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ABSTRACT

Governments spend over a billion US dollars annually on firm support programs, yet application rates are low and outcomes modest. Attracting enough and the right firms may alter the program's effect and statistical power to detect it. Yet, we document that most firm program evaluations don't report recruitment strategies. We conduct two email experiments involving 5000 SMEs while recruiting for two export support programs in Tunisia, tracking each communication channel's contribution to registrations. In experiment 1, we find goal-specific messages targeting firms' supply or demand side constraints attract fewer but better-performing firms. In experiment 2, we find an influencer video emphasizing program benefits attracts better-performing female-led firms, while reducing participation costs via free childcare attracts less-performing firms managed by younger female entrepreneurs with children. Finally, we show open communication channels attract more underrepresented firms. In general, the findings suggest recruitment strategies substantially impact sample size and composition.

Keywords:Firms;Recruitment;Sampling; Experiment; Export; Female

Attracting Firms to Government Programs: Theory and Evidence from Randomized Controlled Trials in Tunisia

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ABSTRACT
Governments spend over a billion US dollars annually on firm support programs, yet application rates are low and outcomes modest. Attracting enough and the right firms may alter the program's effect and statistical power to detect it. Yet, we document that most firm program evaluations don't report recruitment strategies. We conduct two email experiments involving 5000 SMEs while recruiting for two export support programs in Tunisia, tracking each communication channel's contribution to registrations. In experiment 1, we find goal-specific messages targeting firms' supply or demand side constraints attract fewer but better-performing firms. In experiment 2, we find an influencer video emphasizing program benefits attracts better-performing female-led firms, while reducing participation costs via free childcare attracts less-performing firms managed by younger female entrepreneurs with children. Finally, we show open communication channels attract more under-represented firms. In general, the findings suggest recruitment strategies substantially impact sample size and composition.

1. Introduction

Governments spend over a billion US Dollars annually on programs to promote firms management practices, innovation or export (McKenzie et al. 2021). Yet, average effects are often modest (McKenzie et al. 2021), and few firms decide to enrol, resulting in failed or simplified scientific evaluations (Campos et al. 2017; Fuller 2021; Goldstein 2011).[1] It is puzzling that few firms enrol, given program participation is free or highly subsidized and, conditional on application, admission rates tend to be high (Afcha and Chu (2023)). Why is this the case? In this paper, we concentrate on how recruitment efforts by implementing agencies may affect firms' decision to enroll in a government support program.[2]

We focus on two information frictions affecting firms' decisions to enrol in a program. Firstly, firms may not register for a program if they lack awareness of the opportunity (Freixanet 2012; Cruz 2014; Lederman, Olarreaga, and Payton 2006; Kedia and Chhokar 1986; Negri, Lemos, and Negri 2006). This may be the result of insufficient communication. For example, Campos et al. (2017) report limited marketing efforts by implementing agencies as one reason why several World Bank-funded matching grant programs led to a small number of applications. Limited disclosure of information can also be strategic. For example, Goldstein (2011) illustrates an example where the partner agency only communicated information about the program to its members. Limited communication may reduce sample size and alter sample composition since only well-connected firms, such as existing clients or cronies, may be aware of the program. Limited communication can also have a gender dimension since female entrepreneurs have access to smaller networks (World Bank 2020; Drine and Grach 2012). In such cases, the program's potential effect is not a function of the interested firms in the total population. It is limited to connected businesses and their characteristics, reducing the external validity of the findings and potentially the program's impact.

Secondly, the framing of communication may change firms' perceived benefit and, therefore, their decision to enroll in a program. Government support programs aim to incentivize firms to engage in an activity, such as innovation or export, which governments consider is individually beneficial to the firm and has positive societal externalities. Yet, firms may perceive the benefits and costs differently, depending on the framing of the communication and the firms' priors. For example, Breinlich et al. (2017) provide firms in the UK with information about the benefits of exporting and find that not-yet exporting firms, the most important target group, are discouraged. A discouragement effect of positively intended information about the benefits of exporting is corroborated in a similar study among textile firms in Vietnam (Kim et al. 2018). A discouragement effect from positively intended information may also occur if entrepreneurs cannot relate to or lack representative role models (Serra 2022;

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[1] A first wave of unsuccessful randomized controlled trials (RCTs) with firms was published in a World Bank report titled "Learning from experiments that never happened" (Campos et al. 2017). Six out of the seven failed trials suffered from low take-up. Similarly, a review of two European experimentation funds dedicated to evaluating firm support programs found that "recruitment [proved] much more difficult than originally thought" (Fuller 2021). Goldstein (2011) confirms that low take-up is a common challenge for evaluating firm support programs using encouragement designs. A common response to recruitment difficulties is simplifying research design, e.g., by reducing the number of treatments and packaging treatments, which prevents more refined research findings.

[2] We use the term recruitment, but it could be interchangeably replaced with "outreach", "communication", or "sampling" strategy. We mean the efforts to motivate firms to enroll and participate in government support programs.

Jayachandran 2021). This phenomenon may be particularly salient among female entrepreneurs in contexts where gender norms associate entrepreneurship primarily or exclusively with men. For example, only 8 of the 59 papers we reviewed stated that they had gender-sensitive recruitment campaigns.

Overall, there is little systematic knowledge about the effect of recruitment or sample strategy on sample size and composition. The first reason is that most studies do not report how firms were recruited to the program. Of the 59 (quasi-) experimental evaluations of firm support programs we reviewed, 34 or more than half did not document their recruitment strategy. Secondly, there is little academic evidence, e.g., from randomized controlled trials or A/B testing, with the exceptions of Crama et al. (2022) and Broughton et al. (2019), as the issue is sidelined as an implementation challenge. As a result, we know little about attracting more firms and diversifying applicants, e.g. increasing female entrepreneurs' participation in government support programs.

We conduct two randomized experiments in the context of two export support programs involving around 5000 SMEs in Tunisia to provide novel evidence on how recruitment strategies relate to program registration.

In the first experiment, we sent the same email to around one thousand SMEs in each group but vary the email subject line and trace firms' responses via email opening, link clicks, and registration rates. We test two messages compared to a control group "call for applications" subject line: a supply-side message focusing on productivity gains from quality control and a demand-side message focusing on winning international clients' trust via signalling export product quality. Against our expectation, we find that the control group email subject line attracted most firms. The effect is quite substantial, given it resulted from a few changes in words and comes at no cost. We would have attracted an additional 30 firms, equivalent to 11 percent of the sample size of registered firms, had we only used the neutral email subject. However, the control "call for applications" subject is not strictly optimal. The targeted supply-side productivity and demand-side signalling subject lines attracted ex-ante better-performing firms. This suggests neutral call-for-action messages maximize reach, while goal-specific messages appeal to "specialists". Depending on the implementer's objective, either should be used.

In a second experiment, we use the best-performing email subject line of experiment 1 and embed two ways to incentivize participation, particularly from female entrepreneurs, in an export via e-commerce program. The first incentive is a link to the video of a successful female e-commerce entrepreneur who shares her story and encourages entrepreneurs to apply to the program. This incentive emphasizes the program's <u>benefits</u> and seeks to raise registration via a role model and inspiration rather than material support. In contrast, the second incentive offers material support through free childcare during program activities. It focuses on lowering participation <u>costs</u> for participants with childcare responsibilities, predominantly women in the Tunisian context. We find that the female influencer video attracts already successful female entrepreneurs, while free childcare increases registration among firms with lower performance and size. Additional evidence suggests free childcare is essential for younger female entrepreneurs with more children. Overall, the findings suggest that recruitment strategies substantially impact sample size and composition.

This paper makes the following contributions. First, we reveal that few firm-level studies report their recruitment strategies in the literature on private sector development (McKenzie et al. 2021), and export promotion specifically (Makioka 2019). We add to the few existing studies (Crama et al. 2022; Broughton et al. 2019) by showing that different messages and participation incentives affect sample size, how

many firms register, and sample composition, which firms register. We consider this work contributes to understanding the microeconomic decisions that govern firms' decisions, e.g., to participate in government programs in the general literature on firms, development, and industrial policy (Juhász, Lane, and Rodrik 2023) where it is generally assumed firms are interested in receiving public support; the practical experience reveals it is hard to motivate firms to engage in activities, such as export or innovating, which are considered socially beneficial from a developmental point of view. Third, this work complements the literature on political capture, government failure, and industrial development (Evans 1995; Bai et al. 2019; Hellman, Jones, and Kaufmann 2000). We document that more transparent communication channels, particularly social media, help diversify the pool of registered firms, reducing the share of incumbents toward under-represented firms, such as female-managed or remotely-located firms. This is relevant for many governments transitioning from authoritarian to democratic rule and, in general, government agencies and international organizations interested in promoting inclusive economic development. Based on the empirical evidence, we develop a formal model of how recruitment relates to firms' decisions to enrol in support programs. The formal model should help other researchers think through their recruitment efforts and inform future experimental work in this area.

In the following, section 2 describes the design of the two email experiments. Section 3 provides the results, and section 4 concludes.

2. Experimental design

2.1. *Context*

The two experiments are part of the recruitment campaigns of government programs to help SMEs export in Tunisia. The first program offered SMEs information, consulting, and a matching grant to improve product quality control for the international standards required for export. The second program provided SMEs with training in e-commerce and a fully subsidized remake of their websites and social media activities to cater to international clients. The projects were implemented with two Tunisian Ministries, international donors, and the research team. One key objective was to diversify the pool of recipient firms, given strong path dependency and clientelism, at least in previous regimes (Rijkers, Baghdadi, and Raballand 2015; Rijkers, Arouri, and Baghdadi 2016; Arouri, Baghdadi, and Rijkers 2019; Kruse, Martínez-Zarzoso, and Baghdadi 2021), as well as the under-representation of certain socioeconomic groups, such as female entrepreneurs and entrepreneurs outside the coastal centres.

2.2. *Treatments*

Experiment 1 tests whether messages emphasizing supply- or demand-side constraints attract more firms to the export support program. Experiment 2 examines whether making potential benefits more salient or reducing costs to participate in the export support program attracts more firms, focusing specifically on female entrepreneurs. In both experiments, we include a control group (see Figure C2 for an overview).

2.2.1. Experiment 1: Supply vs. Demand-Side Constraints

In experiment 1, we tested three email subject lines. The first subject line, referred to as *Productivity* treatment, seeks to attract firms by emphasizing productivity improvements from investing in export quality (Figure 1, Productivity subject line). Here, the focus is on the supply side. The second subject line, referred to as *Signaling* treatment, underlines firms can attract international clients via quality standards, focusing on the demand side (Figure 1, Signaling subject line). The treatments are compared to a control or business-as-usual subject line "Call for applications".

The subject lines are designed to encapsulate the theoretical constraints and benefits SMEs face when investing in quality control for exporting. Compliance with international quality standards is one of the main fixed export costs for SMEs worldwide, including in Tunisia (Melitz 2003; Baldwin, McLaren, and Panagariya 2000; Cao and Prakash 2011). Quality certification is designed to solve the fundamental economic information asymmetry or chicken-and-egg problem (Akerlof 1970; Bold et al. 2021) where international clients are unwilling to buy local products unless compliant with international quality standards (demand-side), while local SMEs are unwilling to invest in quality control unless international clients are willing to pay for it (supply-side).

2.2.2. Experiment 2: Salience of benefits vs. costs of participation

In experiment 2, we selected the most performing email subject line and tested two nudges placed directly before the link to the registration platform in the outreach email.

The first nudge is a video[3] from a female entrepreneur who built a successful e-commerce business and encourages viewers to register for the e-commerce program (see Figure 1, experiment 2, Influencer video). This nudge emphasizes the benefits of participating in the e-commerce for export program. Few private sector programs have gender-sensitive communication campaigns with female role models, and female entrepreneurs tend to have lower levels of self-confidence (Kirkwood 2009; Koellinger, Minniti, and Schade 2013; Iacovone, Maloney, and McKenzie 2022; Macko and Tyszka 2009; Dalbor, von Friedrichs, and Wincent 2015). Role models may "stir individuals to different life paths" as they act as realizations of the possible (Serra 2022, p.3). For example, Dalton et al. (2020) and Lubega et al. (2021) show that providing best practice or role model videos can increase entrepreneurs' time in the business, adoption of business practices, and business revenue. Accordingly, we hypothesized that the influencer video may motivate entrepreneurs to register, particularly female entrepreneurs who may relate more to the female role model.

