24)	(6.29)	
N	206	281	

Notes: Source is application data for reference year (2017 for Rounds 1-4, 2018 for Round 5) for Randomization Sample. (See Section 4 for details.) Standard deviations in parentheses. First row reports 0/1 indicator for being a "totally exporting" firm (administratively in a free-trade zone). Sales and exports are winsorized at the 3%/97% level. Agricultural firms were not required to report sales, export status, or exports in their applications, hence sample size is slightly smaller for these three variables (N=473, instead of 487). P-values in Column 3 are from OLS regressions of variable on treatment indicator controlling for round and stratum fixed effects. F test of joint null of no treatment-control differences (conditional on round, stratum fixed effects) for first four variables (N=487) has p-value 0.92; for all variables (N=473), the p-value also happens to be 0.92. Monetary values were deflated to 2015 dinars using the CPI provided by INS. The average exchange rate over our study period was approximately 3 TND/USD and we use that rate throughout the paper. Additional details are in Section 4 and Appendices C and D.

Table 3. **Exports, Two-Part ANCOVA and PPML**

	Dependent variable:	
	exporter (0/1)	exports
	(1)	(2)
A. ANCOVA (exports in logs)		
Treated	0.05	0.39**
	(0.04)	(0.19)
Dep. var., refyear	0.61***	0.63***
	(0.05)	(0.05)
R2	0.46	0.65
N	377	244
Strata dummies	Y	Y
Round dummies	Y	Y
Mean of dep. var. (level)	0.66	3.92
B. PPML (exports in levels)		
Treated × Post	0.09	0.24*
	(0.06)	(0.15)
Proportional effect: $\exp(\hat{\beta}) - 1$	0.10	0.27
	(0.07)	(0.19)
Pseudo R2	0.18	0.82
N (obs.)	754	754
N (firms)	377	377
Firm FE	Y	Y
Year FE	Y	Y
Mean of dep. var. (level)	0.66	2.47

Notes: Sample is RNE Export Sample, for which sales and exports information is available in RNE in reference year and 2021. The reference year (refyear) is 2017 for Rounds 1-4 and 2018 for Round 5. Panel A reports estimates of equation (19) in text. Panel B reports Poisson Pseudo Maximum Likelihood (PPML) estimates of equation (20). Column 1 outcome is a 0/1 indicator for whether the firm has positive exports. Column 2 outcome is ln(exports) in Panel A, level of exports in Panel B. Means of dependent variables are for control firms at endline (2021). Column 2 dependent variable means are for the level of exports (including in Panel A). The second row of Panel B transforms the parameter estimate from the first row so that it can be interpreted as a proportional effect. In Panel B, errors are clustered at the firm level. Exports are in millions of 2015 dinars. Exchange rate: 3 TND/USD. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. **Numbers of Destinations and Products**

	Dependent variable:			
	exporter (0/1)	exports	# destinations	# products
	(1)	(2)	(3)	(4)
A. ANCOVA (exports in logs)				
Treated	0.04	0.41	-0.21	-0.53
	(0.05)	(0.26)	(0.34)	(1.16)
Dep. var., refyear	0.54***	0.70***	0.81***	0.78***
	(0.06)	(0.07)	(0.04)	(0.09)
R2	0.44	0.60	0.75	0.42
N	210	141	210	210
Strata dummies	Y	Y	Y	Y
Round dummies	Y	Y	Y	Y
Mean of dep. var. (level)	0.74	5.14	3.54	5.96
B. PPML (exports in levels)				
Treated × Post	0.05	0.34*	0.00	-0.09
	(0.07)	(0.20)	(0.10)	(0.21)
Proportional effect: $\exp(\hat{\beta}) - 1$	0.05	0.41	0.00	-0.09
	(0.08)	(0.28)	(0.10)	(0.19)
Pseudo R2	0.17	0.81	0.61	0.69
N (obs.)	420	420	420	420
N (firms)	210	210	210	210
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Mean of dep. var. (level)	0.74	3.44	3.54	5.96

Notes: Sample is Customs Sample, the non-service firms in RNE Export Sample, with zeros imputed for firms that do not appear in the customs records in a given year. The reference year (refyear) is 2017 for Rounds 1-4 and 2018 for Round 5. Panel A reports estimates of equation (19) in text. Panel B reports Poisson Pseudo Maximum Likelihood (PPML) estimates of equation (20). Column 2 outcome is ln(exports) in Panel A and the level of exports in Panel B. # destinations is number of countries to which firm has positive exports. # products is number of distinct 11-digit trade categories in which firm has positive exports. Means of dependent variables are for control firms at endline (2021). Column 2 dependent variable means are for the level of exports (including in Panel A). The second row of Panel B transforms the parameter estimate from the first row so that it can be interpreted as a proportional effect. In Panel B, errors are clustered at the firm level. Exports are in millions of 2015 dinars. Exchange rate: 3 TND/USD. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. **Numbers of *New* Destinations and Products**

