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Winners and losers in industrial policy 2.0

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Abstract: Large-scale business subsidies tied to national industrial development promotion programmes are notoriously difficult to study and are often inseparable from the political economy of large government programmes. We use the Tunisian national firm registry panel database, data on treated firms, and a perceptions survey administered by the National Research Institute to measure the impact of Tunisia's Industrial Upgrading Program. Using inverse propensity score re-weighted differences-in-differences regressions, we find that small treated firms hire more and higher-skilled labour. In small firms, wages increase 10–17 per cent, with growth in employment and net job creation. However, in larger firms the programme does not support labour and wages fall, suggesting that there are no benefits to labour when funds go to large firms.

Key words: firm subsidies, fiscal policy, industrial policy, firm size, impact analysis, labour

JEL classification: H2, O1, L2, O2

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1 Introduction

Industrial policy has long lost favour in light of difficulties regarding the effectiveness and political economy of structural adjustment programmes. The unpopularity of industrial policy grew from its capacity to produce and exacerbate market distortions, as well as the fear of political capture of subsidies in developing countries (Rodrik 2008). However, little differentiates the failures of these types of policies from the failures in long-entrenched and accepted 'horizontal' policies, such as those that subsidize education or health services.[1]

Despite concerns and common pitfalls, industrial policy remains a common intervention for governments. Initiatives targeting industrial development[2] are often used to stimulate growth and employment. At the same time, they have unrevealed political economy ramifications, particularly in authoritarian regimes where the emergence of a robust private sector is considered a threat to state power and control of rents (Cammett 2007; Malik and Awadallah 2013; Rougier 2016). The state guarantees its clients a non-competitive environment and endogenous regulation protecting their interests (Rijkers et al. 2017). An example of one such policy is the Tunisian Industrial Upgrading Program (IUP) implemented in the 1990s to facilitate the country's integration into the world economy.

The literature on firm subsidies suggests that this type of industrial policy can have a positive impact on jobs and output (Bernini et al. 2017; Criscuolo et al. 2019; Einiö 2014), even if this is not always the case for all types of subsidies (Wallsten 2000). Other studies find evidence that positive change in total factor productivity is only captured in the long run, if at all (Bernini et al. 2017; Criscuolo et al. 2019; Einiö 2014). According to Wallsten (2000), firm-level investment subsidies crowd-out self-raised investments. On the other hand, Einiö (2014) finds no evidence of crowding out, and McKenzie et al. (2017) find a positive impact on capital investments and innovations. Furthermore, most studies report finding an anticipation effect before treatment, changes in behaviour during the treatment period, and varying impacts by the type of subsidy received (Bernini et al. 2017; Criscuolo et al. 2019; Hottenrott et al. 2017; Wallsten 2000). The objective of this paper is to contribute to this literature through two main avenues. First, we investigate how firm subsidies impact labour and wages over time in large-scale programmes with staggered treatments. Second, we explore whether the effects are heterogeneous by firm size.

This paper is the first impact evaluation of a large-scale industrial programme in the Middle East and North Africa, and a rare example in developing countries. The size and coverage of these programmes often involve a roll-out of programme implementation over time (De Janvry and Sadoulet 2015). Our identification strategy is based on non-perfect identifiers, similar to the identification strategy used by Criscuolo et al. (2019).[3] Once we identify firms, we use a weighted propensity score matching method to create control groups and extended the analysis with a fixed effects differences-in-differences regression

[1] 'Horizontal' policies can impact all sectors, and do not necessarily have a sector-specific component. Nevertheless, they can impact some sectors differently than others. For example, the protection of business and labour interest groups, or education, training, or health-related policies, are 'horizontal' industrial policies. However, 'horizontal' policies such as training and education are not 'sector-blind'. They are economy-wide but impact some sectors more than others. For example, focusing on technical computer skills training will not help manufacturing production lines as much as it will provide skilled labour for services. On the other hand, 'Vertical' industrial policies have a sector- or firm-specific component and can target entire sectors or firms within sectors. Vertical industrial policies can include, for example, policies specifically for tradable sectors or business subsidies or interest rate reductions targeting one type of sector or economic activity. In the context of developing countries, political capture occurs as frequently in the basic social welfare, or 'horizontal' policies, as in business subsidies and 'vertical' policies.

[2] We focus here on firm subsidy programmes as one type of major industrial policy.

[3] In this paper we provide an additional robustness test on the credibility of our identification strategy, and restrict the analysis to where we have a higher level of sureness in our identification strategy.

analysis (Cadot et al. 2015; Hirano et al. 2003). Similar multiple treatment studies use basic matching techniques or more aggregate synthetic control group methods. This approach is analytically similar to the synthetic control group method with a differences-in-differences regression, except by following the Hirano et al. (2003) approach, we keep the possibility of firm-level analysis.

As a first result, the fixed effects ordinary least squares (OLS) estimates suggest that the programme increased overall employment and wages. However, the effect on employment was not robust to the inclusion of controls and regression readjustment, suggesting that on an aggregate level, the programme did not increase employment, and that this positive impact was due to selection bias. On a more disaggregate level, employment and wages grew in small firms. In our full model, we observed increases in net job creation in smaller firms. The estimates suggest that in small firms workers retained some of the benefits of this programme because they gained in jobs and job quality. Inversely, there is little evidence of wage growth in large firms, but more often significant drops in wages. The decrease in wages is observed jointly with the losses in employment. This observation on wages and employment suggests that, in large firms, there was a substitution of labour. This finding suggests that it is likely that capital owners retained the benefits from the programme. We conclude that this programme's political purpose is welfare-enhancing in small firms, but clientelism in larger firms.

The rest of the paper (1) describes the Tunisian upgrading programme; (2) provides a data description; (3) proposes an identification strategy and econometric approach; (4) discusses the descriptive analysis, regression results, and robustness and sensitivity tests; and finally (5) discusses the results within the political-economy context of Tunisia.

2 The Tunisian Industrial Upgrading Program

The Tunisian IUP, implemented in anticipation of full entry into the free trade agreement with the European Union, initially aimed at bringing the competitiveness of firms to a comparable level after the Multi-Fiber Arrangement for textiles and before the EU free trade agreements (Cammett 2007). At the time of entering the free trade agreement, Tunisia requested additional time and financial assistance to support structural adjustment and competitiveness.

The IUP was initially limited to the manufacturing sector. However, in 1997, services with strong links to manufacturing were added to the list of beneficiary sectors. More than 5,000 grants have been distributed in the last 20 years, corresponding to a total amount of TND1,260 million (Tunisian dinars; around US$500 million). Two-thirds of the amount was spent on material purchases and the rest on immaterial acquisitions (Ben Khalifa 2017).[4] The bureau of the IUP prioritized material investment initiatives that improved product conception, research and development, and laboratory equipment, and immaterial investments that improved productivity and quality of products, the development of new products, and costs related to hiring higher-educated managers (Amara 2016).[5]

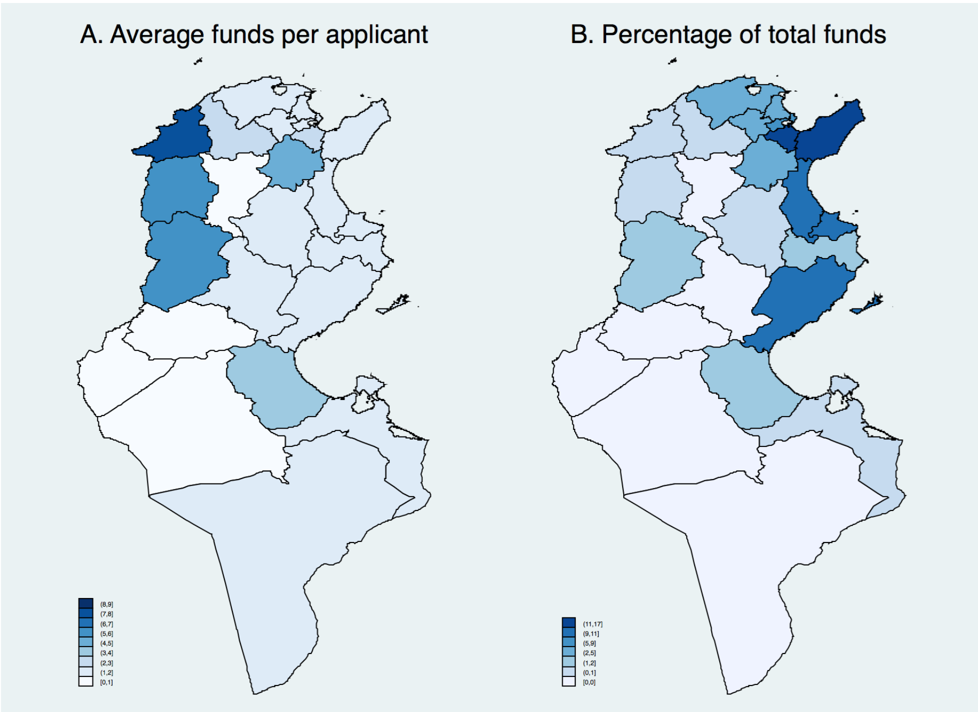
[4] Material investments included technical equipment for management, research and development, and quality-control purposes. These could be targeted at modernizing production equipment, adopting new technologies, diversifying the production of goods, integration of new processes in the production cycle, maintenance, and installation of basic utilities (for example, production chemicals, electricity). Immaterial investments included computer programs and technical assistance (in the upgrading of the productivity within the production process), consulting services, financial advisory, technology transfer, and support in the acquisition of patents and licences.

[5] Like in the Yemeni firm subsidy matching initiative (McKenzie et al. 2017), when the IUP provided material investment support, it required at least some cost matching by firms, and subsidized up to 70 per cent of immaterial expenses up to a ceiling.

The qualification conditions for the IUP are rather straightforward. Firms need at least two years of formal registration (incorporation) and, critically, they need to belong to eligible industries, which include the following: agriculture and food; construction; ceramics and glass; chemicals; textiles, clothing, and leather; mechanical, metal, and electrical work; and diverse industries such as services related to these activities. Inherently, it also required firms to be in the formal market, and that firm fiscal accounts were up to date and legible by the selection committee. According to Murphy (2006), firms wishing to benefit from the programme made an initial application that responded to a set of principles and objectives, rather than a standard application form.[6] If accepted, firms are asked to provide a strategic and financial diagnostic. Technical support was then provided by either technical centres, the *Agence de Promotion de l'Industrie* for public–private partnerships, or private firms.[7]

Most firms that applied received funding.[8] The average funds per applicant[9] was higher in the north-eastern region (Figure 1, Panel A). However, in the last 20 years (1996 to mid-2017), the distribution of total funds for the IUP has been primarily concentrated in the northern coastal regions (Figure 1, Panel B and Appendix Figure A1). Over the entire period of funding, 17 per cent of total funds for the IUP went to the region of Ben Arous. The region of Nabeul received 13 per cent of all funds, and the regions of Monastir, Sfax, and Sousse each received 11 per cent of the total funding pool. The relatively higher approval-to-applicant ratio in the north and eastern coasts (Figure A1, Panel D) was also reflected in the fact that fewer firms applied in those regions (Figure A1, Panel B).[10]

Figure 1: Distribution of IUP funds by region



Note: Rates are weighed by total applicants per region. Total and average funds are in current millions of TND.

Source: Authors' compilation, based on data from the Bureau de Mise à Niveau.

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[6] This was also the case in the firm matching subsidy programme in Yemen (McKenzie et al. 2017).

[7] Further information on the decision to select firms was unfortunately not available for this research.

[8] The fact that firms had to apply to receive funding also means that there is implicit selection bias before firms applied. Firms that applied likely have different observable and non-observable characteristics from those that did not.

[9] Average funds per applicant are weighed by total applicants per region.