The second nudge makes free childcare provision during program activities more salient (see Figure 1, experiment 2, Childcare). Free childcare provision aims to reduce participation costs for entrepreneurs with childcare responsibilities. Existing research has documented that female entrepreneurs are disproportionately affected by household and family responsibilities (Tur-Porcar, Mas-Tur, and Belso 2017; Jayachandran 2021; Fafchamps et al. 2014; Delecourt and Fitzpatrick 2021), which may inhibit their participation in government support programs. One common solution to increase female labor force participation is childcare services (De Barros et al. 2011; Martínez A. and Perticará 2017; Williams 2004). Note that we did not randomize the access to childcare services, advertised openly on the program's website and accessible to all interested entrepreneurs for ethical reasons. Instead, we randomized whether the

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[3]The video can be viewed here.

program's outreach email contained an additional sentence placed in front of the registration link emphasizing the free childcare provision during program activities. We hypothesized that the free childcare service would particularly help female entrepreneurs with children (or other household chores) register for the program.[4]

While the influencer video targets perceived benefits and immaterial psychological barriers, the childcare constitutes tangible support to reduce (female) entrepreneurs' participation costs. Both nudges were also provided to male entrepreneurs. We compare both nudges to a control group that does not receive either nudges.

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[4]For future research, it is interesting to mention that (female) project implementation partners showed little enthusiasm for our proposal to apply previously tested strategies, such as inviting women to participate with a (female) friend or male ally (e.g., a husband, brother or friend), and that some female entrepreneurs even considered additional support targeting female entrepreneurs irrelevant as they felt participants would need to have sufficient intrinsic motivation. Moreover, the actual take-up/demand for childcare services was virtually absent. Qualitative interviews suggest the absence of professional childcare services in Tunisia, a related lack of trust toward professional childcare services, and reliance on extended family members for childcare provision in the cultural context are some explanatory factors.
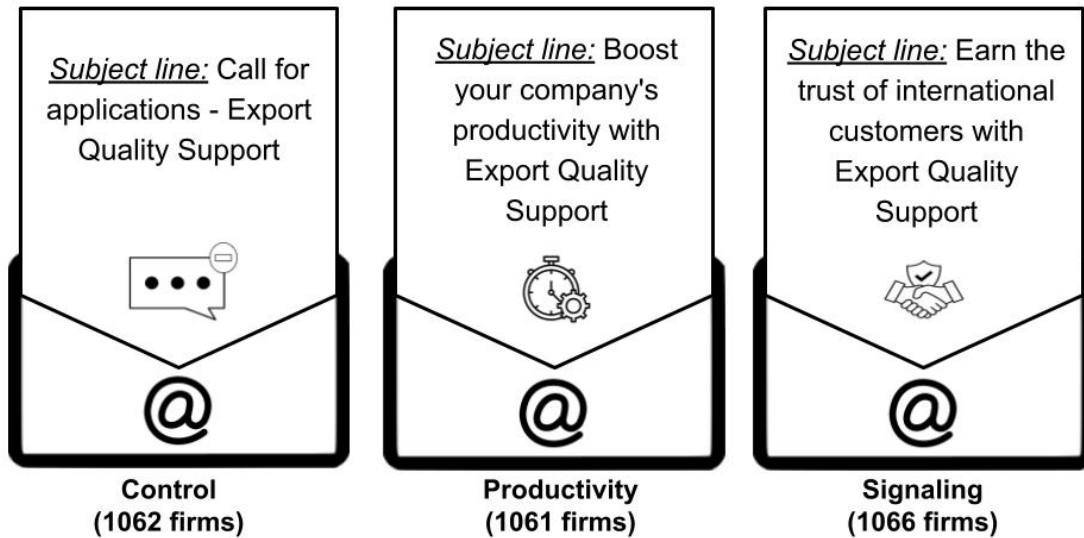
(a) Experiment 1: Supply vs. Demand-Side Constraints
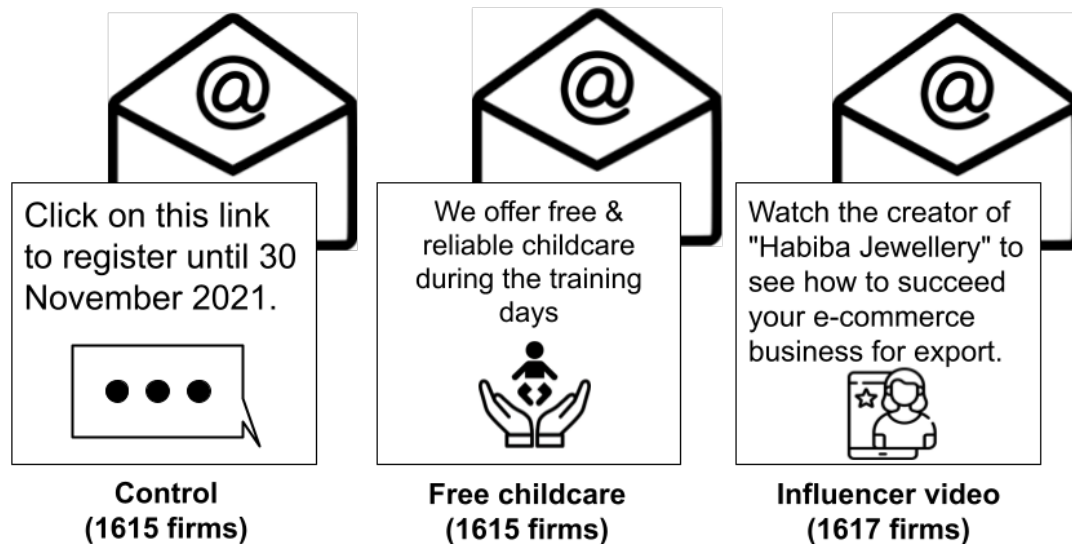
(b) Experiment 2: Benefits vs. Costs of Participation

Figure 1.: Treatments in experiment 1 and experiment 2. In experiment 1, we varied the subject line. In experiment 2, we varied the treatment sentence and positioned it in front of the registration link.

2.3. *Design of the experiments*

The contact information for the sample firms was sourced from the national industrial firm registry, maintained by the National Investment and Innovation Promotion Agency (API).[5] The dataset contains information regarding the firm's email address, the name of the firm, the first and last name of its director, and some information about firm characteristics (e.g. 16 sectors, number of employees). When firms are in-

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[5]The national industrial firm registry can be accessed here.

corporated in Tunisia, they are asked to register in the portal if they have more than 6 employees.

The firm populations for the two experiments were 3189 SMEs in experiment 1 and 4847 SMEs in experiment 2, with approximately 60 percent of the firms figuring in both experiments. We dropped all firms without email addresses or more than 200 employees as the program was limited to SMEs. In experiment 1, we focused exclusively on firms that undertake an industrial activity, excluding services, while we also integrated service firms in experiment 2. Experiment 2 benefited from an additional database from previous programs. This database, composed of 1149 firms, contains information about the name and email of the firm and the first and last name of its director but has no information about the firms' sector, export status, or sales. The director's gender was coded using an algorithm in the case of experiment 2 and was manually coded with the help of local research assistants for experiment 1. In both cases, the gender was assigned based on the first name(s). Names that remained undetected were coded manually, and a quality check of the automatic allocation was performed before the finalization. Ten observations with indeterminable gender based on the first name(s) were dropped. Duplicates were identified via the firm's name and email addresses and were removed.[6]

We randomly allocated the SMEs into three treatment groups in both experiments. We used stratified randomization to guarantee balance on factors impacting a firm's registration probability (Macko and Tyszka 2009; Bruhn and McKenzie 2009). In experiment 1, we stratified on firms' exporter status and sector. The exporter status indicates whether a firm is partially or export-only.[7] The sectoral variable indicates one of the 16 industrial sectors in Tunisia. Table D2 shows randomization achieved balance among the available administrative firm characteristics. In experiment 2, we stratified on CEO gender and sector. We selected gender, as the treatment primarily targeted female entrepreneurs and sectors, as we expected interest in e-commerce would differ by sector. Table D3 illustrates randomization led to balance across the three groups.

We linked the sampling populations with the registered firms via two different ways. In experiment 1, we used firm-specific codes and create an openly accessible and a closed survey, which facilitated identifying the registered firms in the sampling population. In experiment 2, we identified the registered firms by matching the firm names and firm email addresses. We used tracking software in experiment 1 and a direct question at registration in experiment 2 to examine how the firms heard about the program. For further details, please consult appendix B.

2.4.   *Design of the email*

We designed the emails based on the following empirical evidence. Firstly, we used the most successful message from previous experiments (*"You have been selected because we believe your company might be eligible"*) (Broughton et al. 2019). Secondly, we consulted existing work on the average time individuals spent reading an email (8 seconds) and the average number of words people read per second (4 words). Accordingly, we placed the link to the registration platform, which we wanted potential applicants to click on, after roughly 32 words. In between the first sentence and the link, we briefly

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[6]An additional problem is erroneous and inactive email addresses. In appendix B.0.2, we document how we dealt with the problem and provide guidance to others on how to do it better.

[7]In Tunisia, a specific regime called "offshore" is comparable to Special Economic Zones for export-only firms.

presented the core services offered by the program and the application deadline. More-over, we used an official government email address, including the program logo, in the case of experiment 1. We made sure the name of the sender was *Ministry of Industry* and *GIZ* respectively to leverage the partners' authority and legitimacy (Broughton et al. 2019). Finally, we ensured the email subject length remained short and within the length suggested by commercial providers of bulk email programs. The content of all three emails differs only in terms of the subject line in experiment 1 (section 2.2.1) and in one additional sentence presenting the add-on incentive or nudge in the experiment 2 (section 2.2.2). Example emails are provided in figures C4 and C5 in the appendix.

2.5. *Design of the recruitment campaigns*

The email experiments were part of a nationwide communication and recruitment campaign (see Figure C1. The implementing agencies used various communication channels to inform about the program. The call for applications was shared through their communication channels: website, newsletters, Facebook, and LinkedIn. Face-book was chosen as it has the highest market share in Tunisia, and LinkedIn, given it focuses on professionals. In addition, program representatives participated in ra-dio shows, given radio is the second most influential media in Tunisia after television (Media Ownership Monitor Tunisia 2019).

2.6. *Regression Models*

For experiment 1, we regress three binary firm response outcome variables - opened the email, clicked on the link, and registered for the program on binary treatment status indicator variables, controlling for the randomisation strata (Bruhn and McKenzie 2009):

$$response_i = \alpha + \beta_1 productivity_i + \beta_2 signaling_i + \gamma strata_i + \varepsilon_i \tag{1}$$

where i indicates the unit of observation, one of the 3189 SMEs, *productivity* and *signaling* are treatment dummies relative to the control group and *strata* are dummies for the 28 sector-exporter status strata. ϵ represents the error term. We use logistic regression, given outcomes are either zero or one. The sample is limited to firms that received at least one email.

We proceed in the same way for experiment 2 but add interaction terms between the treatment and the gender of the firm representative :

$$\begin{aligned} response_i = \alpha &+ \beta_1 childcare_i + \beta_2 video_i + \beta_3 female_i \\ &+ \beta_4 childcare_i \cdot female_i \\ &+ \beta_5 video_i \cdot female_i \\ &+ \gamma strata_i + \varepsilon_i \end{aligned} \tag{2}$$

where i indicates the unit of observation, one of the 4847 SMEs, *childcare* and *video* are dummies for the free childcare and influencer video treatments relative to the control group and *strata* are dummies for the 34 sector-gender status strata. *fe-male* is a dummy variable indicating the gender of the firm's director. $\beta 4$ and $\beta 5$ are

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the coefficients of the interaction terms between treatments and the firm's managers' gender.

Moreover, we assess whether the results are sensitive to how we assign a gender to the company, distinguishing between the CEO's and the representative's gender as indicated at registration.