	Dependent variable:					
	any new dest. (0/1)	# new dests.	any new targeted dest. (0/1)	# new targeted dests.	any new product (0/1)	# new products
	(1)	(2)	(3)	(4)	(5)	(6)
A. OLS						
Treated	0.03	-0.14	0.07	0.10	0.08	-0.42
	(0.07)	(0.33)	(0.06)	(0.08)	(0.07)	(1.17)
R2	0.17	0.23	0.11	0.12	0.15	0.22
N	210	210	210	210	210	210
Strata dummies	Y	Y	Y	Y	Y	Y
Round dummies	Y	Y	Y	Y	Y	Y
Mean of dep. var.	0.57	1.67	0.17	0.19	0.60	3.97
B. PPML						
Treated	0.04	-0.08	0.33	0.42	0.13	-0.10
	(0.11)	(0.17)	(0.27)	(0.29)	(0.10)	(0.27)
Proportional effect: $\exp(\hat{\beta}) - 1$	0.04	-0.07	0.39	0.52	0.14	-0.10
	(0.11)	(0.16)	(0.38)	(0.44)	(0.12)	(0.25)
Pseudo R2	0.04	0.17	0.08	0.10	0.03	0.23
N (obs.)	210	210	210	210	210	210
Strata dummies	Y	Y	Y	Y	Y	Y
Round dummies	Y	Y	Y	Y	Y	Y
Mean of dep. var.	0.57	1.67	0.17	0.19	0.60	3.97

Notes: Sample is Customs Sample, the non-service firms in RNE Export Sample, with zeros imputed for firms that do not appear in the customs records in a given year. Panel A reports OLS regressions of dependent variable on treatment indicator, with strata and round dummies in 2021 data. Panel B reports PPML, also with strata and round dummies in 2021 data. New destinations and products are those with positive exports in 2021 and zero exports in reference year (2017 for Rounds 1-4, 2018 for Round 5). New targeted destinations are new destinations included in list of 2-3 targeted destinations in Tasdir+ applications. A product is defined as an 11-digit trade category. Means of dependent variables are for control firms at endline (2021). The second row of Panel B transforms the parameter estimate from the first row so that it can be interpreted as a proportional effect. In Panel B, errors are clustered at the firm level. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. **Domestic & Total Sales**

	Dependent variable:		
	sells domestically	dom. sales	tot. sales
	(1)	(2)	(3)
A. ANCOVA (dom. and total sales in logs)			
Treated	0.03	0.08	0.08
	(0.02)	(0.10)	(0.07)
Dep. var., refyear	0.82***	0.73***	1.00***
	(0.02)	(0.04)	(0.04)
R2	0.85	0.86	0.90
N	377	288	377
Strata dummies	Y	Y	Y
Round dummies	Y	Y	Y
Mean of dep. var. (level)	0.79	6.70	7.66
B. PPML (dom. and total sales in levels)			
Treated × Post	0.03	0.05	0.11*
	(0.02)	(0.06)	(0.06)
Proportional effect: $\exp(\hat{\beta}) - 1$	0.03	0.05	0.12*
	(0.02)	(0.07)	(0.07)
Pseudo R2	0.19	0.88	0.85
N (obs.)	754	754	754
N (firms)	377	377	377
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Mean of dep. var. (level)	0.79	5.19	7.66

Notes: Sample is RNE Export Sample, for which sales and exports information is available in RNE in reference year and 2021. The reference year (refyear) is 2017 for Rounds 1-4 and 2018 for Round 5. Panel A reports estimates of equation (19) in text. Panel B reports Poisson Pseudo Maximum Likelihood (PPML) estimates of equation (20). Column 1 outcome is a 0/1 indicator for whether the firm has positive domestic sales. Column 2-3 outcomes are log domestic and total sales in Panel A, levels of domestic and total sales in Panel B. Means of dependent variables are for control firms at endline (2021). Columns 2-3 dependent variable means are for the level of domestic and total sales (including in Panel A). The second row of Panel B transforms the parameter estimate from the first row so that it can be interpreted as a proportional effect. In Panel B, errors are clustered at the firm level. Domestic and total sales are in millions of 2015 dinars. Exchange rate: 3 TND/USD. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. **Employment**