[10] This was weighted by total applicants per region.

3 Data description

The primary source of our paper is from the national firm-level enterprise registry (*Répertoire nationale des entreprises*, RNE) administered by the National Statistical Institute (*l'Institut national de la statistique*, INS). It includes data for all formal firms for 18 years, from 2000 to 2017, with close to four million observations. This resource is the most exhaustive source for firm-level data in Tunisia. The database is linked with business turnover, profits, and firm-level employment data from the Ministry of Finance. It was also possible to link the data directly with the national export–import customs database, including export values and volumes on the HS6 product level and by country (for years 2005 to 2010).

For our analytical purposes, we use a sub-sample of firms that had six or more employees in at least one period in the database. The use of this sub-sample means that firms with fewer than six employees at time t may exist as long as the firm had at least six employees at some point in the 18 years of data available in the RNE. The reason for the restriction on the size of firms is two-fold. First, the quality of the data collected for firms with fewer than six employees is low. Second, and more importantly, only firms with six or more employees are *required* to file taxes and can therefore benefit from government subsidies and tax breaks. Firms with fewer than six employees are often informal and do not benefit from the same financial incentives as firms with more than five employees. These two reasons make firms that never had more than six employees incomparable with the former. In firm-level research papers, it is common practice to limit the analysis of firm-level initiatives to firms with more than five employees. This subset, therefore, implicitly reduces differences in observables and non-observables by restraining the subset of firms to those with six or more employees. Furthermore, in the same line of thought, we only apply the analysis to firms that officially qualify to receive funding in the following two ways: (1) by belonging to the eligible manufacturing and services sectors; and (2) who at some year during the panel were at least two years old. Once we identified our treated firms, we used a random sample of firms to draw from as the control group.

In order to identify treated firms, we gathered a database with information on treated firms that included the firm identification number, year of treatment, sector of activity, number of workers, location, and exporter status of firms from 2005 to 2011. Because of administrative barriers and because individual firm identifiers were not always reliable, we used firm characteristics available in the treated data to identify treated firms rather than firm IDs.[11] In the sensitivity analysis section, we discuss how we tested the strength of this treatment identification strategy.

Although financed by the government budget and many donors, a quantitative assessment of this programme incorporating key economic performance data was never undertaken, but as in the case of most evaluations of industrial policies, a qualitative perceptions survey was administered (Ben Khalifa 2017). The raw data of the perceptions survey were made available by the institute of economic studies of the Ministry of Development (*Institut tunisien de la compétitivité et des études quantitatives*, ITCEQ) after our request for research purposes. The questionnaire provides qualitative descriptive, perception-based information. The identification of selected firms within the treatment and control groups was conducted internally in the ITCEQ offices.[12] It includes information from 140 treated firms and 98 non-treated firms that were matched using inverse-propensity score matching on observable firm criteria. The survey is descriptively interesting but limited in its application to rigorous impact evaluation. One limitation of the perceptions survey is that treated firms are firms that were treated in any year before the year of

[11] This is similar to the method used by Criscuolo et al. (2019), who also faced issues related to the reliability of identification of treated firms.

[12] The survey was administered to a sample of treated firms using the same stratification methods as the data collected from the INS. It gathered perceptions of the impact of the IUP on firms for one year.

the survey: 2014. We do not have further information on the year of treatment for the perceptions survey.[13]. Second, ITCEQ reported difficulties in following up with some firms, and therefore there is a slight attrition bias.[14] The perceptions survey is therefore only used descriptively and is not the main basis of the analytical work.

4 Identification strategy and econometric approach

4.1 Identification of treated firms

We faced administrative barriers in matching firm ID numbers to the national business registry data and reliability issues related to the quality of firm IDs. Therefore, part of the barriers to conducting a direct matching was administrative, while others were technical. To address these concerns, we took an approach that most closely resembles an intention-to-treat design that identifies firms that were likely treated, but among whom compliers are unknown.

To identify firms, we merged firm-level treatment identifiers containing information on firm characteristics such as size, sector, locality, and exporter status. Critically, this information was available for firms treated each year from 2005 to 2011. Therefore, our analysis is only on firms treated between these years, with outcomes 1–3 years after treatment. In terms of treatment information, the year of treatment reflects the first year of treatment, but no information is known about subsequent treatments, nor the length of treatment.[15] While not a perfect approach, this method is associated with a downward attenuation bias of our point estimates. All results reported here, therefore, are lower-bound estimates.

In practice, this is similar to the steps taken in the recent paper by Criscuolo et al. (2019), who also faced difficulties on direct firm identification by firm ID numbers when evaluating a large-scale firm-subsidy programme. We can consider the resulting outcome as intention-to-treat effects and a *lower bound of the average treatment effect* since there is a percentage of firms in the treatment group that may not have been treated in reality and some who may have been treated but misplaced into the control group (Chakravarty et al. 2019). We discuss how we test our identification strategy further in the sensitivity analysis in Section 5.3.

4.2 Econometric approach

This paper's econometric approach has multiple steps. As a first step, the paper reports OLS panel fixed effects outcomes with and without controls. Because there is a selection bias to get treatment from the IUP, in the casual inference framework a simple OLS will be biased. One method for overcoming this bias is to find similar control groups that, at least observably, have similar characteristics to those in the treated group. In practice, the matching literature following the seminal work of Rosenbaum and Rubin (1983) is a first step in trying to find comparable firms. In the process, we compared matching algorithms

[13] This is different from the source where we identify treated firms, where we do have the year of treatment

[14] The attrition bias is less than 10 per cent in the data, as reported from the ITCEQ report. Unfortunately, it was not possible to combine this perceptions survey with the RNE due to administrative barriers and authorization requirements.

[15] In total, the treatment identifiers contained 128 sectors, ranging from small to large firms with different export activities totalling 2500 such combinations across the span of years 2005–11. The 'strata' of identified treated firms that is a result of the imperfect identification limitation captures firms that are highly likely to be treated in reality. This strategy is synonymous with a theoretical intention-to-treat model in which there are compliers and non-compliers. The treated groups are weighed by the number of treatments and the number of treated firms within the universe of each stratum. For example, if two firms had the same characteristics and were identified as one treated firm, their group was assigned a weight of 0.5 to account for potential bias in the identification step. More information on the testing of this identification is presented in Section 5.3.

using different calipers and adjustments through bootstrapping, double-robust methods, and a dynamic panel matching model.[16] There were no substantial differences in the estimates, and a marginal change in standard errors.[17] We decided on a distance caliper of 0.001 that reduced the number of firms in our comparison group, but left enough firms in the regression to allow us to use various time-invariant and time-variant controls.

Next, we generated the inverse propensity scores within the common support range and included them in a weighed differences-in-differences regression (IPWDID) as in Hirano et al. (2003), Imbens and Wooldridge (2009), and Cadot et al. (2015). Imbens and Wooldridge (2009) argue that, asymptotically, the use of estimated propensities leads to a more efficient estimator as compared to true propensities. Furthermore, they find that after estimating propensities, the weights given to observations are unbiased. However, one caveat of this method is that very small and very large propensity scores can lead to problems. The intuition behind this problem is that weights will be either too heavy or too weak at the extremes of the distribution and can lead to imprecise estimations. Nevertheless, this issue is less severe in the IPW estimator than a non-weighted regression because the IPW estimator at least reflects the uncertainty of the estimation.[18] In addition to the standardized IPW process, we also include additional weighting to address the issue of non-compliers in our identification strategy, as discussed in the previous section.

Our full econometric specification model is as follows:

$$\begin{aligned} y_{i,t} =& \beta_0 + \beta_1 Treated * After_{i,t} + \beta_2 \sum_{t+n}^{n=3} Treated * After_{i,t+n} + \beta_3 TreatmentGroup_i + \\ & \beta_4 Anticipation_{i,t-1} + \beta_5 \sum_{t}^{n} Treated * After * Year_{i,t} + \beta_6 X'_{i,t}\gamma + \tau_t + \lambda_i + \zeta_i + \varepsilon_i \end{aligned} \tag{1}$$

where the main variable of interest, $y_{i,t}$, is firm-level outcome. Depending on the regression, the main outcome variable will be (the log of) employment, (the log of) average wages per worker, and (the log of) net job creation.[19] A description of all variables is available in Table A1.[20] β_1 captures the interaction term of the treatment in the year of treatment. This interaction term is our main variable of interest. β_2 is a series of time-specific treatment effects that capture the impact of the programme 1–3 years after treatment. β_3 captures the change of the main outcome variable associated with belonging to the treated group. This variable is dropped in the fixed effects panel model but retained in the IPW individual fixed effects model with clustering at the firm level. β_4 estimates the anticipation effect of the programme, one year before treatment. β_5 is a year-specific treatment effect that controls for interactions that the treatment has in each specific year of treatment. β_6 captures the impact of a series of control variables, including age, age-squared, size, distance to ports, and lagged and growth components of the production function. The lagged and growth components control for non-linear time trends before treatment. They include the second lags plus averages of the past 2–4 years of the log of employment, average wages, net job creation, profit, sales, and exports. In the final specifications, we also control for time trends in

[16] Double-robust post-estimation regression-adjustment is an additional procedure that corrects bias in the standard errors when either the propensity score model or the regression model is incorrectly specified.

[17] There were no substantial differences and the analysis with bootstrapping, double-robust estimations, and dynamic panel models requires more computing power than available in the computers in the INS.

[18] More recent proposals for the use of inverse propensity weighting in dynamic treatment models and their applications are being presented by van den Berg and Vikström (2019).

[19] All estimates are in TND deflated for world prices.

[20] While we would have liked to calculate productivity, the database is missing key investment and intermediate input variables.

the treatment variables with time-treatment fixed effects. Finally, we apply year (τ_t), regional (λ_i), and sector (ζ_i) individual fixed effects.[21]

5 Descriptive analysis and regression results

5.1 The perception survey and descriptive findings from the national firm registry

Only one qualitative evaluation of the IUP has been carried out in recent years. The ITCEQ survey administered to IUP recipients collected data on the perceptions of treated and a random set of non-treated firms with retrospective information from 2014 and 2015.[22] The survey found that in the past, treated firms (treated at any point in time) were underperforming against non-treated firms. In comparison to non-treated firms, treated firms less often reported increases in revenue, and less often reported increases in revenue from exports, and employment (Figure 2 and see Figure A2 in the Appendix). If there is no heterogeneity in financial reporting between firms that were treated and those that were not, this suggests that firms were not selected for treatment based on pre-existing performances. On the other hand, a higher share of treated firms had expectations of growth in revenues and employment for the next three years.If this is the case, we should be able observe this empirically.[23]

However, how do these qualitative descriptive findings compare when we evaluate similar outcome variables using registered data on firms (RNE)? To gather similar data from the RNE, we need to create two groups to reflect the groups in the ITCEQ survey, a treatment group (without specifying which year they were treated), and one that includes a comparable set of firms. Descriptive statistics from the national firm registry show only level differences between the treated and control groups, but no difference in growth trends. After treatment, average employment is higher in treated firms, but treated firms pay lower average wages (Figure 3). Interestingly, average revenue per worker is lower after treatment for firms in the treated group than in the control group.

[21] When this combination over-identifies the desired estimation, we use fewer controls and describe it in the table.

[22] The ITCEQ asked firms to report sales, employment, and exports for previous years.