3. Results

3.1. *Experiment 1: Specific messages targeting supply or demand side constraints attract less, but better-performing firms*

Small changes in the wording of the email subject line significantly affect the total number and the composition of registered firms. Both the productivity (supply side) and the signalling (demand side) subject line attract fewer firms than the neutral control group "Call for applications" subject line (Figure 2). This holds for the likelihood that firms opened the email, clicked on the link to the registration platform, and registered for the program (see table D4 in the appendix for details). Against our expectations, the neutral control group email subject line outperformed the other two subject lines.
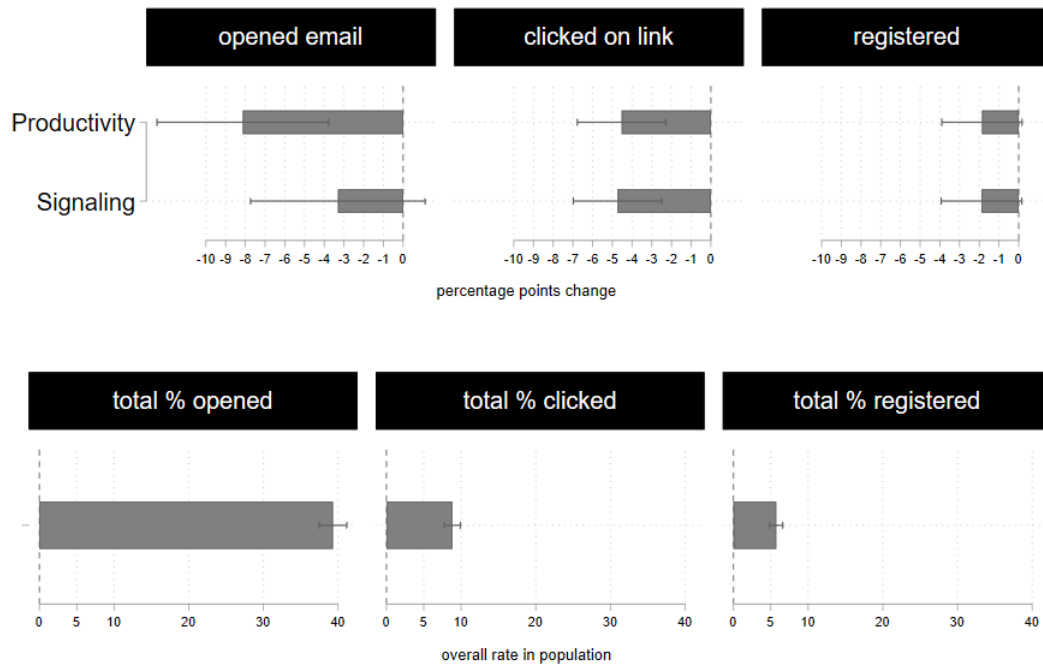

Figure 2.: Top-panel shows percentage point change in average marginal probabilities relative to neutral control group. Bottom-panel provides email opening, click and registration rate in the firm population to assess the magnitude of the effect.

More specifically, seven percent of the firms in the control group registered for the program - 2 percentage points more than in the productivity (supply side) or signaling (demand side) group (Figure 2). This effect is statistically significant at the

10 percent level. In absolute numbers, this corresponds to a difference of around 15 firms per treatment group or 30 firms in total, equivalent to 11 percent of the sample size of registered firms. In other words, had we simply used the neutral control group subject, the results suggest we would have attracted around 300 instead of 266 firms. This effect is quite substantial given that no cost is associated with a change in the email subject's wording.

Opening and click rates provide reassuring evidence that firms' differential registration rates directly affect the email subject lines. In the control group, 43 percent opened the email and 12 percent clicked on the registration link. The average predicted probability of opening the email is 8 percentage points lower in productivity and 3 percentage points lower in the signaling group, with the first effect being statistically significant at 1 percent and the latter being marginally insignificant. Regarding clicking on the registration link, the average predicted probability is 4.5 and 4.7 percentage points lower in the productivity and the signaling group, respectively statistically significant at the 5 percent level. In summary, the neutral or control group subject line had the highest email opening and link click rate, which translated in lower program registrations.

Next, we investigate whether the different email subjects also changed the sample composition or the characteristics of the companies that registered for the program. The firms in the neutral control group differ from those attracted by the productivity and signaling subjects in important ways. On average, firms attracted via the neutral subject line are 10-16 percent more likely not to have a quality certificate, have only around half the capital and export sales in 2020, and have 15-20 fewer employees. These differences are statistically significant at the 90 percent level (see Figure 3, and Table D5 in the appendix for a full balance table overview). Firms attracted via the neutral email are twice as likely to be in the textile sector, which has the lowest average quality control capabilities at baseline across all sectors (see figure C6 in the appendix).

Overall, the neutral, control group subject attracted more firms as it also catered towards firms that were less performing in terms of quality management and export, which were the two main themes of the program, and which were substantially smaller in terms of both capital and number of employees.

Why does the neutral, control group subject have a wider reach, and why does a targeted message attract more specialists? In our view, the most compelling explanation points toward the interaction between firms' priors and the framing of marketing messages, which we formalize in a model (see section A in the appendix). The core idea is that firms' priors and the framing of the marketing message condition whether firms evaluate program participation as beneficial and therefore register or not. Specific marketing messages resonate with specific priors but lose interest from the rest. General framing, in contrast, enables recipients to project their priors to the program, attracting broader interest. Alternative explanations are, for example, that the marketing messages were bad, e.g. because they were too catchy or because economists' and firms' views about the benefits of export quality diverge. The latter, however, does not explain why we observe heterogeneous effects.
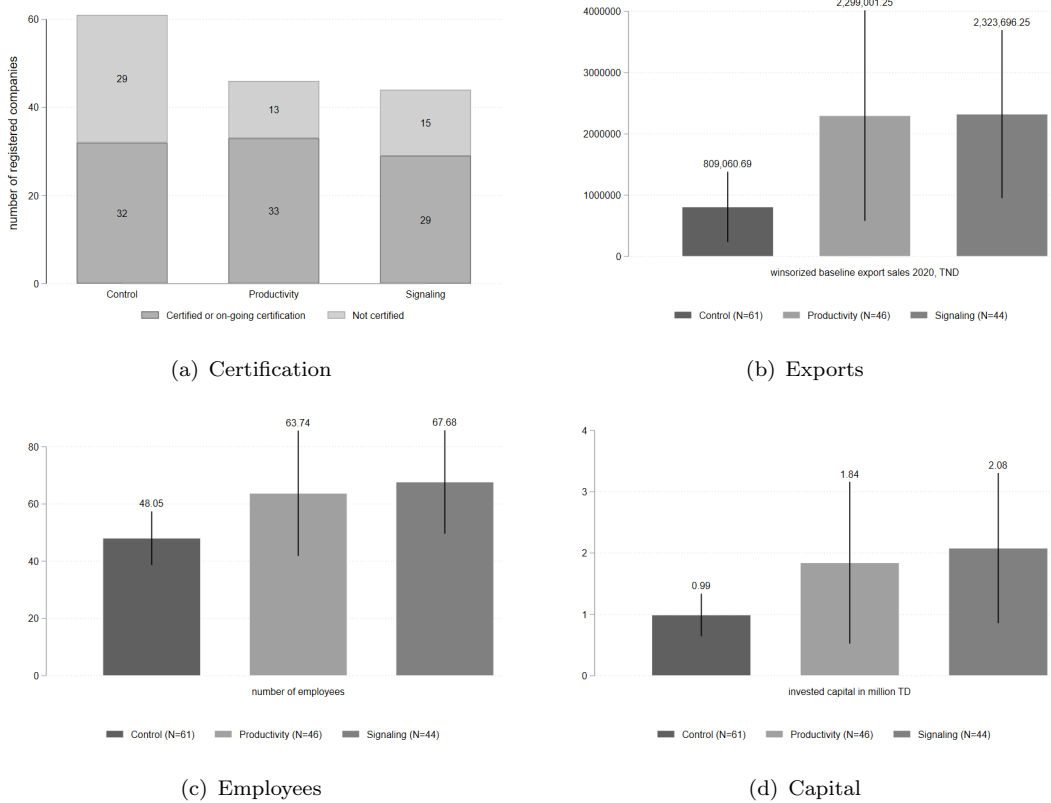
(a) Certification

(b) Exports

(c) Employees

(d) Capital

Figure 3.: Main difference between the firms attracted via the neutral/control, productivity, and signaling email subject line.

3.2. *Experiment 2: Emphasizing benefits attracts better while reducing costs attracts less performing female-led firms*

Figure 4 examines the following three questions. Firstly, does making the influencer video or free childcare provision more salient attract more firms to the program? The first three coefficient and confidence interval estimates, "Control", "Free childcare", and "Influencer video", show that we do not observe a statistically significantly higher registration rate among firms in either group. Around 8 percent of the firms randomly exposed to the neutral (control) group email registered, and around the same registration rate is observed in the free childcare and influence video group.

Secondly, does the program registration rate differ between male-led and female-led firms? Indeed, around 12 percent of female-led firms (with a confidence interval ranging from 9.3 to 14.5 percent) registered for the program, compared to 7 percent of male-led firms (with a confidence interval ranging from 6.2 to 7.9 percent). This 5 percentage point increase is significant at the 10 percent level and corresponds to a 36.5% increase in the share of female-led firms relative to the sampling population.

Thirdly, does the response to the free childcare provision and the influencer video depend on the firm representatives' gender? The response is not straightforward for the following reasons. The pattern point estimates suggest that female-led firms registered at a higher rate than male-led firms; however, they did so even in the control group and, as the estimates are imprecise for female-led firms, they display large, partly overlapping confidence intervals. For example, in the influencer video treatment, the

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confidence interval for male-represented firms' registration likelihood ranges from 5.6-8.6 percent, compared to 8.7- 18.1 percent for female-represented firms. The confidence intervals overlap for the signaling and the control group.

Two factors are central to explaining the murky pattern in the results: low power and control group exposure to treatment via other communication channels. There are few female-represented businesses. Each treatment group has roughly 200 female-represented vs 1,000 male-led firms. Given that the (positive) treatment effect seems to have been concentrated among female-represented businesses, the power to detect the effect decreased. Moreover, the power to identify a statistically significant interaction is generally lower due to higher standard errors for interaction terms (Muralidharan, Romero, and Wüthrich 2019). Accordingly, we would interpret the insignificant but differential response among female and male-led firms in response to the treatment at least as an encouragement to test the treatment again, potentially with only one arm and/or ideally with larger number of female-led firms, e.g., across several programs and in countries where the share of female-directed businesses is higher. As described in section 2.3, the influencer video and the free childcare offer were visible to any firm interested in registering for the program on the implementation partners' social media and websites. The political partner had invested considerable resources in developing it, and it was considered unethical to withhold information about childcare services from potential applicants. Treatment was, therefore, "only" an additional nudge that increased the salience of both treatments to the specific firms in the respective group.
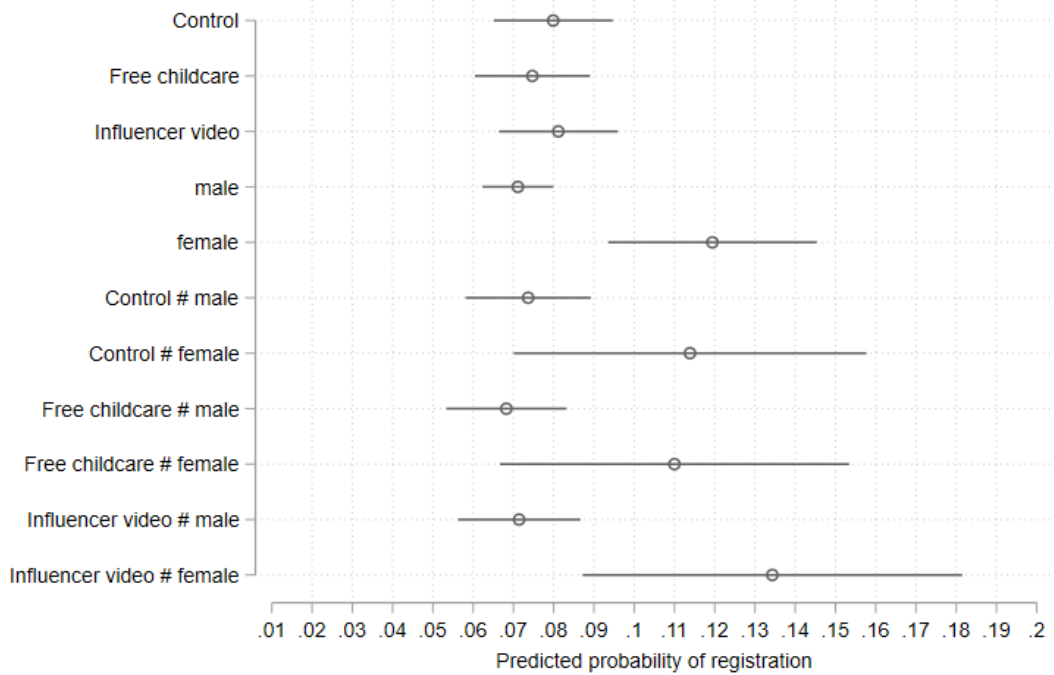

Figure 4.: Predicted registration probabilities by treatment group and treatment-gender interaction. Values correspond to estimates presented in Table D7.