	Dependent variable:				
	exporter (0/1)	ln(exports)	ln(emp.)	ln(avg qtr earnings)	ln(wage bill)
	(1)	(2)	(3)	(4)	(5)
Treated	0.04	0.40*	-0.01	0.01	0.00
	(0.04)	(0.20)	(0.06)	(0.03)	(0.07)
Dep. var., refyear	0.61***	0.67***	0.87***	0.76***	0.85***
	(0.05)	(0.05)	(0.03)	(0.04)	(0.03)
R2	0.48	0.66	0.90	0.75	0.88
N	327	209	327	327	327
Strata dummies	Y	Y	Y	Y	Y
Round dummies	Y	Y	Y	Y	Y
Mean of dep. var. (level)	0.66	4.18	64.60	11.14	712.73

Notes: Sample is RNE Employment Sample, the subset of RNE Export Sample for which employment and earnings (from social security agency) are available in RNE in reference year and 2021. The reference year (refyear) is 2017 for Rounds 1-4 and 2018 for Round 5. Table reports ANCOVA estimates of equation (19) in text. Means of dependent variables are for control firms at endline (2021). Columns 2-5 dependent variable means are for levels (not logs). Earnings and wage bill are in thousands of 2015 dinars. Exchange rate: 3 TND/USD. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. **Survey Outcomes**

	Dependent variable:						
	new contract with foreign dist./ agent/partner	new foreign affiliate/ representative	participated in int'l fair	spent on certifications	spent on new tech.	spent on travel	spent on consulting
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.12*	0.09**	0.07	0.03	0.01	0.07	0.07
	(0.06)	(0.04)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)
R2	0.32	0.34	0.29	0.22	0.22	0.24	0.23
N	204	204	204	204	204	204	204
Strata dummies	Y	Y	Y	Y	Y	Y	Y
Round dummies	Y	Y	Y	Y	Y	Y	Y
Mean of dep. var.	0.24	0.06	0.23	0.19	0.54	0.57	0.30

Notes: Sample is Survey Sample, omitting round 5 firms. Table reports OLS estimates of simple regressions of dependent variable on treatment indicator, strata and round dummies. Dependent variable in Column 1 is indicator for whether firm contracted with distributor, local agent or partner in foreign market since randomization. In Column 2, it is indicator for having established a new foreign affiliate or representative. In Column 3, it is indicator for having participated in international fair/expo. In Columns 4-7, they are indicators for having positive spending in indicated category in calendar year before survey. ANCOVA specifications including the dependent variable at baseline are reported in Appendix Table A9. Means of dependent variables are for control firms at endline. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9. **Imports**

	Dependent variable:	
	importer (0/1)	imports
	(1)	(2)
A. ANCOVA (imports in logs)		
Treated	0.09**	-0.02
	(0.04)	(0.18)
Dep. var., refyear	0.50***	0.85***
	(0.06)	(0.05)
R2	0.61	0.78
N	210	161
Strata dummies	Y	Y
Round dummies	Y	Y
Mean of dep. var. (level)	0.79	9.30
B. PPML (imports in levels)		
Treated × Post	0.09	-0.06
	(0.06)	(0.12)
Proportional effect: $\exp(\hat{\beta}) - 1$	0.09	-0.06
	(0.06)	(0.11)
Pseudo R2	0.13	0.86
N (obs.)	420	420
N (firms)	210	210
Firm FE	Y	Y
Year FE	Y	Y
Mean of dep. var. (level)	0.79	7.10

Notes: Sample is Customs Sample, the non-service firms in RNE Export Sample, with zeros imputed for firms that do not appear in the customs records in a given year. The reference year (refyear) is 2017 for Rounds 1-4 and 2018 for Round 5. Panel A reports estimates of equation (19) in text. Panel B reports Poisson Pseudo Maximum Likelihood (PPML) estimates of equation (20). Column 2 outcome is ln(imports) in Panel A and the level of imports in Panel B. Means of dependent variables are for control firms at endline (2021). Column 2 dependent variable means are for the level of imports (including in Panel A). The second row of Panel B transforms the parameter estimate from the first row so that it can be interpreted as a proportional effect. In Panel B, errors are clustered at the firm level. Imports are in millions of 2015 dinars. Exchange rate: 3 TND/USD. Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10. **Profits**

	Dependent variable:	
	profit (level)	profit/sales
	(1)	(2)
Treated	0.19	0.03
	(0.12)	(0.03)
Dep. var., refyear	0.43***	0.45***
	(0.05)	(0.06)
R2	0.31	0.23
N	341	341
Strata dummies	Y	Y
Round dummies	Y	Y
Mean of dep. var.	0.40	0.02