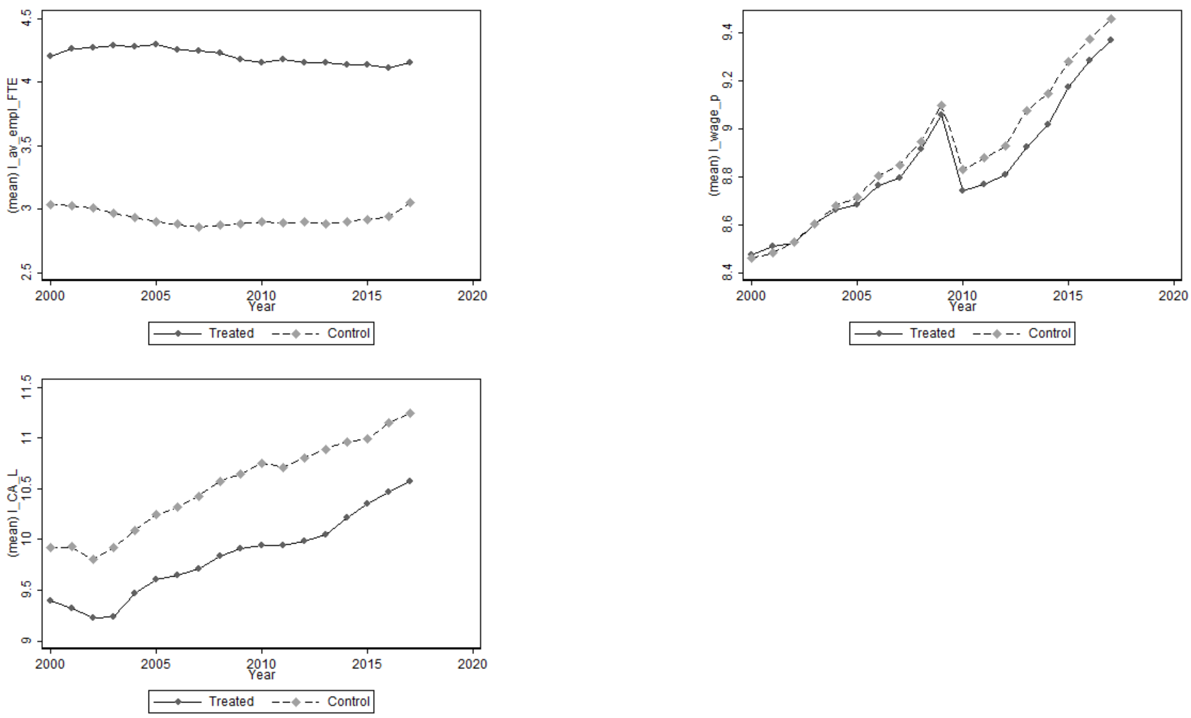
[23] In addition to better future revenue and employment expectations, a higher share of treated firms expected to increase investments after treatment as compared to control firms. The type of investments expected by treated firms differs from those in the control group (Figure A3). Firms belonging to control groups expected higher material investment, whereas more treated firms expected to have more immaterial investments. (While it is not possible to decompose total investment by origin within the ITCEQ survey, the channelling of programme funds to increase immaterial investment suggests that the IUP could have had a substitution effect rather than a complementary effect on firm investments as we saw in the crowding-out effect reported by Wallsten (2000). Unfortunately, without further information on capital and investment on a firm level, we cannot investigate this trend.) Third, treated firms *believed* that they were more competitive after treatment. They reported having observed the most improvements in product quality. Treated firms increased productivity, organization and culture, ICT, and human resources (Figure A4 in the Appendix), as well as used more innovative technology, communications infrastructure, and automated technologies in the workplace (Figures A5 and A6 in the Appendix). For treated firms, the most considerable innovations were innovations in the process and the product, to a lesser degree in innovations in firm organization, and to the lowest degree in marketing. In terms of tangible aspects of innovation, 10 per cent more treated firms reported registering trademarks and filing patents or licences. Remarkably, there were approximately 30 per cent more treated firms that reported filing for ISO 9001 certificates—an international standard that measures the quality of products—than firms in control groups.

Figure 2: Reported increases in employment, revenue, and export outcomes (perceptions survey)

Note: The figure reports the percentage of firms reporting any type of increase of each outcome.

Source: Authors' calculations based on ITCEQ survey.

Figure 3: Employment, revenue, and wages, 2000–17 (registered data)



Note: To make a comparison with the ITCEQ survey, the figure compares firms that were ever treated after treatment, and firms that were never treated.

Source: Authors' calculations based on RNE.

It is likely that the differences in employment, wages, and revenues between the perceptions survey and the registered data are due to the Hawthorne effect—differences are purely due to the perception that outcomes will be better because of treatment.[24] While most evaluations of industrial policies are conducted using perception-based surveys, this comparison shows that perception-based surveys are not an optimal tool for understanding the impact of such programmes. As with other perceptions-based surveys and studies, there are strong incentives for reporting managers to provide overly optimistic responses and be affected by the pure effect of being in the treatment group. The differences between the perceptions survey and the analysis using registered data illustrates why impact assessments linking programme treatment with outcomes for firms are essential.

5.2 Regression analysis

Generating propensity scores to adjust regression analysis

The first step of our analysis involves the estimation of propensity scores. We use a simple logit model to estimate the propensity to be treated. Variables were matched on firm characteristics, including size, restrictiveness, firm origin (foreign or national), firm type (public or private), year, sector, age, age-squared, and coastal (regions by the coast).[25] Like Cadot et al. (2015), the matching criteria also included two-year lags and the average of the 2-4 year lags of employment, total wages, profits, and turnover that accounts for previous trends and avoids direct temporal endogeneity. To control for export ease, we calculated the average distance to the closest two ports using the territorial distance from the city centre where the firm was located to the ports.[26] A summary table of the sample differences between the matched treatment and control group is available in Table A2.[27] The resulting propensity score measured the average treatment effects of the intention-to-treat group and dropped observations that did not fit in the common support range of propensity scores.[28]

The graphical results of the matching procedure based on several outcomes are presented in Figures 4 and 5. Graphically assessing the matching quality of covariates in treated and control groups suggests that the distribution of each of the variables is very similar in both groups (Figure 4) and that they both follow a normal distribution. The bias reduction associated with this process is depicted in Figure 5,

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[24] This is why placebo tests were introduced to perception-based experiments in physical sciences. Alternatively, they may be due to misreporting in registered data. For misreporting to have a net effect, all treated firms would have to similarly misreport, such that the outcome shows no systematic difference between the growth rates of the two groups. We believe this is not credible.

[25] The coastal variable captured trends associated to being located on the coast of Tunisia rather than inland. Much of the economic activity lies in the coastal regions.

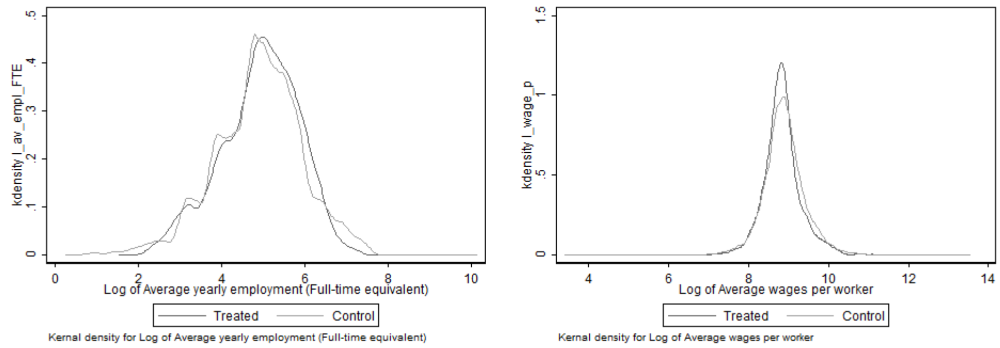
[26] Distance-to-ports variables were estimated using geographical distances from GPS coordinates of the city where firms were located to the GPS coordinate of all current ports in Tunisia. Using the average of two ports establishes some degree of stability of access to ports and other markets in case of recent developments in port expansions.

[27] In the process of estimating the propensity scores, we applied several matching methods starting with the simplest matching algorithm and extending it to tighter restrictions. Following this, we tested whether the performance of the matching improved using Rosenbaum tests, and observed the density plots of propensity scores for treated and control groups. Among the methods used, we applied a strict dynamic Mahalanobis matching, one-to-one nearest-neighbour matching, and kernel-matching procedures with various sizes of calipers. Observing covariate matching and analysis of Rosenbaum bounds on matching estimators guided the selection of matching procedures. In consideration of the marginal changes and the limited improvements of matching using more complicated procedures, we pursued a matching algorithm with a caliper of 0.001, restricted to the common support area, that uses the Abadie and Imbens (2006) standard errors with conditional covariances calculated using two neighbours. Computational limitations on-site and time access controls limited how many variations of the algorithm we were able to appropriately assess using our final specifications, but in practice not much changed between different matching options. The initial matching procedure with different calipers and a full Mahalanobis-metric matching reduced matching bias in approximately the same amount, but was heavier in computational power.

[28] We decided against using a dynamic replacement model both because of computational limitations and because we want to prioritize consistently estimating the closest matching propensities within the regressions.

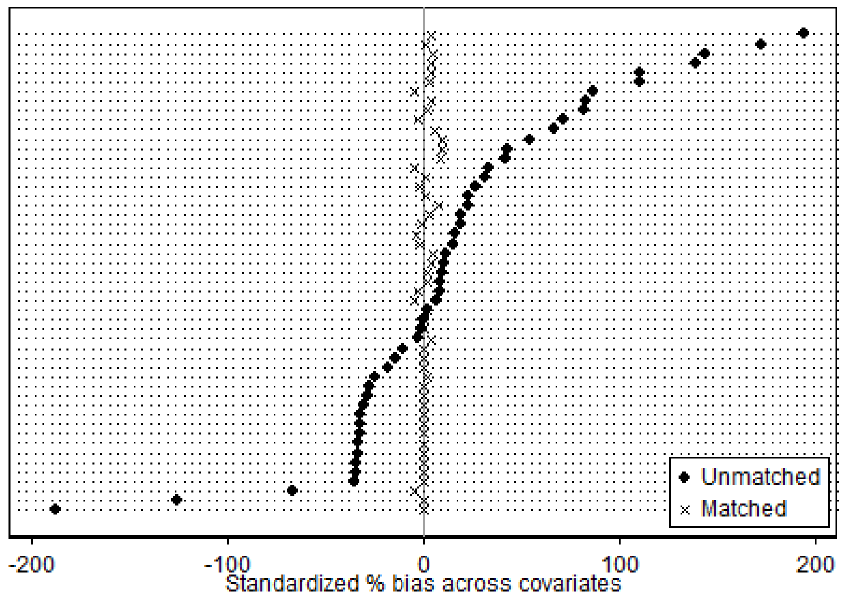
which consists of a list of all matching covariates, and graphically illustrates the gains in comparability resulting from the removal of observations outside the common support and further away from the propensity values of the treated variables.

Figure 4: Matching performance: kernel density of employment and wages



Source: Authors' calculations based on RNE.

Figure 5: Matching performance: variable bias reduction



Source: Authors' calculations based on RNE.

After the removal of firms outside the common support, there were approximately 2,000 treated observations, and approximately 68,000 observations in the control group. These numbers were similar when we ran matching algorithms separately for employment and wages. While the matching performance looks promising, there are still caveats. We argue, however, that regression adjustment methods such as the IPW builds on a simple OLS. The limitations to the use of the propensity score method are well-known (Caliendo and Kopeinig 2008; Dehejia and Wahba 2002; King and Nielsen 2019).

In the literature, there are, in general, two types of issues that occur when using propensity scores in estimation procedures. The first involves how the researcher uses the method, and the second involves its econometric limitations. To avoid the first type of issue, we started from the most stringent model, based on the empirical literature that uses this method. One-by-one, we relaxed conditions until we found a combination that brought us the closest to finding matched pairs using both visual propensity score plots and bias reduction summary statistics after matching. We pursued the model on which we were able to include a reasonable amount of controls in the final IPW regression without losing so many observations that our regressions with standard controls were over-fitted.