Beyond the average effect, we find evidence that each treatment attracted different types of women-led firms (Table 1). Firms attracted via the free childcare incentive have consistently and statistically significantly lower average sales, profits, exports,

and employees than the firms attracted via the control and influencer video incentive. However, the difference in employees and age is not statistically significant. The influencer video attracts more established women-led firms with the highest average sales and online revenues. This may reflect that it was more natural for equally established and successful businessmen and women to self-identify with the influencer, already an established and wealthy businesswoman. Finally, both the free childcare and the influencer video incentive attracted firms with a statistically significantly higher share of female employees, around 6 percentage points in the influencer video group and even 9 percentage points higher in the free childcare group, and a six and five percentage points higher share of female CEOs.

Table 1.: How do the firms attracted via neutral, free childcare and influencer video incentive differ?

Variable	(1) Control Mean/SD	(2) Free childcare Mean/SD	(3) Influencer video Mean/SD	(1)-(2)	T-test P-value (1)-(3)	(2)-(3)
employees	34.77 (43.97)	29.93 (43.62)	38.03 (48.93)	0.42	0.61	0.21
share of female employees	0.46 (0.23)	0.55 (0.27)	0.52 (0.26)	0.01**	0.06*	0.47
CEO gender (1 = female)	0.21 (0.36)	0.27 (0.40)	0.26 (0.40)	0.25	0.39	0.79
HQ in Tunisia	0.91 (0.29)	0.97 (0.17)	0.97 (0.17)	0.07*	0.06*	0.95
winsorized baseline domestic sales 2020	7,532,095.96 (7,546,871.34)	5,767,200.06 (3,156,510.34)	8,715,175.38 (18,202,402.73)	0.02**	0.54	0.10
winsorized baseline profit 2020	259,154.17 (301,048.35)	202,652.79 (189,317.57)	221,708.32 (352,299.07)	0.10*	0.40	0.63
winsorized baseline export sales 2020	2,779,940.33 (3,325,637.49)	2,061,040.81 (1,701,409.46)	2,381,292.13 (3,240,987.14)	0.05**	0.37	0.37
winsorized baseline digital revenue 2020	30,676.15 (19,310.97)	28,588.62 (18,697.62)	64,331.96 (198,029.69)	0.43	0.08*	0.07*
N	112	100	105			

Notes: The value displayed for t-tests are p-values. Standard deviations are robust. All missing values in balance variables are treated as zero.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

To further explain these results, we asked female entrepreneurs in a separate program what support measure would help them participate in the program. The results suggest that child care is only a concern for a smaller number of female CEOs or women firm representatives than we expected (Figure C7). 41 out of 176 or 23 percent of the female CEOs indicated that free child care during the program activities would help them participate in the program. Instead, virtual meetings and transport or accommodation support were selected three times as much (75 and 71 percent of the female CEOs indicated a preference for these options). The female CEOs who did select child care had, on average, 1.5 children below 18, which is 0.5 more than women who did not select this option. Their companies had, on average, half the sales, were 2 years younger, and were 10 percentage points less likely to be exporters than the female firms that did not select this option. Accordingly, the combination of the experimental regression results and these descriptive statistics suggest that childcare is, at least for the average type of female SME entrepreneur interested in a government support program in Tunisia, not a major concern. However, it may be particularly helpful for younger female microentrepreneurs who have more children and manage

less established firms.

3.3. *How do different communication channels affect sample size and composition?*

In this section, we descriptively examine the importance of each recruitment channel and its characteristics to firm enrollment. Although not causal, the results offer interesting insights, including for practitioners, into firms' behaviour and level of engagement with program content before registration.

We document several common trends across both programs (see Figure C9 in the appendix). Firstly, emailing was a main driver of registrations. In experiment 1, about half of the firms registered in response to an email. Even in experiment 2, about a quarter or 27.2 per cent, of the registrations originated from the email campaign (the relative share is lower as the political partner in experiment 2 had a stronger established social media presence). Marketing on social media can equally attract a substantial number of firms, especially if implementation partners dispose of a strong pre-existing social media presence. The dominant social media platform in Tunisia is Facebook, which contributed the highest registration share in experiment 2, where political partners had an established and active Facebook presence and paid for sponsored campaigns. In contrast, without an established presence, the social media reach was substantially smaller in experiment 1. Finally, the websites and newsletters of implementation partners contribute a smaller but non-negligible share of interested firms. The closer to firms an implementation partner operates, the higher the number of firms attracted. For example, Ministerial web pages generated little attention. In contrast, the web pages and newsletters of the export promotion agency and, to a lesser extent, specialized technical centres and business associations contributed more to the number of applications.

Firms spend little time engaging with the program information but more if recruited via professional channels. Firms attracted via email spent an average of 2:04 minutes and looked at 8 tabs on experiment 1's website. In comparison, firms attracted via Facebook spent 00:24 minutes and looked, on average, at 4 tabs. Individuals looked at social media mostly on their smartphones, while e-mails were usually opened on computers, thereby increasing the time spent on the website of the project. Interestingly, we measure a substantial difference in the interest of potential applicants attracted via Facebook and LinkedIn. Although LinkedIn attracted fewer applicants, webpage visitors attracted via LinkedIn spent 6:29 minutes and looked at 8 pages on average. Social media (Facebook) is accessed in different settings, mostly via mobile phones, potentially in people's free time or transport, while LinkedIn is mostly accessed via computers. Given its professional focus, LinkedIn users may also be a better fit for the program than the average Facebook user.

Different recruitment channels attract firms with different characteristics, particularly in export statuses and sizes, measured by employees and corporate capital (see Table D1). Radio and especially Facebook seem to be the most diversifying means of communication regarding the size of attracted companies. Half of the firms attracted via Facebook do not export vs. 23 percent on average, they have 13 employees vs 23 on average, and 416,000 vs. 1.6 million in corporate capital on average. Facebook also fares best regarding the share of female CEOs. Radio attracts firms with similar characteristics except female-led firms, potentially due to its strength in rural areas (see next paragraph). In contrast, the firms attracted via the export promotion office's web-

site are the most likely to be already exporters and have the highest average amount of capital as well as the highest average number of employers, which illustrates that a program that would have exclusively relied on the implementation partners means of communication would have targeted primarily incumbents. Firms that are already performing much better than the average and have also very likely benefited from similar programs in the past. Finally, the importance of recruitment channels varies by region (Figure C8 in the appendix). In large urban centers, like the Tunis metropolitan area (Tunis, Ben Arous, Ariana), Sousse, Sfax, or Monastir, a large variety of communication channels were deemed as decisive for registration by the applying firms. In more rural areas, with a potentially lower density of SMEs, Facebook, LinkedIn, and e-mail were almost exclusively important. The ministry's (yellow) or export promotion agency's (red) websites were relevant in larger urban centers like Tunis or Sfax but not very much in more rural districts. Comparatively few respondents mentioned radio as a decisive communication channel.

Overall, we find evidence that the communication channel used is associated with different numbers of applications and types of firms. Existing communication channels of implementation partners (i.e. their website) were more likely to attract firms that are likely 'incumbents'. In contrast, more open channels, particularly social media, diversified the type and number of interested firms, attracting more under-represented firms, e.g., female-led firms and firms in rural areas.

4. Conclusion

In this paper, we argue that knowledge frictions and communication are crucial, yet understudied elements in existing impact evaluations of government support programs with firms. The extent and framing of recruitment efforts may attract firms with different characteristics and thus lead to different potential outcomes for program participation. Reviewing existing firm-level impact evaluations shows that most studies do not document their communication strategies, making it hard to learn from previous work and omitting important information on the programs's effect and generalizability. To learn more about the effectiveness of different communication channels, we integrate two randomized controlled trials in the communication campaigns of two government support programs in Tunisia and show that small changes in the framing of email subject lines and additional nudging information affect sample size (number of registrations) and sample composition (type of firms that register).

We find direct emailing using neutral email subjects and social media campaigns, independent from political partners, attracts more and less established companies outside of urban centers with lower levels of main outcomes of interest (e.g., export, employees, sales). We recommend researchers and policy-makers follow such an approach if they intend to diversify the pool of participants or if firms may hold negative priors about the program's goals. However, there is a trade-off involved. Communication via political partners and subject-specific messages attracts more established firms (in terms of sales, employees, financial capital, and age) and better performing in the program's goal (e.g. export, innovation). The best intentions, such as trying to improve marketing messages by underlining specific benefits, may have the unintended consequence of deterring worse-performing firms that are less acquainted with the specific topic but which may have more potential to benefit from the program. Yet, in case project implementers consider firms need to have passed a certain performance threshold, e.g. already export or hold a quality certificate, more specific, tailored marketing

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messages and communication through traditional partner networks seem to improve targeting of companies that have met these thresholds.

Finally, the gender-sensitive communication and email campaign paid off overall, attracting 6 percentage points more women-led firms. Due to the small number of female entrepreneurs and contamination to the control group, we can not distinguish whether free childcare or the influencer video drives the result. An analysis of the firm characteristics suggests both incentives attracted different firms. Firms attracted via the free childcare nudge have lower average sales, profits, exports, and employees than the control and the influencer video group. The firms attracted via the influencer video nudge have the highest sales and digital revenues, suggesting the influencer video resonated with female entrepreneurs in charge of established and digitized firms. Free childcare mattered more for smaller firms represented by younger female entrepreneurs with more children.

Overall, we consider this study a first step to systematically document and provide evidence about the effects of communication on sample size and composition. This study has several limitations, which should be addressed in future research. The first concern relates to external validity. Given both experiments and communication campaigns were implemented with the same firm population in the same country and for programs with the same ultimate goal, different means of communication may have a different response rate in other contexts. Future studies would ideally explore the randomization of different communication channels on the sectoral-, market- or regional level to test for the differential firm enrollment, firm characteristics, and cost-effectiveness of various means of communication. In addition, future studies may include a control group simply excluded from emailing to identify the effect of direct emailing vs. business as usual.

We hope this work will inform and spark future research and analysis on how recruitment strategies affect firms' program registration decisions and that researchers and policymakers use it to target their recruitment campaigns.

Author contributions

Florian Anselm Münch: General conceptualization, Conceptualization email experiment 1 & 2, Data curation email experiment 1, Investigation, Formal analysis, Writing - Original Draft, Writing - Review & Editing, Visualization, Supervision, Project administration. **Amira Bouziri:** Conceptualization email experiment 1, Conceptualization email experiment 2, Investigation, Project administration. **Fabian Scheifele:** Conceptualization email experiment 2, Investigation, Writing - Review & Editing, Project administration. **Kais Jomaa:** Conceptualization email experiment 2, Investigation, Data curation email experiment 2, Investigation, Writing - Original Draft, Writing - Review & Editing, Project administration. **Teo Firpo:** General conceptualization, Conceptualization email experiment 2, Investigation, Writing - Review & Editing.

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Appendix A. A simple model: Attracting firms to government programs

In the following, we develop a model of firm i's decision to enroll D_i in a government support program. We built the model based on features from the Rubin causal model (Rubin 1974), related latent linear index models of individuals' decision to enroll in a program (Heckman and Robb 1985), and the instrumental variable framework, also known as local average treatment or complier average causal effect model (Imbens and Angrist 1994; Angrist, Imbens, and Rubin 1996).