Notes: Sample is firms in RNE Export Sample for which profits are reported both in the reference year and 2021. Table reports ANCOVA estimates of equation (19) in text. Column 1 dependent variable is level of reported profits, in millions of 2015 dinars. Column 2 dependent variable is ratio of profits to sales in same year. The reference year is 2017 for Rounds 1-4 and 2018 for Round 5. Means of dependent variables are for control firms at endline (2021). Additional details are in Section 4 and Appendices C and D. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11. **Heterogeneity by Trade Regime, Size, Quality Certification**

	exporter (0/1) (1)	ln(exports) (2)	exporter (0/1) (3)	ln(exports) (4)
A. By Trade Regime				
	Non-totally exporting		Totally exporting	
Treated	0.08*	0.71**		0.00
	(0.05)	(0.28)		(0.20)
Dep. var., refyear	0.52***	0.55***		1.00***
	(0.06)	(0.07)		(0.09)
R2	0.43	0.61		0.87
N	277	144		100
Mean of dep. var. (level)	0.54	2.72		5.66
B. By Initial Employment				
	<50 employees		≥50 employees	
Treated	0.05	0.48*	0.04	0.34
	(0.05)	(0.26)	(0.06)	(0.27)
Dep. var., refyear	0.63***	0.70***	0.54***	0.53***
	(0.06)	(0.07)	(0.09)	(0.06)
R2	0.47	0.67	0.52	0.53
N	240	139	137	105
Mean of dep. var. (level)	0.58	0.96	0.80	7.59
C. By Baseline Quality Certification				
	No certification		Has certification	
Treated	0.07	-0.12	-0.02	0.60**
	(0.05)	(0.29)	(0.06)	(0.24)
Dep. var., refyear	0.62***	0.44***	0.49***	0.82***
	(0.06)	(0.07)	(0.11)	(0.06)
R2	0.47	0.58	0.50	0.81
N	241	137	123	98
Mean of dep. var. (level)	0.57	2.71	0.83	5.97

Notes: Table reports regressions similar to Panel A of Table 3 using the RNE Export Sample split along the indicated dimensions. All regressions include strata and round dummies. Means of dependent variables are for control firms at endline (2021). Columns 2 and 4 dependent variable means are for the level of exports. Quality certifications variable is missing for 13 firms in the application data, hence the smaller number of observations in Panel C. Additional details are in Section 4 and Appendices C and D. T tests of null of no coefficient differences for exporter have p-values 0.98 and 0.66 for Panels B and C, respectively; for ln(exports), they have p-values 0.01, 0.61, and 0.03 for Panels A, B, and C, respectively. Exports are in millions of 2015 dinars. Exchange rate: 3 TND/USD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12. **Heterogeneity by Financial Condition, Business Plan Characteristics**

	exporter (0/1) (1)	ln(exports) (2)	exporter (0/1) (3)	ln(exports) (4)
A. By Financial Condition				
	Low assets/liabilities		High assets/liabilities	
Treated	0.05	0.66**	0.07	0.33
	(0.06)	(0.26)	(0.06)	(0.31)
Dep. var., refyear	0.52***	0.69***	0.65***	0.61***
	(0.07)	(0.08)	(0.07)	(0.07)
R2	0.44	0.72	0.54	0.68
N	201	125	176	119
Mean of dep. var. (level)	0.66	3.46	0.65	4.42
B. By Presence of Foreign Capital				
	No foreign capital		Has foreign capital	
Treated	0.02	0.46**	0.09	-0.61
	(0.04)	(0.20)	(0.19)	(1.32)
Dep. var., refyear	0.62***	0.60***	0.74***	0.94**
	(0.05)	(0.05)	(0.24)	(0.36)
R2	0.48	0.66	0.71	0.83
N	343	218	34	26
Mean of dep. var. (level)	0.65	2.87	0.73	12.69
C. By Plan to Spend on Product Tailoring/Innovation				
	No tailoring/innovation plan		Has tailoring/innovation plan	
Treated	0.07	0.22	0.02	0.54
	(0.05)	(0.22)	(0.07)	(0.40)
Dep. var., refyear	0.60***	0.62***	0.64***	0.68***
	(0.06)	(0.06)	(0.08)	(0.10)
R2	0.48	0.73	0.55	0.56
N	259	163	118	81
Mean of dep. var. (level)	0.62	4.21	0.76	3.30

Notes: Table reports regressions similar to Panel A of Table 3 using the RNE Export Sample split along the indicated dimensions. All regressions include strata and round dummies. Means of dependent variables are for control firms at endline (2021). Columns 2 and 4 dependent variable means are for the level of exports. Additional details are in Section 4 and Appendices C and D. T tests of null of no coefficient differences for exporter have p-values 0.52, 0.41, and 0.73 for Panels A, B and C, respectively; for ln(exports), they have p-values 0.35, 0.06, and 0.35 for Panels A, B, and C, respectively. Exports are in millions of 2015 dinars. Exchange rate: 3 TND/USD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.