The second issue is addressed in how we defined our matching algorithm and used it in the next step. We matched on pre-trends and reintroduced controls in the regression adjusted model that incorporated propensity scores. We controlled for lagged trends, current individual fixed effects, and average time trends. The variable that caused the most difficulty was matching on lagged 2–4 years. While this increased the credibility of our matching process, it also limited the availability of years for our regression, as each observation would now need at least four prior years of data and three years of data after the year of treatment. This automatically limited our analysis to firms that had at least seven years of continuous information.

The use of the propensity score matching method is only an initial step in our analysis. The propensity scores are then integrated as weights into our differences-in-differences analysis, which theoretically reduces our bias and improves our estimations from our original OLS differences-in-differences model (Imbens and Wooldridge 2009). While ignorability of matched assignments given observable characteristics may still be a concern, our matching outcomes are not the final results of our estimation strategy. With the IPWDID we have the opportunity to apply additional control variables in a second stage that attempts to control for selection on observed growth and time-trend variables.

Regression results on wages

Results for the impact of the programme on wages and employment are reported in Tables 1 (wages) and 3 (employment). We report the basic OLS results of treatment with and without controls in columns (1)–(3). In column (1) of Table 1 with only year and sector controls, we see that there is no statistically significant impact of this programme on wages. However, there is a positive anticipation effect the year before treatment. Wages increase by 3 per cent the year before treatment. This is consistent with a story that suggests that firms may anticipate demand for higher-waged workers, or rearrange the occupation composition of workers towards workers in higher-paid occupations. We then add further controls for size groups of firms, the age of firms (age and age-squared), the origin of the firm (foreign or local), the type of firm (public or private), whether the firm is geographically in the coastal regions, whether the firm is only an exporting firm (or also sells locally), the distance to the nearest port, and various controls for growth.[29]. Column (2) reports estimates after the inclusion of these additional control variables. The estimated effect of the programme on wages is still not statistically different than 0. However, there is growth in wages in the three years following treatment that is consistently close to 2 per cent. When including additional controls, the anticipation effect is reduced by one-third of its original estimate. Lastly, in column (3), if we include controls for the year-specific treatment effect,[30] the impact of the programme on wages is positive, although small, but significant at the 5 per cent level in the year of treatment, and the three years following treatment. A treated firm has, on average, 1.3 per cent higher average wages, and the impact doubles to close to 2 per cent in the following years.

[29] This includes growth of employment, wages, net job growth, sales, and export value from the previous year; the lag of the values of the same variables, and the 2–4 year lag of values of those same variables.

[30] This is an interaction between when the firm is treated and the fact that it is treated in a specific year. Its purpose is to control for time-specific treatment effects that may vary from year to year. This includes individual controls for treatment in 2000, 2001, and so on.

Table 1: Impact of the IUP on average wages, 2000–17

Log of	OLS fixed effects models			Reg. adj. models	
ave. wages	(1)	(2)	(3)	(4) PSM	(5) IPW
Treatment	–0.003	0.007	0.013**	–0.070***	0.023**
	[–0.447]	[1.208]	[2.081]	[–5.134]	[2.249]
One year after	0.004	0.018***	0.021***		–0.006
	[0.579]	[3.621]	[3.646]		[–0.486]
Two years after	0.007	0.020***	0.020***		–0.012
	[1.118]	[3.625]	[3.249]		[–1.133]
Three years after	0.003	0.019***	0.017***		–0.008
	[0.430]	[3.126]	[2.605]		[–0.672]
Anticipation	0.030***	0.011**	0.022***		–0.008
	[4.654]	[2.052]	[3.687]		[–0.637]
Treat * year	No	No	Yes	No	Yes
Age controls	No	Yes	Yes	Yes	Yes
Growth and lags	No	Yes	Yes	Yes	Yes
Type and origin	No	Yes	Yes	Yes	Yes
Coastal and port	No	Yes	Yes	Yes	Yes
Year and sector	Yes	Yes	Yes	Yes	Yes
Observations	327,234	195,501	195,501	69,077	69,077
R-squared	0.347	0.458	0.458	0.0004	0.693
Method	FE	FE	FE	PSM	IPW

Note: Robust *t*-statistics in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. IPW estimates are double weighted. The first weight corresponds with a logit propensity that weights control and treated groups by their propensity to be treated. The second weight is a correction weight from the identification strategy. The number of firms in the OLS model is 34,559 for column (1) and 28,336 for columns (2) and (3). The differences between the models are due to a lack of historical information for 2–4 years prior to when the firms appear in the database.
Source: Authors' compilation based on RNE.

Nevertheless, selection bias due to the firm application procedure, the selection procedure, and the type of treatment they received would imply that treatment and control groups are not comparable. To get a more credible estimate, it would be best to compare similar groups that at least have similar observable characteristics. As previously discussed, our method to address this issue is to estimate the probability of treatment in the year t (propensity score) and integrate this into an inverse weighted differences-in-differences model. Column (4) of Table 1 provides an estimate of the average treatment effect when matching on observable covariates, without the interacted year and treatment effects.[31] The estimation of the average treatment effect is counter-intuitively –7 per cent. However, the estimation's power is very low (R-squared is 0.0004). When we use the propensity score from this logistical regression and include time-specific treatment effects, our estimated impact of the programme on wages is +2.3 per cent in the year of treatment and not significantly different from 0 in the years thereafter. The explanatory power of this IPW model in column (5) suggests that there is less variance in the error terms in this specification. The difference between the OLS and the IPW model suggests that a simple OLS was indeed suffering from selection bias. If this was not the case, we would have expected the OLS results to look similar to the IPW results that measure differences between observationally similar firms.

Not all firms experienced the impact of the IUP in the same way (Table 2). The most substantial positive impact on wages—in order of strength in magnitude—is in small firms (5–9 employees), small to medium firms (10–19), and medium firms (20–49). Small firms increased wages by close to 18 per cent one year after treatment. Wages in firms with 10–19 employees were more volatile but were net positive 2–3 years after treatment. Lastly, medium-sized firms with 20–49 employees showed a 9 per

[31] With these included, the logistical regression to estimate the propensity for treatment was over-identified.

cent increase in wages in the year of treatment. This increase was followed by a 5 per cent and a 4.5 per cent growth in average wages 2–3 years after treatment.

Table 2: Impact of the IUP on average wages, by size

Log of wages	(1) Small [5, 9]	(2) Sm-Med [10, 19]	(3) Medium [20, 49]	(4) Med-Lge [50, 99]	(5) Large [100, 199]	(6) Very lge [200, 999]
Treatment	–0.004	0.015	0.091***	0.049***	0.019	0.059***
	[–0.082]	[0.528]	[4.594]	[3.256]	[0.918]	[3.985]
One year after	0.177***	–0.0003	0.050*	–0.021	–0.063***	–0.019
	[4.735]	[–0.009]	[1.759]	[–1.319]	[–3.048]	[–1.065]
Two years after	0.219	–0.090**	0.030	–0.031*	–0.048**	–0.031
	[0.861]	[–2.294]	[1.240]	[–1.944]	[–2.353]	[–1.568]
Three years after	–0.134	0.119**	0.045**	–0.015	–0.036	–0.009
	[–1.578]	[2.116]	[2.047]	[–0.900]	[–1.503]	[–0.504]
Anticipation	–0.024	–0.043	–0.002	–0.066***	–0.005	–0.030
	[–0.302]	[–1.333]	[–0.085]	[–4.196]	[–0.190]	[–1.566]
Observations	31,203	12,108	11,314	6,496	4,344	3,354
R-squared	0.783	0.771	0.768	0.745	0.647	0.795
Method	IPW	IPW	IPW	IPW	IPW	IPW

Note: Robust *t*-statistics are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Controls includes fixed treatment group effect, year, treatment and year interaction effects, sector controls, growth and lags of only wage variables. Because of a more limited number of observations, we could not include other controls. The method is IPW with standard errors clustered at the firm level.
Source: Authors' compilation based on RNE.

There was some impact on medium to large (50–99), large (100–199), and very large firms (200–999), but the results do not suggest that the changes due to the IUP were as strongly in favour of higher wages as they were in small firms. Wages grew in the first year for medium to large firms (50–99) and very large firms (200–999), but the growth in wages was to a lesser degree than the growth observed for smaller firms. Furthermore, wages in these two types of firms dropped again 2–3 years later. Medium to large firms may have had positive increases in wages the year of treatment, but the increases were anticipated with a drop in wages the year prior to treatment. These estimates suggests that, in the year prior to receiving treatment, firms either dismissed high-wage workers without replacement or that low-wage workers replaced high-wage workers. Therefore, the growth in wages in the first year of treatment may have either been firms re-hiring high-wage workers or temporarily augmenting salaries. But this growth does not last and even shows signs on a net decrease two years later. Lastly, large firms with 100–199 employees showed a decrease in average wages 2–3 years after having received treatment from the IUP. For large firms, average wages dropped by 6.3 per cent in the second year after treatment, and 4.8 per cent in the third year after treatment. As we will see in the employment section in Table 4, this drop in wages was accompanied by a 3 per cent growth in employment in the third year. Jointly, this suggests that the programme was not effective for jobs creation and quality in large firms, but it is likely that the employment strategy in large firms changed after IUP treatment to one with more low-wage labour.

Regression results on employment

On the aggregate level, we observe a sizeable impact on employment in the first year of treatment and every year thereafter when we only control for year and sector fixed effects (column (1) in Table 3). There is also a relatively large and strong anticipation effect one year before treatment. However, including additional controls diminishes the impact of the programme sharply (down to 1–2 per cent) and the anticipation effect becomes statistically insignificant (column (2) in Table 3). These estimates suggest that most of the initially observed growth in employment is explained by the characteristics of the firms rather than the treatment.

Table 3: Impact of the IUP on employment

Log of employment	OLS fixed effects models (1)	(2)	(3)	Reg. adj. models (4) PSM	(5) IPW
Treatment	0.260***	0.016***	0.011*	1.545***	0.001
	[19.282]	[2.745]	[1.658]	[52.40]	[0.162]
One year after	0.133***	0.021***	0.015**		0.005
	[10.221]	[3.804]	[2.411]		[0.612]
Two years after	0.093***	0.020***	0.017***		0.001
	[6.996]	[3.507]	[2.792]		[0.115]
Three years after	0.099***	0.013*	0.014**		0.012
	[6.177]	[1.940]	[2.010]		[1.166]
Anticipation	0.169***	0.009	0.003		–0.016
	[12.415]	[1.570]	[0.433]		[–1.549]
Treat * year	No	No	Yes	No	Yes
Age controls	No	Yes	Yes	Yes	Yes
Growth and lags	No	Yes	Yes	Yes	Yes
Type and origin	No	Yes	Yes	Yes	Yes
Coastal and port	No	Yes	Yes	Yes	Yes
Year and sector	Yes	Yes	Yes	Yes	Yes
Observations	328,536	195,501	195,501	69,077	69,077
R-squared	0.010	0.606	0.606	0.038	0.949
Method	FE	FE	FE	PSM	IPW

Note: *t*-statistics are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. IPW estimates are double weighted. The first weight corresponds with a logit propensity that weights control and treated groups by their propensity to be treated. The second weight is a correction weight from the identification strategy. The number of firms in the OLS model is 34,234 for column (1) and 28,336 for columns (2) and (3). The differences between the models are due to a lack of historical information for 2–4 years prior to when the firms appear in the database.

Source: Authors' compilation based on RNE.