As a starting point, we adopt the basic setting in Heckman and Robb (1985). Firms currently generate revenue Y_i and program enrolment D_i has an effect γ on revenue given firm characteristics X_i and an error term U_i [8]:

$$Y_i = \beta X_i + \gamma D_i + U_i \tag{A1}$$

We assume that firm i enrolls in the program if its perceived benefit B_i is higher with than without enrolment.[9]

$$D_i = \begin{cases} D_i = 1 & if \ B_i | D_i = 1 > B_i | D_i = 0 \\ D_i = 0 & if \ B_i | D_i = 1 < B_i | D_i = 0 \end{cases} \tag{A2}$$

In applied work in the existing literature, there is an implicit assumption that the decision to enroll $D_{i,t}$ is random ($D_i \perp\!\!\!\perp U_i$) and that γ, the effect of enrolment D_i, is homogeneous among companies. Our argument, however, is that the relationship between D_i and U_i, and the magnitude of γ depend on the extent of communication C^e (extensive margin) and the content or framing of communication C^f (intensive margin). C^e and C^f are not random but are determined by the implementing agency and/or researchers.

Accordingly, we formulate the following definition of firm i's enrolment decision. We introduce K_i, whether a firm has knowledge about the program, and B_i, the perceived benefit from program participation [10]:

$$D_i = K_i * \mathbf{1}B_i \tag{A3}$$

where $\mathbf{1}B_i$ is an indicator function that takes the value of zero if $B_i <= 0$ and one if $B_i > 0$. A first logical implication of rewriting firms' enrolment decision in this way is that if firms have no knowledge about the program ($K_i = 0$), firms do not enroll ($D_i = 0$). We define K_i as a function of C^e, the extent of communication, and firm characteristics X_i.

$$K_i = \mathbf{1}X_i(C^e) \tag{A4}$$

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[8] To simplify, we assume every firm that enrolls in a program gets to participate.

[9] In Heckman and Robb (1985), the authors take the time dimension t into account and consider net present value rather than the potential outcome. We neglect the time dimension for simplicity and as timing does not matter for our argument.

[10] In Heckman and Robb (1985) the authors assume D_i depends on Z_i and V_i, firms' unobservable and observable characteristics. We subsume Z_i and V_i into one firm characteristics term X_i given the observability is not important for our argument.

where C^e takes a value between zero and one. If $C^e = 0$, communication remains private, meaning it is limited to existing clients, members of associations or cronies ($C^e = 0$). Only these connected firms have knowledge ($K_i = 1$) and enroll ($D_i = 1$) if their perceived benefits is larger than zero ($B_i > 0$).

$\mathbf{1}X_i(C^e)$ is a step-wise indicator function that is either one or zero depending on the value C^e takes. As C^e increases, meaning more communication channels are added, a new cohort of firms with certain characteristics acquire knowledge of the program. We illustrate this relationship in figure C3 in the appendix for the relationship between C^e and the share of firms with a given characteristic. As more public communication channels are added, C^e converges to 1 and firms with lower and more diverse values of X_i enroll. Modeling this relationship as a step-wise function implies that as C^e takes on higher values, e.g. smaller or more remotely located firms gain knowledge of the program.

However, not all the additional firms within a size or geographical category decide to enroll in a program as the enrolment decision (D_i) is not only a function of firms' knowledge (K_i) but also their perceived benefit (B_i) from program participation. We model B_i as a function of firms' potential outcome under enrolment ($Y_i^1|D_i = 1$) weighted by α_i, the firms' prior regarding the program, and C^f, the framing of the program's communication and its goal (e.g. innovation, exporting, modern management practices), i.e. the *intensive margin*.[11]

$$B_i = (\alpha_i + s_i C^f)\, Y_i^1, \qquad for \quad Y_i^1 > 0 \tag{A5}$$

Conditional on having knowledge of the program ($K_i = 1$), firms enroll if $B_i > 0$ or, in other words, the product of the sum of priors and framing and potential outcome is larger than zero. Firms' priors α_i and firms interpretation of or signal s_i from the content framing of communication C^f can be:

$$\alpha_i = \begin{cases} \alpha_i = 0 & indifferent \\ -1 >= \alpha_i < 0 & negative\ prior \\ 1 <= \alpha_i > 0 & positive\ prior \end{cases}, \quad s_i = \begin{cases} s_i = 0 & indifferent \\ -1 >= s_i < 0 & negative\ signal \\ 1 <= s_i > 0 & positive\ signal \end{cases}$$

Firms' priors combine with the framing of the communication campaign C^f in their evaluation of their benefit B_i from program enrolment D_i. While firms' priors are exogenous, the framing C^f of the communication campaign is the crucial input under the influence of project implementers, e.g., researchers or policy-makers. Framing refers to, for example, the choice of marketing narratives and incentives to convince firms to enroll (for example, see section 2.2). Importantly, the framing C^f interacts with firms' priors α_i in the following ways (for a mathematical overview see table D6).

First, we focus on firms with positive priors about the (goal of the) program ($\alpha_i > 0$). We call these firms "switchers" in line with the terminology in Angrist, Imbens, and Rubin (1996). Once these firms have knowledge of the program, we expect them to join ($B_i > 0, D_i = 1$) thanks to their positive priors, even if there is no specific framing ($C^f = 0$). In this case, positively perceived framing simply reinforces a pre-

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[11]We focus here for simplicity on the case that $Y_i^1 > 0$. In case $Y_i^1 < 0$, $(\alpha_i + C^f)$ becomes $-(\alpha_i + C^f)$ to ensure a negative sum of prior and framing amplifies a negative potential outcome and results in a negative benefit.

existing positive prior about a program (e.g. its goal to increase exports). However, the communication framing C^f, even if positively-intended, may be negatively perceived and may counteract and even dominate a positive prior, resulting in the decision not to enrol. This could be the case, for example, if positively-intended communication reveals new information, e.g., exporting is more complicated or relatively less lucrative than the firm thought.

Second, we think about firms that have a negative prior about the (goal of the program) as "defiers" (Angrist, Imbens, and Rubin 1996). These firms defy the offer to enroll if there is a neutral, open communication due to their negative priors of the program or its goals ($\alpha_i < 0$). If the communication framing C^f is positively perceived and outweighs the negative prior ($s_i > alpha_i$), defiers may still decide to enroll. C^f can also reinforce negative beliefs in which case defiers do not enroll.

Importantly, firms' priors α_i and the signal they take from the framing of the program s_i can work in the opposite direction of firms' potential outcomes Y^1 under program participation. For example, table D6 in the appendix illustrates how firms with a positive or negative prior α_i that might be out-weighted by a positive signal s_i from the framing of communication campaign, would decide to enroll *even if their potential outcome is negative.* From an individual firm-level perspective this decision is sub-optimal as the firm would have been better off without program participation. Given firms do not know Y^1 beforehand, they can take sub-optimal decisions, which we accommodate in this theoretical framework. As a consequence, the effect of framing on the average treatment effect of a program depends on the potential outcome of the firms that decide to enroll due to the framing and would have not done otherwise.

Appendix B. Technicalities of linking treatment status and treatment response

B.0.1. Matching of registrations with initial list

Figure B1 illustrates how we identified which of the companies that we contacted via email registered to the respective program.

In the case of the experiment 1, we created two different registration platforms that were identical in principle but differed in so far as that one of the platforms was only accessible through a personalised, unique code that the applicant had to provide. We could merge firms registered via the closed platform via the unique code. For firms that registered via the open platform, we used firm name and firms' email address for merging. We checked for dual registrations via open and closed platform, and removed a handful of duplicates from the open platform. Moreover, we used a commercial software to track the email opening and link click rates, which we equally merged back on to the sample list via the unique identifiers. Unfortunately, we did not include a question into the registration sheet asking applicants how they have learned about the program, which means that we cannot identify via which communication channel the firms that signed up on the open platform have been attracted. However, we used a commercial software to monitor the program's website and the registration platform, which enabled us to shed some light on the access channels and applicants behaviour depending on the mean of communication (see section 3).

In experiment 2, we had to proceed differently. Given technical constraints on the side of implementation partners, we could not create individual codes that would have simplified identification. Instead, we had to match companies based on firm name, CEO and firm representative email addresses, which we provided both in the initial population/emailing list and the registered sample. For this purpose, we conducted several rounds of matching, including firstly perfect matches on either of the three variables, as well as fuzzy merging where we excluded all firms with a similarity score of equal to or below 90, and manually decided whether the remaining 1875 candidates were actual matches or not. In addition, we controlled for and dropped duplicate matches that matched on several of the three variables. Finally, we include a multiple-choice question in the registration form ("How did you find out about this program?"), which provides us with an idea about the respective relevance of each mean of communication, including information how the firms that were not contacted via email found out about the program.

To identify the firms that received an email among the registered firms, we will match the registration information with our original email data using string matching on the e-mail addresses and the firms' names. In cases where string matching does not work, the data entries will be matched manually.
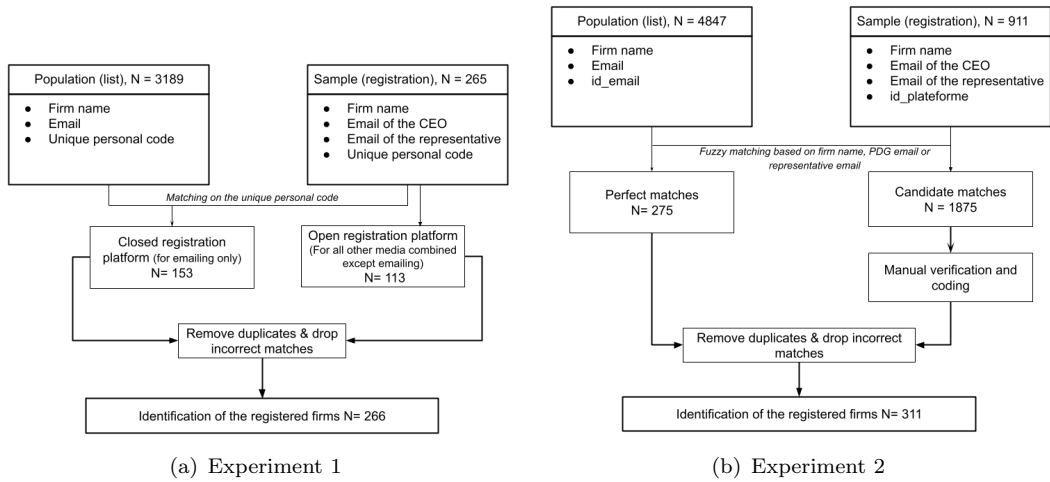
(a) Experiment 1   (b) Experiment 2

Figure B1.: Process of identification of the registered firms

B.0.2. Bounce rates and balance tables

The databases provided by the partner institutions may not always contain the most recent contact information. Accordingly, there are a substantial amount of contacts for which we could not deliver any email in both cases. This is referred to as bounce rate and relates to nonexistent or erroneous email addresses or email addresses with full mailboxes. Given we randomized before sending out the first email, the bounce rate did not only reduce our statistical power but, if it were differential, would have jeopardized the initial randomisation. In the experiment 1, we could never reach 520 contacts, and in the experiment 2 the same number was even 953. Hence, it constituted a considerable loss in power. Fortunately, while there is an imbalance on one variable in the export for experiment 1 and one additional variable in the experiment 2, the bounce rate did not create major imbalances in the sample as documented in table D8 and table D9 in the appendix. Controlling for these small differences in the main regression specification does not change the main results. As a result, we can conclude any observed differences in response rate to the three different email treatments do not stem from structural differences in the three groups of firms but the different email treatments. For future studies, we recommend researchers sent out a first, general email to whole sample, e.g. simply informing firms that there will a new government support program soon but that applications have not yet opened, and only randomizing after all email addresses without bounce.
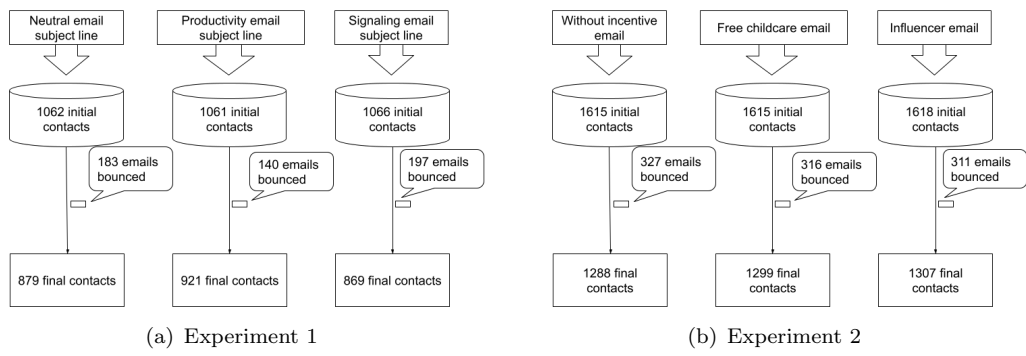
26

(a) Experiment 1   (b) Experiment 2

Figure B2.: Number of emails bounced

Appendix C. Figures

(a) Experiment 1
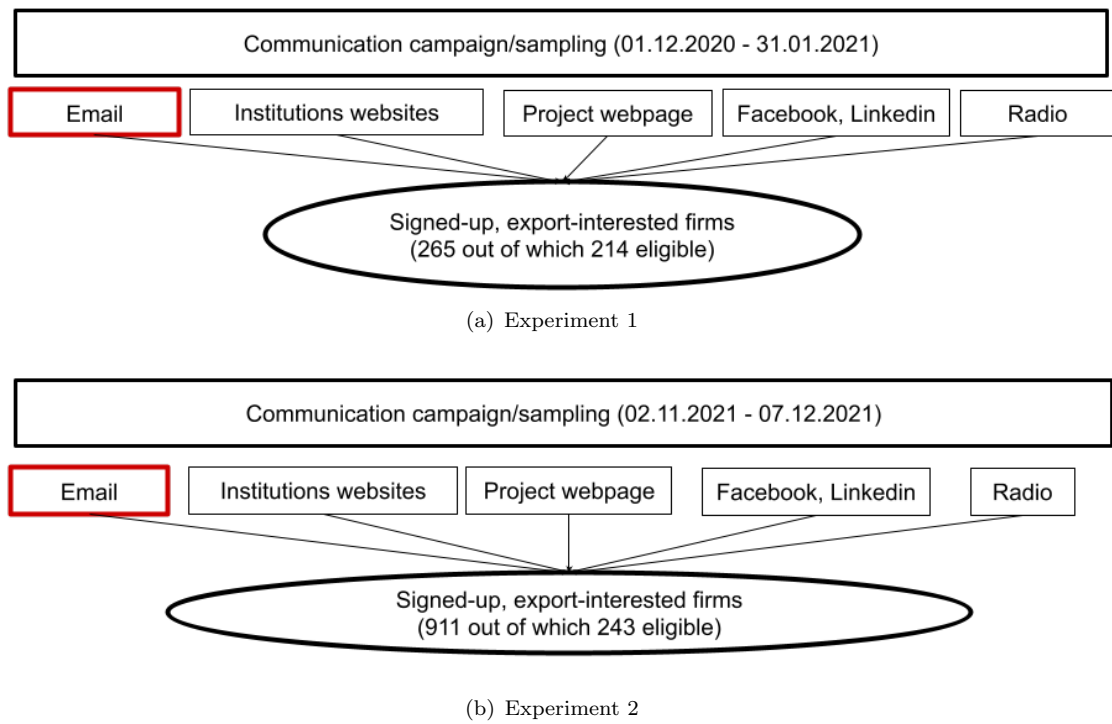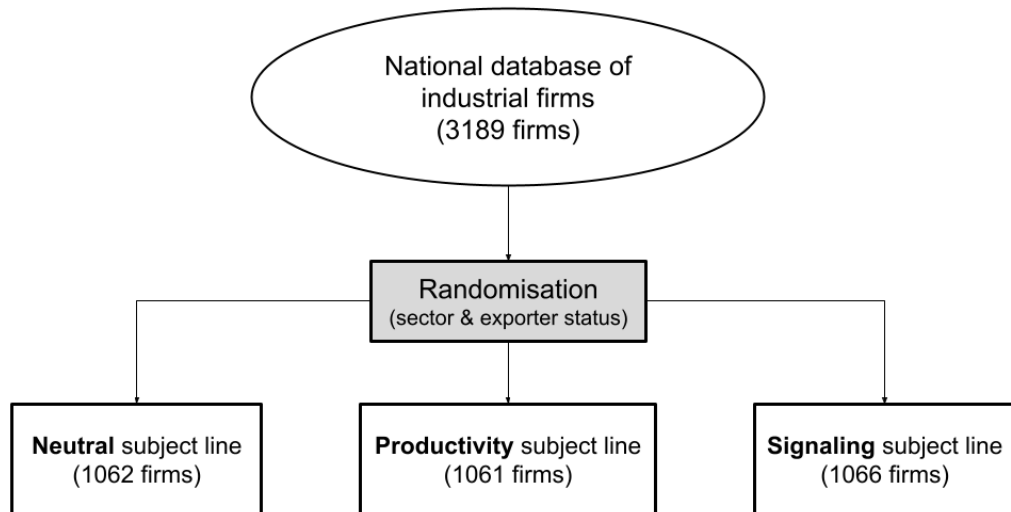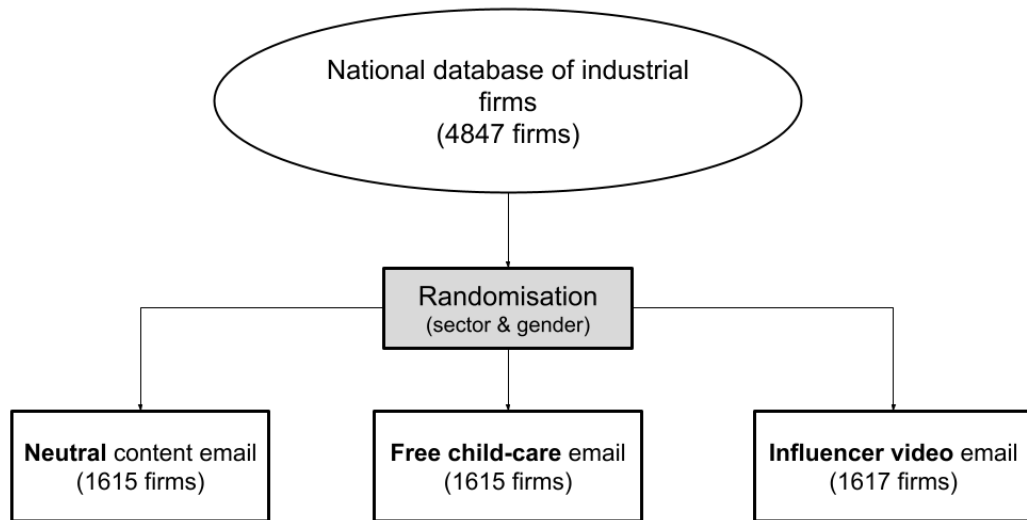

(b) Experiment 2

Figure C1.: Context of the experiments

(a) Experiment 1

(b) Experiment 2

Figure C2.: Visualisation of both experiments

Figure C3.: Functional form: Ce and Xi
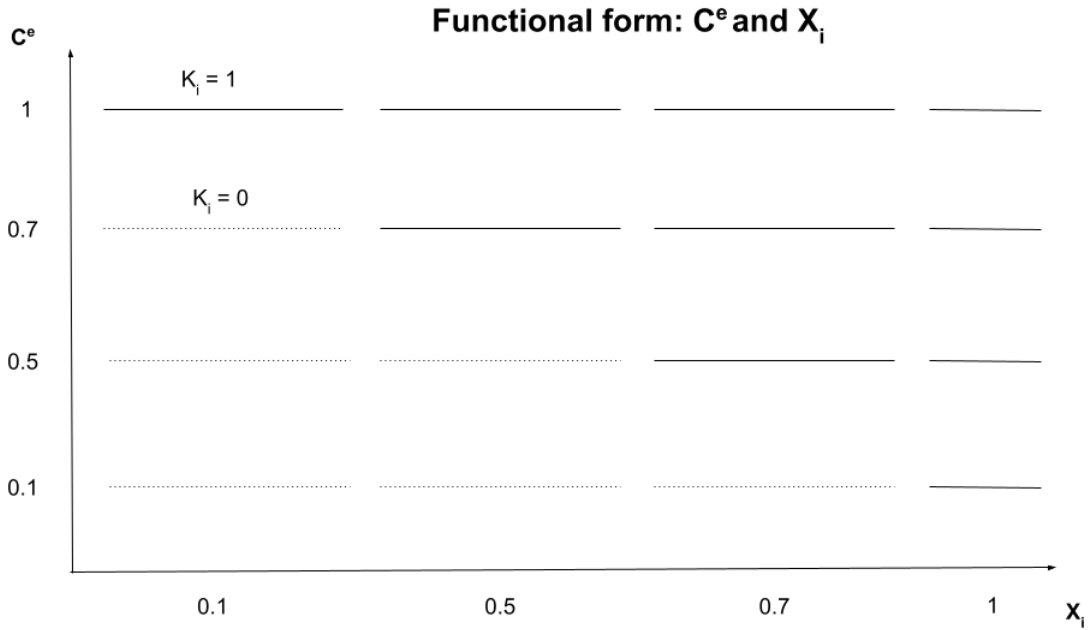

(a) English version of the email

(b) French version of the email

Figure C4.: Email for the subject lines of the email experiment

(a) Neutral email

● Subject: Call for application for e-commerce training for export

Dear business owner,

We are contacting you because we believe your company is eligible for the PEMA II project's e-commerce for export training.

The training offers two activities:
- A classroom training provided by experts in e-commerce and international trade
- Young professionals trained by the same experts to help you implement your online sales strategy

Click on this link to register until November 22, 2021.

Please consult the CEPEX website for more information regarding eligibility criteria, company selection, dates of activities, etc.

The PEMA II project is mandated by the German Cooperation, implemented by GIZ, and in partnership with the Ministry of Trade and Export Development and CEPEX.

Yours sincerely

PEMA Team

Facebook: GIZ Tunisia
LinkedIn: GIZ Tunisia
Website: PEMAII

(b) Free child-care email

● Subject: Call for application for e-commerce training for export

Dear business owner,

We are contacting you because we believe your company is eligible for the PEMA II project's e-commerce for export training.

The training offers two activities:
- A classroom training provided by experts in e-commerce and international trade
- Young professionals trained by the same experts to help you implement your online sales strategy

To facilitate the participation of women entrepreneurs who do not have childcare, we offer free and reliable childcare during the training days.

Click on this link to register until November 22, 2021.

Please consult the CEPEX website for more information regarding eligibility criteria, company selection, dates of activities, etc.

The PEMA II project is mandated by the German Cooperation, implemented by GIZ, and in partnership with the Ministry of Trade and Export Development and CEPEX.

Yours sincerely

PEMA Team

Facebook: GIZ Tunisia
LinkedIn: GIZ Tunisia
Website: PEMAII

(c) Influencer video email

● Subject: Call for application for e-commerce training for export

Dear business owner,

We are contacting you because we believe your company is eligible for the PEMA II project's e-commerce for export training.

The training offers two activities:
- A classroom training provided by experts in e-commerce and international trade
- Young professionals trained by the same experts to help you implement your online sales strategy

Watch the video of Mrs. Sonia FEKI to have an idea of how to succeed in your e-commerce business for export

Click on this link to register until November 22, 2021.

Please consult the CEPEX website for more information regarding eligibility criteria, company selection, dates of activities, etc.

The PEMA II project is mandated by the German Cooperation, implemented by GIZ, and in partnership with the Ministry of Trade and Export Development and CEPEX.