Table 4: Impact of the IUP on employment, by size

Log of employment	(1) Small [5, 9]	(2) Sm-Med [10, 19]	(3) Medium [20, 49]	(4) Med-Lge [50, 99]	(5) Large [100, 199]	(6) Very lge [200, 999]
Treatment	0.518***	–0.031	0.010	–0.005	0.019*	–0.082***
	[12.203]	[–1.577]	[0.689]	[–0.502]	[1.712]	[–3.981]
One year after	0.135	0.076*	0.047**	0.033**	–0.013	–0.047**
	[1.465]	[1.910]	[2.225]	[2.481]	[–1.064]	[–2.298]
Two years after	0.127*	0.110**	0.012	0.012	0.014	0.003
	[1.719]	[2.530]	[0.456]	[0.868]	[1.037]	[0.116]
Three years after	–0.095	–0.064	0.097***	0.002	0.037**	–0.023
	[–0.846]	[–1.620]	[3.506]	[0.098]	[2.426]	[–0.794]
Anticipation	0.173***	0.013	0.023	–0.025**	0.014	–0.074***
	[3.039]	[0.398]	[1.108]	[–2.008]	[0.936]	[–3.013]
Observations	31,203	12,108	11,314	6,496	4,344	3,354
R-squared	0.269	0.103	0.149	0.135	0.131	0.362
Method	IPW	IPW	IPW	IPW	IPW	IPW

Note: Robust *t*-statistics are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Controls includes fixed treatment group effect, year, treatment and year interaction effects, sector controls, and growth and lags of only employment variables. Because of a more limited number of observations, we could not include other controls. The method is IPW with standard errors clustered at the firm level.

Source: Authors' compilation based on RNE.

As in the previous regression analysis, there are still significant differences between the treatment and control groups that are due to selection bias, making a direct comparison between the two groups impossible. To address this, we integrated propensity scores in regressions, adjusted to include propensity weights. The average treatment effect from the simple matching exercise in column (4) of Table 3 shows that employment increased substantially, as do the OLS models with fixed effects in columns (1–3). However, the treatment explained very little of why employment increased (R-squared of 0.038, as compared to 0.606 in the OLS panel fixed effects model). When we integrate the probability of being in the treated group in our differences-in-differences model, our estimates are no longer significant. The inverted propensity score difference-in-differences regression has a rather high explanatory power, with an R-squared of close to 1. The explanatory strength of this specification suggests that changes in employment were not likely due to the IUP treatment.

Nevertheless, these results were not homogeneous for all firms. In the year of treatment, small firms (5–9 employees) grew by close to 50 per cent and continued to grow two years after treatment. They increased employment the year before treatment. Small to medium-sized firms (10–19) grew in the first and second years after treatment. Medium-sized firms (20–49) grew in the first and third years after treatment. At least in small firms, firms increased employment in anticipation of treatment.

There was some measurable impact on employment in firms with over 50 employees, but positive impacts were smaller in magnitude, and in some cases the impact was negative. For example, in medium to large firms (50–99) there was a 3 per cent growth in employment the year after treatment, but this growth was smaller than other groups and was preceded by a similarly sized decrease in employment in anticipation of the treatment. Large firms (100–199) saw a 3.7 per cent increase in employment the third year after treatment, but this coincided with two previous years of drops in wages (from Table 2). Most strikingly, the programme was the least supportive of labour in very large firms (200–999). In very large firms, the programme had a negative impact on employment (8 per cent) in the year of treatment and a 4.7 per cent decrease in the first year after treatment. For these firms, the large drop in employment coincided with the year of a large drop in wages. This fall in employment and wages suggests that large firms adopted strategies not very beneficial for labour (such as restructuring).

Trends in net job creation[32] provide additional support for the findings on employment by looking at the churning patterns of firms within each size group (Table 5). Our decision to include this is to address the concern that stronger growth in smaller firms could only be due to composition effects. In columns (1)–(3), firms in the small to medium-sized groups all show an increase in net job creation in the year of treatment, even if there were some anticipation effects in the small to medium and medium categories. There are almost no measurable results of the programme on net job creation in larger firms. While medium to large firms (50–99) may have some growth in net job creation, it is in the third year, and smaller than the growth in the smaller firms. There is a negative (but not very significant) impact of the program on net job creation in very large firms (200–999).

Size and industrial policies

On an aggregate level, the IUP marginally improved wages (2.3 per cent in the year of treatment column (5) of Table 1), but there is less convincing evidence that the IUP was good for employment. Taken at face value, the global picture shows that the programme increased wages for workers, but did not change the number of workers employed. This finding is in line with the overarching goals of the IUP.

The heterogeneous estimates of the IUP on wages and employment depict a different story. These results suggest that the IUP was successful both in terms of employment and wages in smaller firms in the Tunisian economy. If the strategy of a firm is to increase competitiveness through more qualified workers, then we should observe an increase in wages per worker. If more qualified workers do not crowd out other types of workers, then we should also see a growth in net job creation, and to a lesser degree in employment. In smaller firms, we observe both an increase in wages and a growth in employment

[32] As a reminder, net job creation is an increase in the number of individuals employed, minus the losses in the number of individuals employed.

and net job creation. However, there is almost no evidence that large and very large firms increased net job creation in the year of treatment or any of the three years following. The strategy for smaller firms, therefore, suggests an upgrading of labour resources strategy, while the strategy for larger firms seems to be one of labour substitution.

Table 5: Impact of the IUP on net job creation, by size

Log of net job creation	(1) Small [5, 9]	(2) Sm-Med [10, 19]	(3) Medium [20, 49]	(4) Med-Lge [50, 99]	(5) Large [100, 199]	(6) Very lge [200, 999]
	[4.366]	[2.569]	[3.818]	[0.382]	[–0.527]	[–0.414]
One year after	0.091	0.045	0.028	–0.250	–0.296	–0.723
	[0.257]	[0.128]	[0.143]	[–0.965]	[–1.157]	[–1.621]
Two years after	0.016	0.346	–0.071	–0.155	–0.173	–0.883*
	[0.036]	[1.386]	[–0.338]	[–0.615]	[–0.627]	[–1.931]
Three years after	–0.270	–0.042	0.050	0.481**	0.353	0.754
	[–0.729]	[–0.130]	[0.228]	[1.972]	[1.191]	[1.565]
Anticipation	–0.027	–0.843***	–1.008***	–0.122	0.405	1.384***
	[–0.049]	[–2.863]	[–5.378]	[–0.506]	[1.562]	[2.672]
Observations	31,203	12,108	11,314	6,496	4,344	3,354
R-squared	0.269	0.103	0.149	0.135	0.131	0.362
Method	IPW	IPW	IPW	IPW	IPW	IPW

Note: Robust *t*-statistics are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Controls includes fixed treatment group effect, year, treatment and year interaction effects, sector controls, and growth and lags of only net job creation variables. Because of a more limited number of observations, we could not include other controls. The method is IPW with standard errors clustered at the firm level.

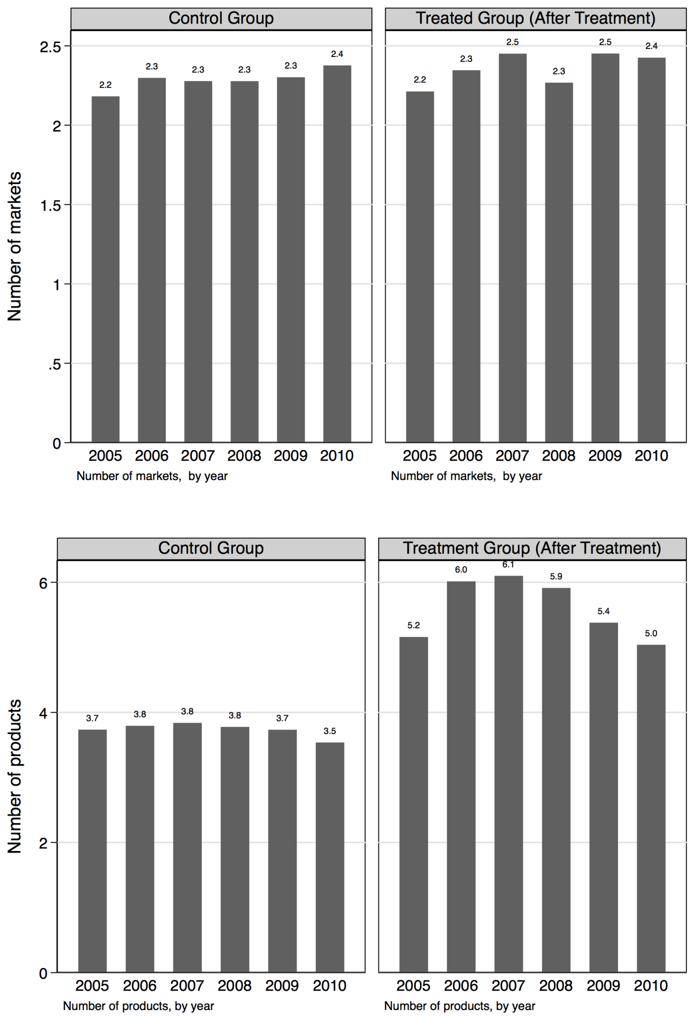
Source: Authors' compilation based on RNE.

Clearly, the strategy associated with the IUP was not the same in larger firms. If machines replaced humans for low-skilled routine tasks only, we should observe increases in wages per worker due to changes in the occupational composition of the workforce and a decrease in employment due to machine-based displacement (Acemoglu and Autor 2011; Acemoglu and Restrepo 2018). While we do not have data on material investments to capture whether there were additional investments in heavy machinery, we do observe this trend in wages and employment only in very large firms (200–999 employees), and only in the first year. While the programme had some benefits for labour in small firms, its purpose in large firms does not suggest that it supports the larger welfare of labour resources. Some authors suggest that the purpose of providing funds for larger firms was a political tactic rather than part of an initiative to support labour (Cammett 2007; Murphy 2006).

Exports by treatment group

One major target of the IUP was to help competition in exports. One way of looking at whether the IUP increased global competitiveness is to evaluate changes in aggregate export outcomes. Because the programme was meant to help firms face new challenges from external competition, we expect firms to compete on either prices, markets, or goods. As demonstrated in Figure 6, treatment groups did not have notably higher access to markets, but they did have a more diverse set of products for exports. While these findings are not causal on a firm level, they demonstrate that treated firms increasingly diversified products but kept similar market strategies on an aggregate level.

Figure 6: Product and market diversification, by treatment status



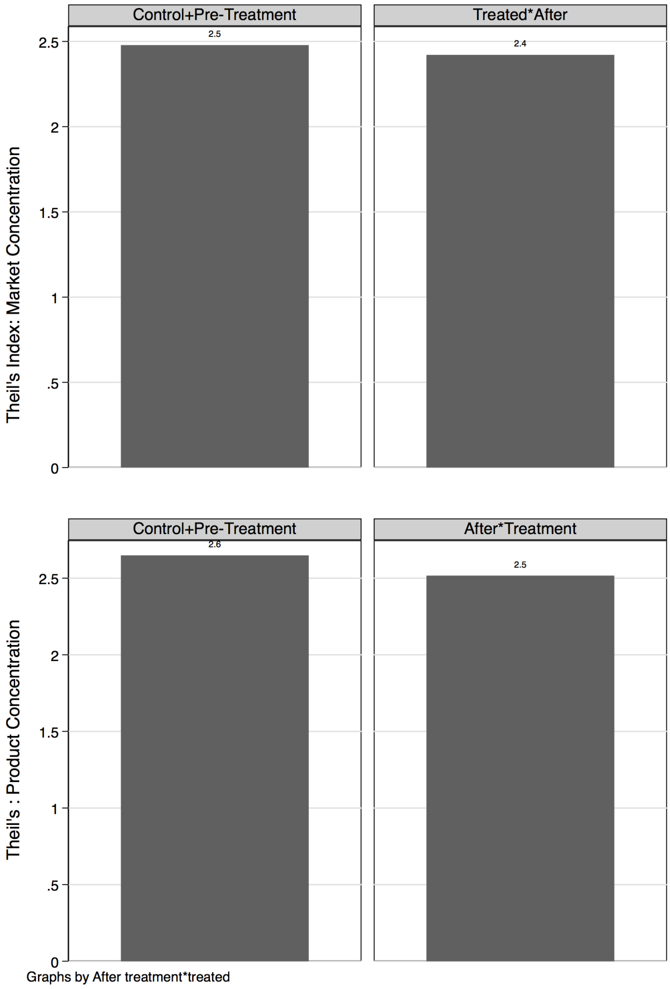
Source: Authors' calculations based on RNE.