Yours sincerely

PEMA Team

Facebook: GIZ Tunisia
LinkedIn: GIZ Tunisia
Website: PEMAII

Figure C5.: Different treatments and neutral emails
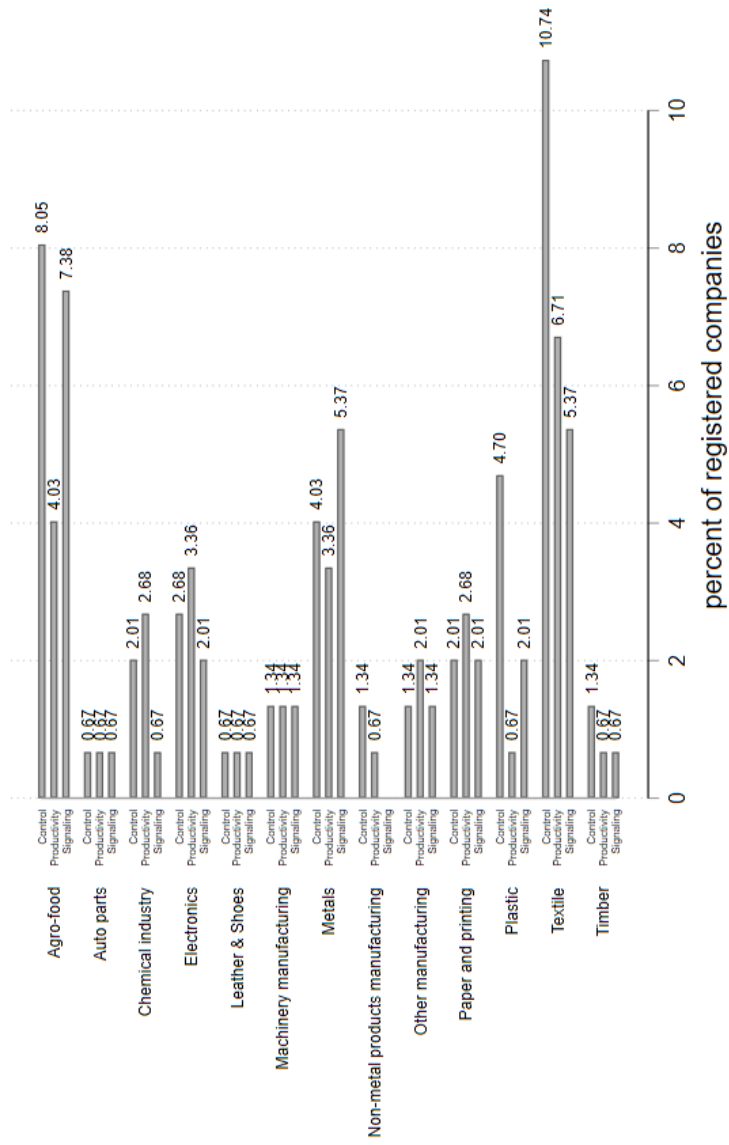
Figure C6.: Registration rate in response to each subject line treatment
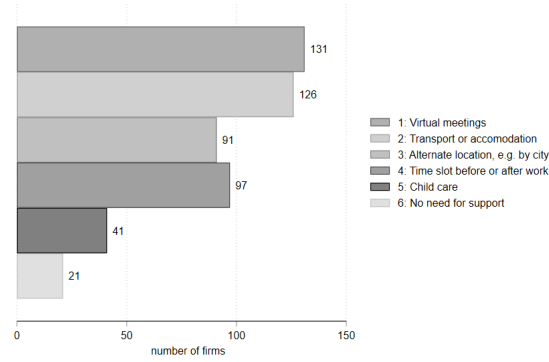
Figure C7.: Preferences for participation support incentives among female CEOs participating in a female export consortia government support program in Tunisia

(a) Urban

(b) Rural
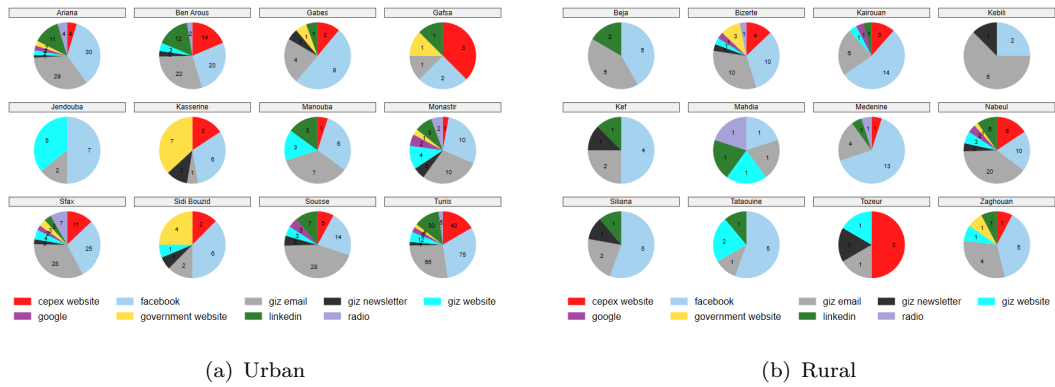
Figure C8.: Regional outreach of each mean of communication for the experiment 2

(a) Experiment 1

(b) Experiment 2
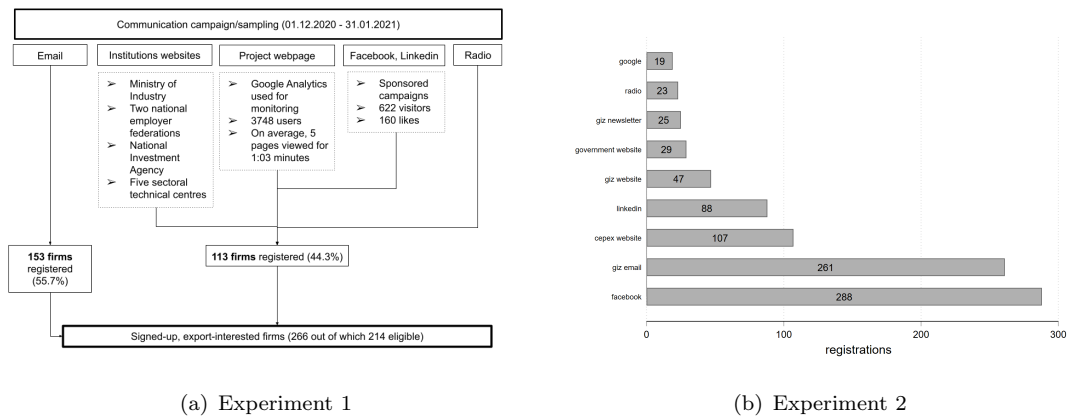
Figure C9.: Online monitoring data (panel a) and self-reported registration by communication channel (panel b)

Appendix D. Tables

Table D1.: Average firm characteristics from registration to e-commerce program for all firms with = 200 employees by mean of communication

Variable	(1) cepex website Mean/SD	(2) giz email Mean/SD	(3) facebook Mean/SD	(4) giz newsletter Mean/SD	(5) giz website Mean/SD	(6) google Mean/SD	(7) government website Mean/SD	(8) linkedin Mean/SD	(9) radio Mean/SD
employees	23.79	24.75	12.84	18.00	25.62	17.00	15.93	21.03	20.82
	(37.19)	(37.29)	(25.03)	(25.21)	(39.35)	(22.27)	(21.17)	(38.32)	(44.34)
share of female employees	0.49	0.50	0.54	0.52	0.50	0.59	0.43	0.49	0.47
	(0.28)	(0.26)	(0.37)	(0.17)	(0.22)	(0.27)	(0.30)	(0.27)	(0.27)
age	10.81	11.58	6.76	10.64	12.77	9.25	8.93	8.60	7.55
	(10.49)	(13.12)	(10.06)	(11.56)	(15.79)	(17.96)	(9.03)	(9.98)	(8.64)
capital	2,948,823.19	1,443,914.65	416,341.20	1,260,311.44	1,136,149.28	1,008,531.25	779,540.79	1,640,301.43	222,409.09
	(11895736.24)	(8,117,889.56)	(2,294,236.54)	(4,375,527.27)	(3,435,214.14)	(3,472,408.72)	(2,148,480.74)	(7,657,947.05)	(644,448.80)
no export	0.19	0.28	0.48	0.24	0.33	0.44	0.29	0.32	0.55
	(0.40)	(0.45)	(0.50)	(0.44)	(0.48)	(0.51)	(0.46)	(0.47)	(0.51)
partial exporter	0.39	0.48	0.41	0.52	0.51	0.38	0.57	0.44	0.32
	(0.49)	(0.50)	(0.49)	(0.51)	(0.51)	(0.50)	(0.50)	(0.50)	(0.48)
exclusive exporter	0.42	0.24	0.11	0.24	0.15	0.19	0.14	0.23	0.14
	(0.50)	(0.43)	(0.31)	(0.44)	(0.37)	(0.40)	(0.36)	(0.43)	(0.35)
intention to export	0.96	0.95	0.94	0.88	0.95	0.94	0.96	0.97	0.86
	(0.21)	(0.23)	(0.24)	(0.33)	(0.22)	(0.25)	(0.19)	(0.16)	(0.35)
CEO gender (1 = female)	0.34	0.30	0.36	0.31	0.34	0.29	0.31	0.31	0.23
	(0.43)	(0.40)	(0.42)	(0.45)	(0.44)	(0.44)	(0.45)	(0.40)	(0.43)
Representative gender (1 = female)	0.52	0.39	0.48	0.36	0.56	0.50	0.54	0.40	0.41
	(0.50)	(0.49)	(0.50)	(0.49)	(0.50)	(0.52)	(0.51)	(0.49)	(0.50)
webpage or social media account (1 = yes)	0.83	0.78	0.74	0.88	0.85	0.81	0.79	0.74	0.82
	(0.38)	(0.41)	(0.44)	(0.33)	(0.37)	(0.40)	(0.42)	(0.44)	(0.39)
N	89	237	271	25	39	16	28	77	22

*Notes*: The value displayed for t-tests are the differences in the means across the groups.
Standard deviations are robust.
All missing values in balance variables are treated as zero.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table D2.: Balance table quality upgrading for export

Variable	(1) Control Mean/SE	(2) Productivity Mean/SE	(3) Signaling Mean/SE	(4) Total Mean/SE	T-test P-value (1)-(2)	(1)-(3)	(2)-(3)
firm age	18.96	19.37	19.68	19.34	0.47	0.21	0.61
	(0.39)	(0.43)	(0.42)	(0.24)			
number of employees	59.68	59.75	61.39	60.28	0.98	0.47	0.48
	(1.68)	(1.63)	(1.66)	(0.96)			
foreign owned	0.30	0.30	0.30	0.30	0.95	0.92	0.87
	(0.01)	(0.01)	(0.01)	(0.01)			
invested capital in million TD	1.47	1.72	1.32	1.50	0.41	0.55	0.16
	(0.19)	(0.24)	(0.15)	(0.11)			
firm representative's gender	0.13	0.12	0.13	0.13	0.33	0.81	0.47
	(0.01)	(0.01)	(0.01)	(0.01)			
tunisian regions	12.99	13.03	13.10	13.04	0.90	0.71	0.80
	(0.21)	(0.21)	(0.21)	(0.12)			
N	1062	1061	1066	3189			
F-test of joint significance (F-stat)					0.34	0.50	0.90
F-test, number of observations					2123	2128	2127

Notes: The value displayed for t-tests are p-values. The value displayed for F-tests are the F-statistics. Standard errors are robust. All missing values in balance variables are treated as zero.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table D3.: Balance table e-commerce for export

Variable	(1) Control Mean/SD	(2) Free childcare Mean/SD	(3) Influencer video Mean/SD	(1)-(2)	T-test P-value (1)-(3)	(2)-(3)
fte	118.96 (319.03)	109.08 (227.79)	120.33 (276.82)	0.31	0.90	0.21
export== not totally exporting	0.52 (0.44)	0.54 (0.44)	0.53 (0.44)	0.26	0.37	0.83
export== totally exporting	0.48 (0.44)	0.46 (0.44)	0.47 (0.44)	0.26	0.37	0.83
size==small	0.37 (0.42)	0.37 (0.42)	0.39 (0.43)	0.96	0.23	0.25
size==medium	0.38 (0.43)	0.38 (0.43)	0.36 (0.42)	0.94	0.16	0.18
size==large	0.15 (0.31)	0.16 (0.32)	0.14 (0.31)	0.45	0.56	0.18
size==big	0.10 (0.26)	0.09 (0.25)	0.11 (0.27)	0.36	0.33	0.06*
origin==api	0.77 (0.42)	0.77 (0.42)	0.77 (0.42)	0.93	0.98	0.95
origin==pema	0.11 (0.32)	0.11 (0.31)	0.11 (0.32)	0.74	0.97	0.71
N	1615	1615	1617			

Notes: The value displayed for t-tests are p-values. Standard deviations are robust.
All missing values in balance variables are treated as zero.***, **, and * indicate
significance at the 1, 5, and 10 percent critical level.