In addition to descriptive trends on the extensive and intensive margins of trade, we may also be interested in export strategies for treated and control groups as an outcome of the implementation of the IUP. One way to do so is to analyse changes in the price concentration of difference exports to international prices in markets. We can evaluate the relative concentration of markets and products using the Theil's entropy index (Theil 1972), given by

$$T = \frac{1}{n}\sum_{k=1}^{n}\frac{x_k}{\mu}ln\left(\frac{x_k}{\mu}n\right) \quad where \quad \mu = \frac{1}{n}\sum_{k=1}^{n}X_k \tag{2}$$

The Theil index measures an entropic 'distance' the population is away from the egalitarian state of everyone having the same values. If all firms have the same values of products (or value of exports to markets), then the index will go towards 0. The index is higher with more diversified values of exports to markets and for products. Unlike the figures provided on the number of products and the number of markets firms access, the Theil index shows how varied export *values* are by markets and products. Figure 7 shows that treated firms have marginally less variance in prices of exports by market and goods than control firms. Prices in export markets and products converge to the egalitarian state after treatment, suggesting a specialization strategy among treated firms.

Figure 7: Price concentration using the Theil entropy index, by treatment status (after)



Source: Authors' calculations based on RNE.

Interpreting these findings together provides some information on export strategies post-treatment. After treatment, treated firms have more concentrated export values both by markets and products (lower variances), but export a higher number of products (even though product price diversification is falling). Non-treated firms are more diversified in values of exports (higher variances) and in goods, but export fewer products than treated firms. Treated firms are producing more exports of goods to markets that are sold at relatively similar prices.[33] In the literature, Cadot et al. (2011) suggest that exports trends move between diversification and concentration in a U-shaped pattern as GDP per capita as PPP prices increase. The macroeconomic comparison here suggests that the IUP pushed firms to more diversified product exports (but not markets) and to more concentrated prices.[34]

We have broken down export diversification to understand group trends (Appendix Tables A3 and A4). The tables suggest that an overwhelming portion of outcomes is explained by changes in the intensive margin of trade (or specialization) rather than diversification (extensive margin). The decomposition of market and product concentration show that the relatively more concentrated outcomes are a result of specialization within treatment and control groups, rather than due to movement in or out of treatment

[33] Similar to other treated firms.

[34] If we take this as progress, then this situates Tunisian firms to the left side of the midpoint of the U-shaped Theil curve for exports, placing them on the left of the median level of export diversification. We expect that as the Theil index approaches 0, the concentration of exports will slow down, and eventually reverse, pushing firms to diversify again.

and control groups. Anecdotally, the Tunisian shoemaker that is a beneficiary of the programme is now more likely to be selling the same leather sandals as his neighbour, who is a beneficiary of the programme, but this will differ from the type of shoe their non-treated neighbour sells.

While we were unfortunately not able to capture this in the regression analysis due to data limitations,[35] the macro-level analysis suggests macroeconomic-level diversification of exports and concentration of prices.

5.3 Caveats and sensitivity testing

The results and interpretations elaborated in the regression analysis and the macroeconomic trade section should be interpreted with some caution. First, there may be concern about the robustness of our identification strategy. We argue that, like an intention-to-treat intervention, there is a variation among firms in the treatment group that are always-takers, never-takers, and defiers that can potentially confound results. We argue that this would cause a downward bias in the magnitude and preciseness (attenuation bias), which is the lesser evil of two less-than-optimal outcomes. We argue that outcomes of this choice of identification strategy only dilute the full measure of the impact of the programme.

After sensitivity tests, we kept treated firms where we were reasonably sure of the identification. We measured how 'sure' we were of treatment by ranking the weights and selecting only those above the median level of sureness. Because some firms were treated multiple times, the identified firms that we keep in the regression have weights ranging from 1 to 7. In the sample we dropped, the range for weights within groups was from 0.2 to 1.[36] Additional sensitivity and robustness tests in further sections suggest that our estimates are lower-bound estimates and that the dilution of our treatment variable is only causing attenuation bias (regression dilution caused by random noise) as manually inducing errors in variables even by marginal amounts (1, 2.5, and 5 per cent) results in outcomes that lose significance and fall in magnitude.

To convince readers that our identification is not spurious, we can show in Figure 6 that even inducing marginal errors of 1, 2.5, and 5 per cent of a transitional type (movement in and out of treatment groups) induces attenuation by reducing the standard errors and magnitude of the beta terms, but does not overestimate the treatment effect. If anything, the results presented demonstrate that even the non-significant findings are potentially informative. To induce the error in variables of the treatment, we randomly allocated half of the 1 per cent (and later 2.5 and 5 per cent) of treated firms and placed them into the non-treatment group, and a second random half of the non-treatment group into the treatment group. The results in Table 6 show that values decrease in significance and magnitudes, as measured by a decrease in *t*-scores, but do not change in direction.

Another caveat is that we have no information on the types of subsidies received. The programme information provided officially tells readers that the programme varies between material and non-material subsidies and firm support, but it was not clear how each type of subsidy is distributed among firms or in what form. There may be heterogeneities in the type of treatment received by firms that may also be driving results. Unfortunately, we are not able to distinguish the impact of the different types of material and non-material investments that were provided to beneficiaries. As we saw from the ITCEQ survey, both are relevant for treated firms, and treated firms reported expecting more non-material investments than non-treated firms. A strong shift in material and non-material investments may have been useful

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[35] Detailed export data were only available for a subset of the years that other data were available (2005–10).

[36] Thanks to Bob Rijkers for suggesting this sensitivity test, and subsequent adaption. We ranked all the weights by distribution. At the 18th percentile, all strata showed at least a one-to-one match. In testing the sensitivity of our choice of 'level of sureness', we increased the level of stringency until we reached a limit, after which our betas were no longer stable. This is the identification issue associated with the re-weighting that appears in the regression.

to provide more information on how the funds are being used, but this was not available. It would have been optimal, if possible, to have access to further information on investments and value-added to estimate measures of productivity, as boosting competitiveness is more sustainable through productivity growth.

Table 6: Sensitivity check using artificially induced errors in identification of treated firms

	Average treatment effects (PSM)			
	Original	0.5% error	1% error	2.5% error
Log of employment	1.545***	0.259***	0.092***	0.047***
	[52.397]	[8.110]	[3.899]	[2.634]
Log of wages	–0.070***	–0.024	–0.005	–0.010
	[–5.134]	[–1.444]	[–0.448]	[–1.020]

Note: *t*-statistics are reported in brackets.

Source: Authors' calculations based on RNE.

6 Conclusion: winners and losers

The Tunisian IUP was not a failure, but it was not a landslide victory either. There were variations in the distribution of outcomes between different firms, which gave the programme its wins and losses. This paper links the heterogeneity of outcomes to the model of economic governance in countries with a strong state and close government–business ties. As predicted by the literature, some aspects were successful. The reported perceptions from the ITCEQ survey led us to believe that employment should grow as a result of the programme. However, workers are winners only when small firms receive IUP funds. Inversely, it is more likely that profits are retained by capital owners when treatment is assigned to large and very large firms. When smaller firms receive treatment, wages, employment, and net job creation increase and the firm strategy is to employ more high-skilled labour. When larger firms receive treatment, the firm strategy is one in which employment and wages fall. Lastly, macroeconomic outcomes suggest that the IUP increased diversification of products exported but encouraged a price concentration strategy among treated firms.

Some command-led initiatives can provide benefits in the initial stages of development (Cammett 2007; Murphy 2006). However, the findings of this paper do not suggest that the IUP always serves the wider public. Furthermore, stakeholders argued that the IUP would help Tunisian firms access new markets. As we have seen from the trade and employment analysis, there is little evidence of outreach to new markets, and we see an increase in skills (proxied by wages), but only in small firms. While there was at least some increase in employment and wages in small firms and evidence of increased product exports, there is little to show for modernization impacts on overall job quality, except in small firms. While the literature tells us that big firms are important (Freund and Pierola 2015), if we care about jobs then industrial policies are better targeted towards small firms (Criscuolo et al. 2019).

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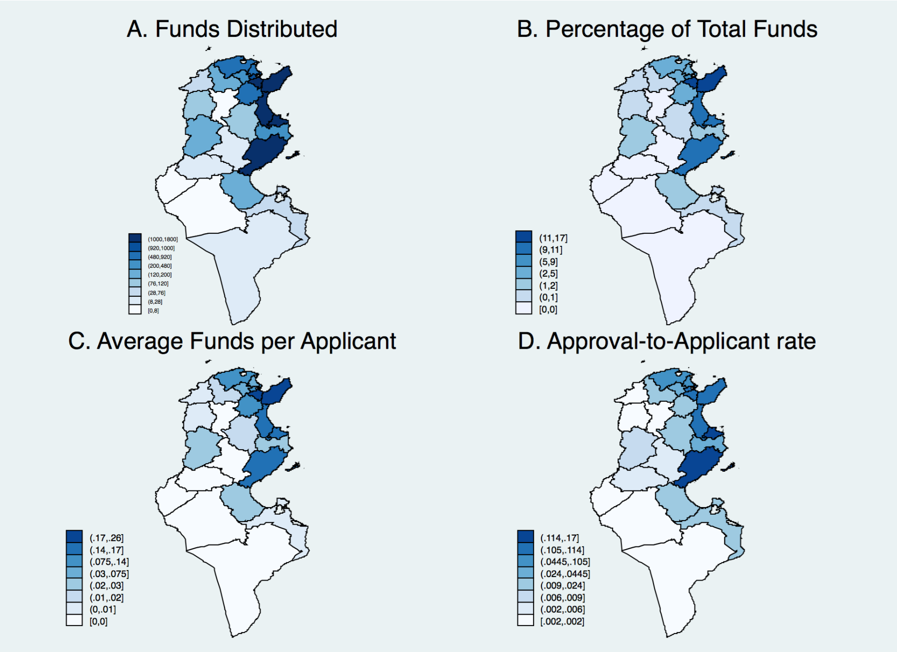
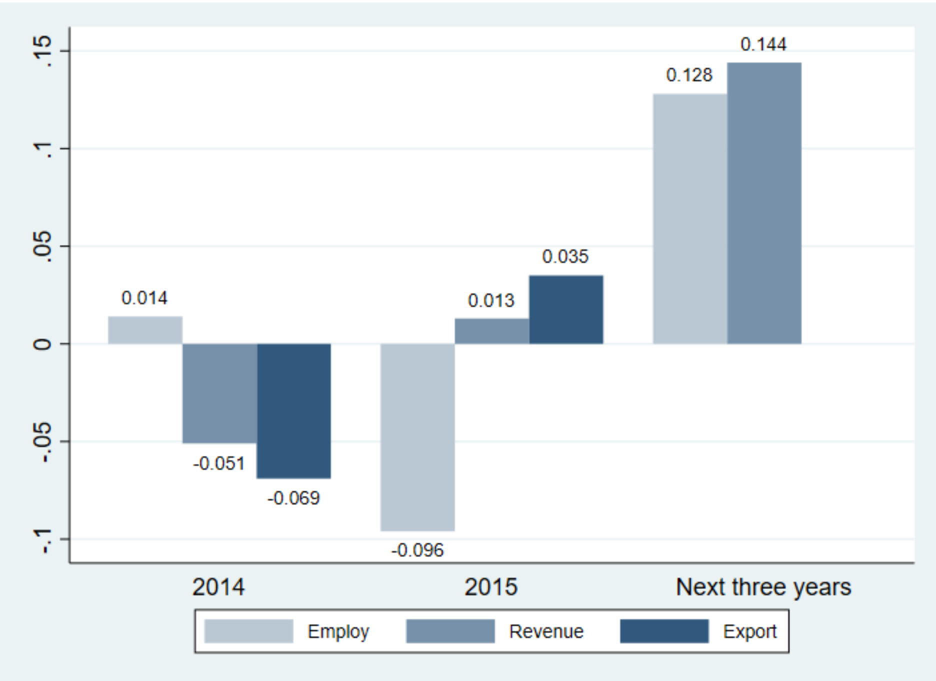
Appendix