Table D4.: Effect of subject line on email opening, click on registration link, and registration

	(1) beta	(2) pp	(3) beta	(4) pp
Panel A: Opened email				
Productivity	-0.34*** (0.10)	-0.08*** (0.02)	-0.35*** (0.10)	-0.08*** (0.02)
Signaling	-0.13 (0.10)	-0.03 (0.02)	-0.14 (0.10)	-0.03 (0.02)
Constant	-0.28*** (0.07)		-0.58** (0.27)	
Observations	2669	2669	2669	2669
controls			yes	yes
Panel B: Click on registration link in email				
Productivity	-0.61*** (0.16)	-0.05*** (0.01)	-0.61*** (0.17)	-0.05*** (0.01)
Signaling	-0.63*** (0.17)	-0.05*** (0.01)	-0.65*** (0.17)	-0.05*** (0.01)
Constant	-1.96*** (0.10)		-3.84*** (1.01)	
Observations	2669	2669	2663	2663
controls			yes	yes
Panel C: Program registration				
Productivity	-0.35* (0.20)	-0.02* (0.01)	-0.35* (0.20)	-0.02* (0.01)
Signaling	-0.34 (0.20)	-0.02* (0.01)	-0.36* (0.21)	-0.02* (0.01)
Constant	-2.60*** (0.13)		-3.28*** (0.72)	
Observations	2669	2669	2507	2507
Strata controls			yes	yes

All models are estimated in Stata 15 SE using logistic regressions. PP stands for predicted probability. In column (3), algorithm based firms' gender assignment has been corrected manually based on the name of the person who registered the company and (s)he being the CEO. The sample is reduced to 2669 firms as we exclude 520 firms with malfunctioning email addresses.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ denote the significance level.

Table D5.: How do the firms attracted via neutral, productivity and signaling subject differ?

Variable	(1) Control Mean/SE	(2) Productivity Mean/SE	(3) Signaling Mean/SE	(1)-(2)	T-test P-value (1)-(3)	(2)-(3)
% exports in total sales	55.33 (5.21)	53.50 (6.05)	55.39 (6.41)	0.82	0.99	0.83
firm exports	0.93 (0.03)	0.87 (0.05)	0.93 (0.04)	0.28	0.96	0.33
intends to export within 12 months	0.98 (0.02)	1.00 (0.00)	0.98 (0.02)	0.32	0.82	0.32
number of employees	48.05 (4.74)	63.74 (11.08)	67.68 (9.14)	0.20	0.06*	0.78
invested capital in million TD	0.99 (0.18)	1.84 (0.67)	2.08 (0.62)	0.22	0.09*	0.79
foreign owned	0.21 (0.05)	0.17 (0.06)	0.30 (0.07)	0.61	0.35	0.18
certification in progress	0.11 (0.04)	0.24 (0.06)	0.18 (0.06)	0.10	0.35	0.51
no certification	0.44 (0.06)	0.28 (0.07)	0.34 (0.07)	0.09*	0.29	0.56
certified	0.41 (0.06)	0.48 (0.07)	0.48 (0.08)	0.49	0.50	0.99
multinational company	0.08 (0.04)	0.07 (0.04)	0.07 (0.04)	0.74	0.79	0.96
gender CEO	0.92 (0.04)	0.93 (0.04)	0.89 (0.05)	0.74	0.60	0.43
firm age	19.36 (1.48)	21.05 (1.89)	19.92 (1.88)	0.48	0.81	0.67
seconds to complete registration, winsorized 90th pct.	686.74 (92.99)	833.16 (145.73)	1,010.38 (183.54)	0.40	0.12	0.45
days firm took to register	40.92 (2.64)	39.78 (3.52)	45.09 (3.18)	0.80	0.31	0.27
qii index, baseline	0.02 (0.05)	-0.09 (0.06)	-0.00 (0.06)	0.12	0.73	0.31
winsorized baseline export sales 2020	1,236,942.38 (166,044.53)	2,080,743.48 (524,019.25)	2,085,415.27 (406,770.69)	0.13	0.06*	0.99
winsorized baseline domestic sales 2020	3,216,032.92 (696,519.41)	2,831,103.70 (714,298.56)	2,710,261.22 (399,712.10)	0.70	0.53	0.88
winsorized baseline profit 2020	193,355.40 (21,735.68)	168,874.96 (25,594.30)	183,145.25 (23,148.74)	0.47	0.75	0.68
export readiness, baseline	3.54 (0.25)	3.39 (0.30)	3.08 (0.26)	0.71	0.21	0.43
export perception, baseline	16.85 (0.46)	15.91 (0.51)	16.72 (0.54)	0.17	0.86	0.28
N	61	46	44			

Notes: The value displayed for t-tests are p-values.

Standard deviations are robust.

All missing values in balance variables are treated as zero.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table D6.: ¨Prior-signal combinations with potential outcome and firm group

	Y_i	α_i	C^f	Result: B_i	Group
1	+	+	+	+	Switchers, reinforced
2	+	-	+	+/-	Defiers, counteracted
3	+	+	-	+/-	Switchers, counteracted
4	+	-	-	-	Defiers, reinforced
5	-	-	-	-	Defiers, reinforced
6	-	+	-	+/-	Switchers, counteracted or reinforced
7	-	-	+	+/-	Defiers, counteracted or reinforced
8	-	+	+	+	Switchers, reinforced

Table D7.: Effect of particiation incentives on registration and eligibility

	Treatment dummy (1) beta	(2) pp	Gender interaction (3) beta	(4) pp	CEO gender corrected (5) beta	(6) pp	Rep. gender (7) beta	(8) pp	Strata (9) beta	(10) pp
Panel A: Registration										
Free childcare	-0.17 (0.15)	0.07*** (0.01)	-0.19 (0.17)	0.07*** (0.01)	-0.24 (0.17)	0.07*** (0.01)	-0.24 (0.19)	0.07*** (0.01)	-0.20 (0.15)	0.07*** (0.01)
Influencer video	0.00 (0.14)	0.08*** (0.01)	-0.03 (0.16)	0.08*** (0.01)	-0.11 (0.16)	0.08*** (0.01)	0.01 (0.18)	0.08*** (0.01)	0.00 (0.15)	0.08*** (0.01)
Control		0.08*** (0.01)		0.08*** (0.01)		0.08*** (0.01)		0.08*** (0.01)		0.08*** (0.01)
female			0.48* (0.25)	0.12*** (0.01)	0.48* (0.25)	0.13*** (0.01)	1.24*** (0.21)	0.19*** (0.02)		
Free childcare × female			0.11 (0.36)	0.11*** (0.02)	0.28 (0.35)	0.12*** (0.02)	0.19 (0.31)	0.18*** (0.03)		
Influencer video × female			0.17 (0.34)	0.13*** (0.02)	0.45 (0.33)	0.16*** (0.03)	0.01 (0.30)	0.19*** (0.03)		
male				0.07*** (0.00)		0.07*** (0.00)		0.06*** (0.00)		
Control × male				0.08*** (0.01)		0.08*** (0.01)		0.06*** (0.01)		
Control × female				0.12*** (0.02)		0.12*** (0.02)		0.19*** (0.03)		
Free childcare × male				0.06*** (0.01)		0.06*** (0.01)		0.05*** (0.01)		
Influencer video × male				0.08*** (0.01)		0.07*** (0.01)		0.06*** (0.01)		
Constant	-2.39*** (0.10)		-2.48*** (0.11)		-2.48*** (0.11)		-2.71*** (0.13)		-3.51*** (0.46)	
Observations	3894	3894	3894	3894	3894	3894	3894	3894	3894	3894
Strata controls									yes	yes
Panel B: Eligibility										
Free childcare	0.45 (0.30)	0.38*** (0.05)	0.37 (0.34)	0.38*** (0.05)	0.33 (0.34)	0.38*** (0.05)	0.31 (0.40)	0.37*** (0.05)	0.41 (0.32)	0.37*** (0.05)
Influencer video	0.59** (0.29)	0.41*** (0.05)	0.54* (0.32)	0.41*** (0.05)	0.51 (0.33)	0.41*** (0.05)	0.86** (0.37)	0.41*** (0.05)	0.66** (0.32)	0.42*** (0.05)
Control		0.28*** (0.04)		0.28*** (0.04)		0.27*** (0.04)		0.28*** (0.04)		0.29*** (0.04)
female			-0.81 (0.60)	0.27*** (0.05)	-1.20* (0.66)	0.26*** (0.05)	0.12 (0.44)	0.34*** (0.04)		
Free childcare × female			0.47 (0.79)	0.32*** (0.10)	0.86 (0.83)	0.32*** (0.09)	0.32 (0.62)	0.44*** (0.08)		
Influencer video × female			0.38 (0.76)	0.33*** (0.09)	0.74 (0.79)	0.33*** (0.08)	-0.74 (0.60)	0.32*** (0.07)		
male				0.38*** (0.03)		0.39*** (0.03)		0.36*** (0.03)		
Control × male				0.31*** (0.05)		0.32*** (0.05)		0.27*** (0.05)		
Control × female				0.17** (0.08)		0.13* (0.07)		0.29*** (0.07)		
Free childcare × male				0.39*** (0.06)		0.40*** (0.06)		0.33*** (0.06)		
Influencer video × male				0.43*** (0.05)		0.44*** (0.06)		0.46*** (0.06)		
Constant	-0.96*** (0.22)		-0.80*** (0.24)		-0.75*** (0.23)		-1.00*** (0.28)		-1.70 (1.12)	
Observations	311	311	311	311	311	311	311	311	307	307
Strata controls									yes	yes

All models are estimated in Stata 15 SE using logistic regressions.

PP stands for predicted probability.

In column (3), algorithm based firms' gender assignment has been corrected manually based on the name of the person who registered the company and (s)he being the CEO.

In column (4), the same correction as in column(3) but gender is now defined as that of the person who registered the company even if that person was not the CEO.

When strata controls are included, we do not include an interaction term with gender given strata are based on gender.

The sample is reduced to 3894 as we exclude 953 firms with malfunctioning email addresses.

** $p < 0.05$, * $p < 0.1$

Table D8.: Balance table after bouncing for the subject line email experiment

Variable	(1) Control Mean/SE	(2) Productivity Mean/SE	(3) Signaling Mean/SE	(4) Total Mean/SE	(1)-(2)	T-test P-value (1)-(3)	(2)-(3)
firm age	19.19	19.77	20.43	19.79	0.36	0.06*	0.32
	(0.44)	(0.46)	(0.48)	(0.26)			
number of employees	59.83	60.88	62.31	61.00	0.69	0.34	0.58
	(1.85)	(1.81)	(1.86)	(1.06)			
foreign owned	0.30	0.30	0.29	0.30	0.90	0.87	0.78
	(0.02)	(0.02)	(0.02)	(0.01)			
invested capital in million TD	1.50	1.83	1.30	1.55	0.36	0.49	0.10
	(0.23)	(0.28)	(0.17)	(0.13)			
firm representative's gender	0.13	0.12	0.14	0.13	0.55	0.46	0.18
	(0.01)	(0.01)	(0.01)	(0.01)			
tunisian regions	12.94	12.96	13.17	13.02	0.95	0.47	0.50
	(0.23)	(0.22)	(0.23)	(0.13)			
N	879	921	869	2669			
F-test of joint significance (F-stat)					0.31	1.12	1.48
F-test, number of observations					1800	1748	1790

Notes: The value displayed for t-tests are p-values. The value displayed for F-tests are
the F-statistics. Standard errors are robust. All missing values in balance variables are
treated as zero.***, **, and * indicate significance at the 1, 5, and 10 percent critical
level.

Table D9.: Balance table after bouncing for the different incentives email experiment

Variable	(1) Control Mean/SD	(2) Free childcare Mean/SD	(3) Influencer video Mean/SD	(1)-(2)	T-test P-value (1)-(3)	(2)-(3)
fte	111.00	105.94	113.29	0.63	0.84	0.41
	(313.31)	(204.27)	(249.56)			
export== not totally exporting	0.52	0.54	0.55	0.16	0.11	0.83
	(0.43)	(0.43)	(0.43)			
export== totally exporting	0.48	0.46	0.45	0.16	0.11	0.83
	(0.43)	(0.43)	(0.43)			
size==small	0.37	0.37	0.39	0.97	0.26	0.24
	(0.42)	(0.42)	(0.42)			
size==medium	0.39	0.39	0.37	0.57	0.20	0.47
	(0.42)	(0.42)	(0.42)			
size==large	0.15	0.16	0.14	0.33	0.26	0.04**
	(0.31)	(0.32)	(0.29)			
size==big	0.09	0.09	0.10	0.83	0.11	0.07*
	(0.24)	(0.24)	(0.27)			
N	1288	1299	1307			

Notes: The value displayed for t-tests are p-values. Standard deviations are robust.
All missing values in balance variables are treated as zero.***, **, and * indicate
significance at the 1, 5, and 10 percent critical level.