Figure A1: Distribution and rejection rates of IUP funds by region



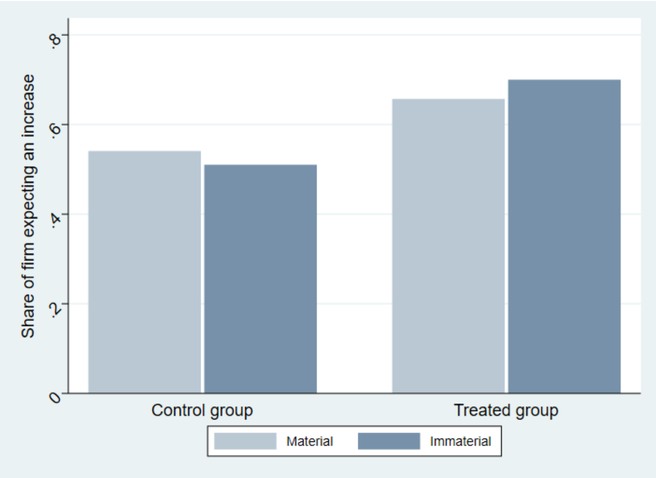
Note: Rates are weighed by total applicants per region. Total and average funds are in current millions of TND.
Source: Authors' calculations based on Bureau de Mise à Niveau.

Figure A2: Differences in reported increases in employment, profitability and export outcomes



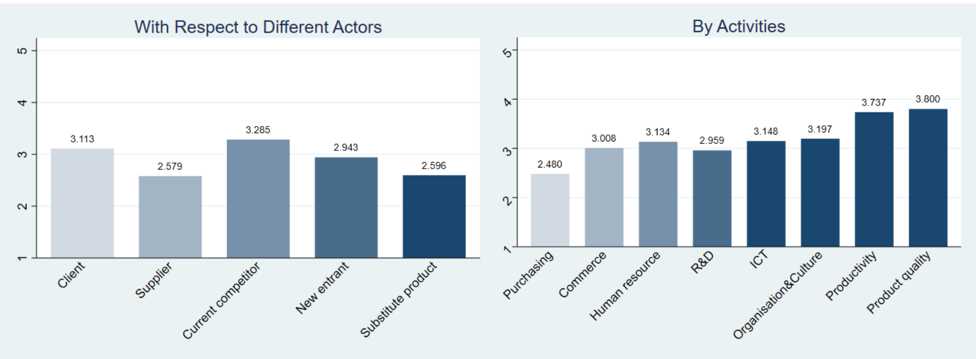
Note: The figure reports percentage of firms reporting any type of increase of each outcome. A positive percentage indicates higher reported outcomes for treated firms.
Source: Authors' calculations based on ITCEQ Survey.

Figure A3: Decomposition of expected investment



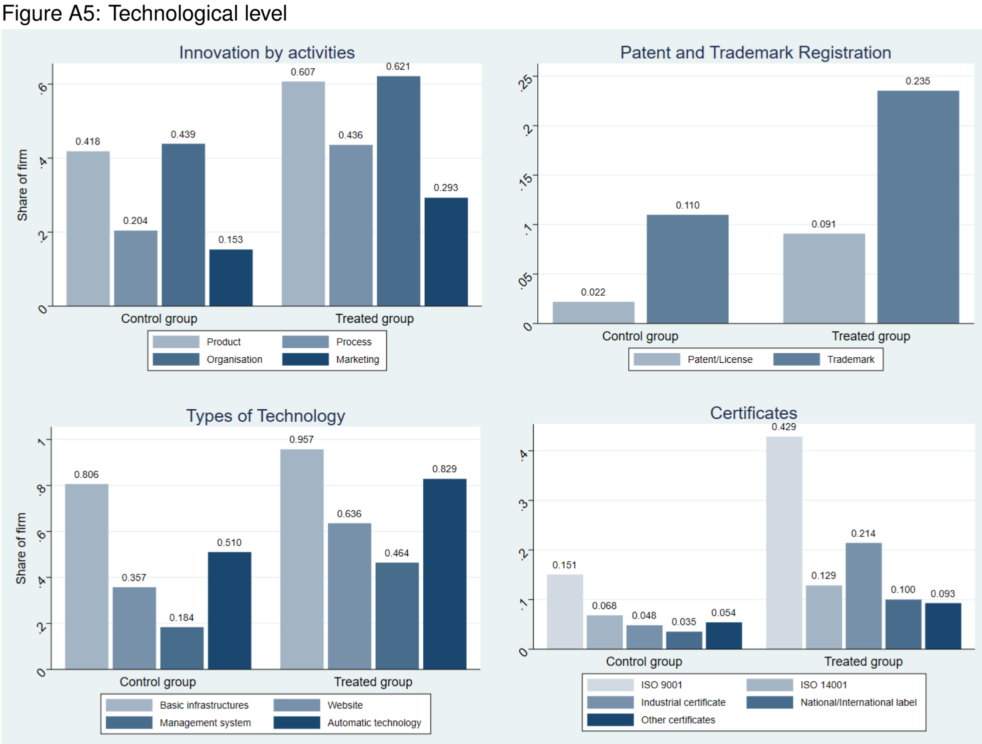
Source: Authors' calculations based on ITCEQ Survey.

Figure A4: Competitiveness



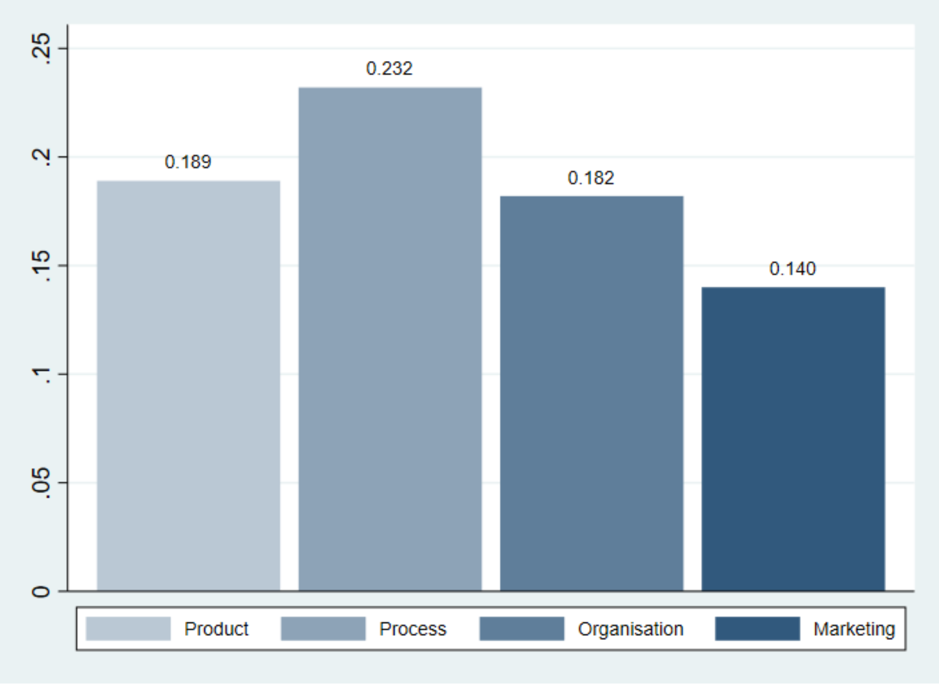
Source: Authors' calculations based on ITCEQ Survey.

Figure A5: Technological level



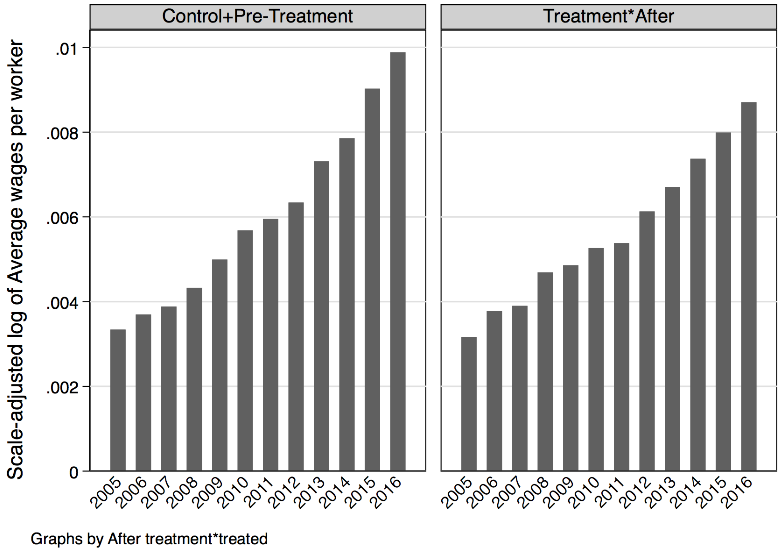
Source: Authors' calculations based on ITCEQ Survey.

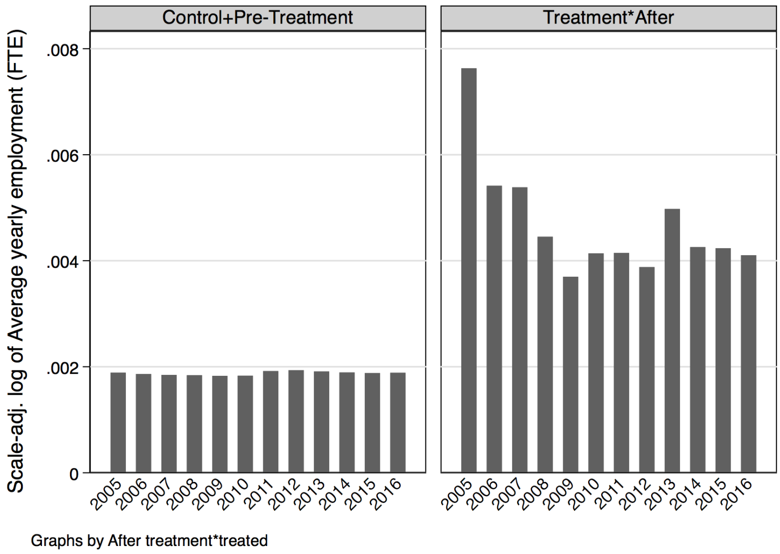
Figure A6: Innovation



Source: Authors' calculations based on ITCEQ Survey.

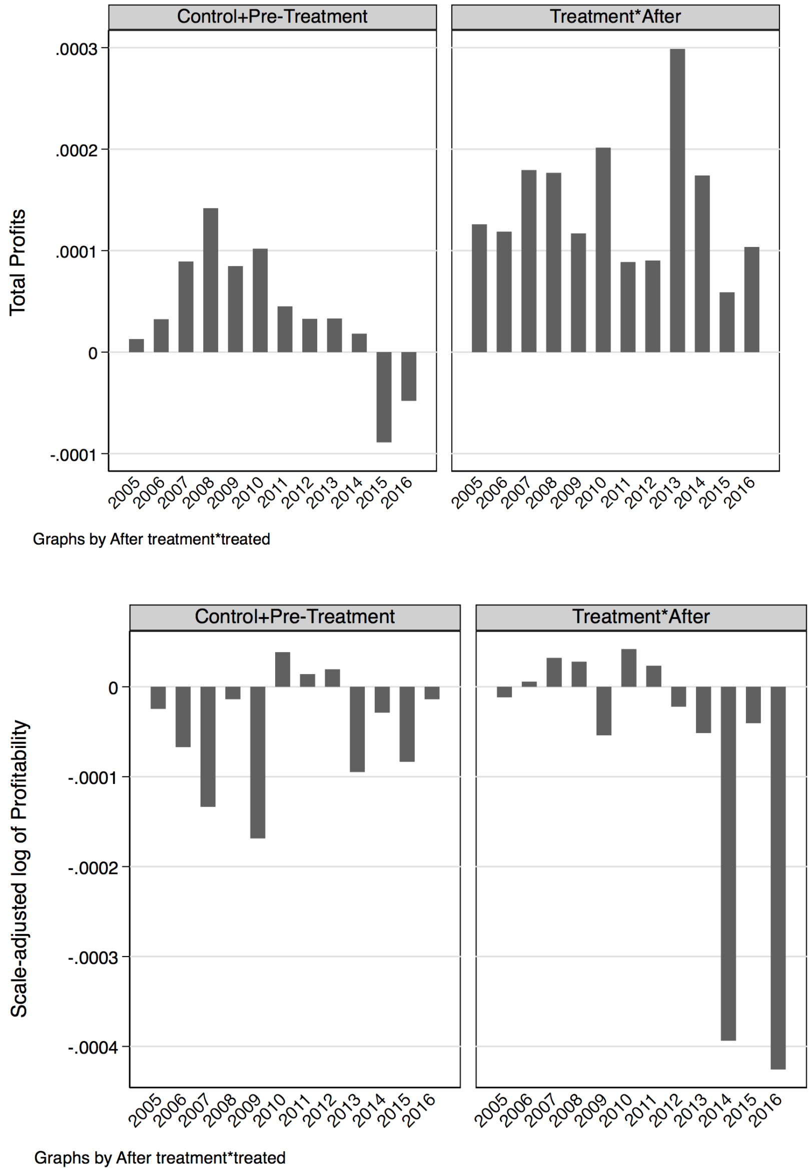
Figure A7: Wages and employment, by treatment status (after)





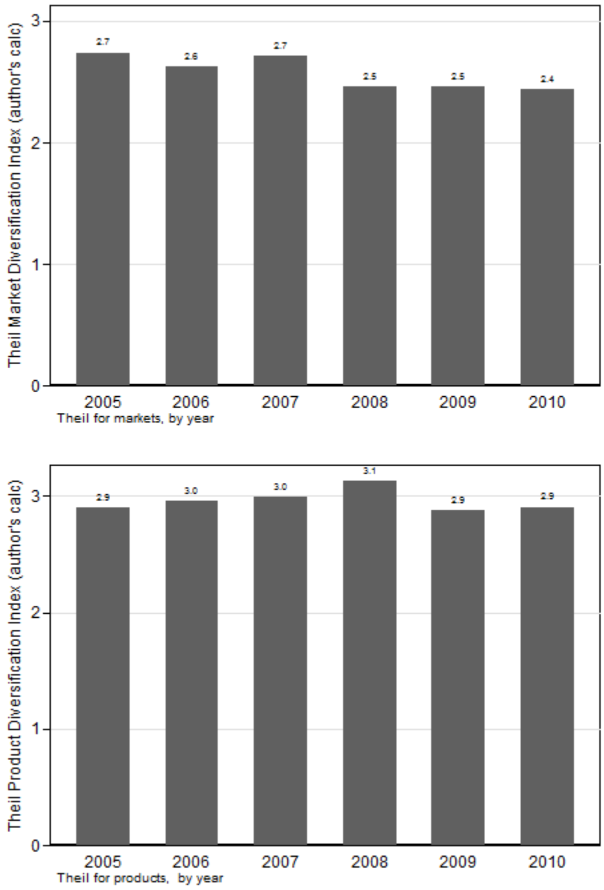
Source: Authors' calculations based on RNE.

Figure A8: Profits and profitability, by treatment status (after)



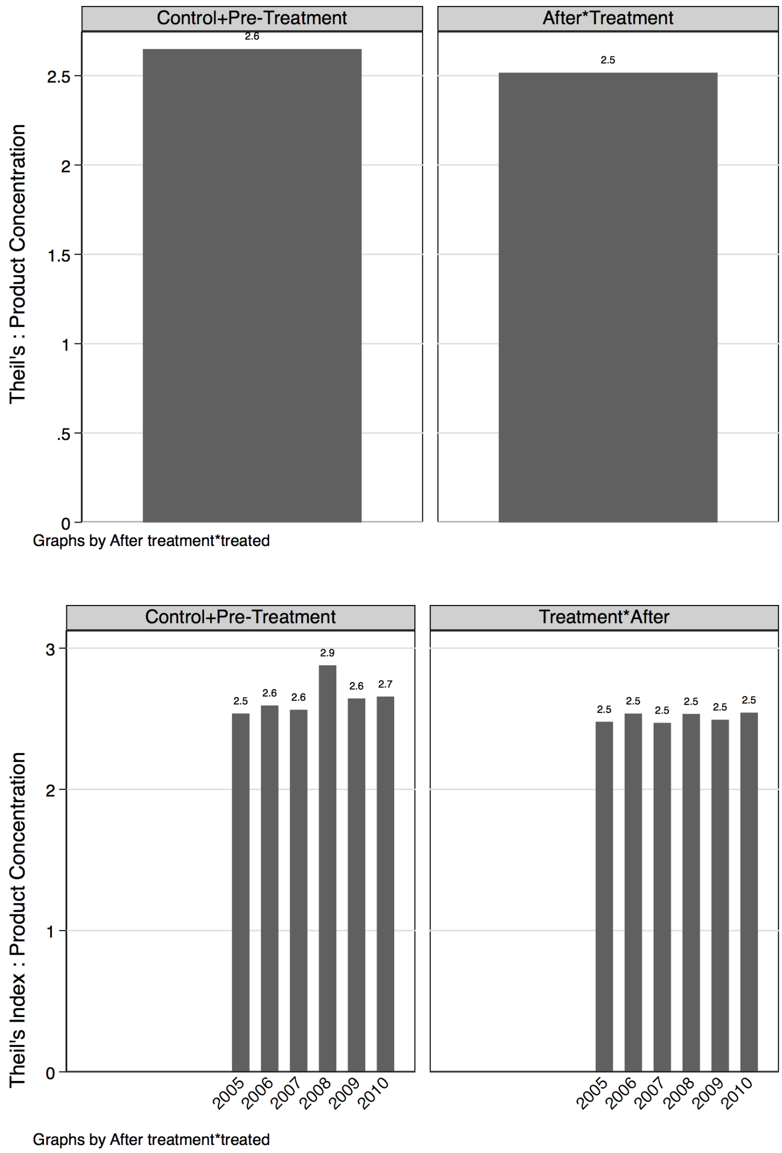
Source: Authors' calculations based on RNE.

Figure A9: Thiel's generalized entropy index for market and product concentration



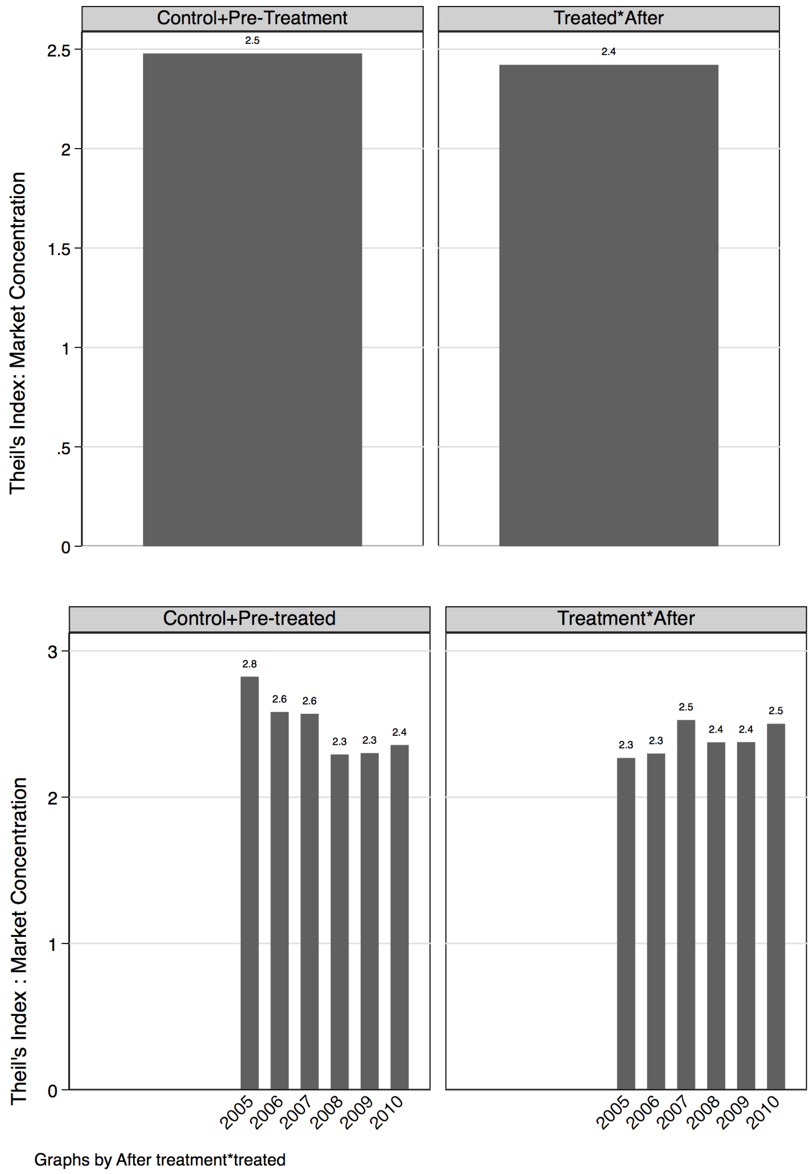
Source: Authors' calculations based on RNE.

Figure A10: Thiel's generalized entropy index for product concentration



Source: Authors' calculations based on RNE.

Figure A11: Thiel's generalized entropy index for product concentration



Source: Authors' calculations based on RNE.

Table A1: Description of main variables

Variable	Description
Average employment	The average yearly number of persons employed in full-time equivalent.
Net job creation	Total yearly jobs creation minus jobs destruction.
Wages (per worker)	Total wages and benefits paid by firms divided by number of employees.
Sales	Total turnover reported.
Sales per worker	Total turnover reported divided by total employed
Exports	The value of yearly exports
Profits	Total value of profits reported.
Profits per worker	The value of total profits divided by employees. Also referred to as profitability.
Value per unit	The total value of exports per unit exported. Only available for 2005–10.
Number of products	Number of exported products. Only available for 2005–10.
Number of markets	Number of exported markets. Only available for 2005–10.
Size group	Categories of firms by size groups. This varies between the tables as indicated.
Restrictiveness	Ordinal variable capturing how restrictive foreign direct investment rules are within the firm's sector (Rijkers et al. 2017).
Firm origin	A dummy variable for whether a firm is foreign or national.
Firm type	A variable for whether the firm is public or private.
Age	The age of firm from registration date.
Exporter status	A variable measuring whether a firm is an export-only form, partially export-oriented, or completely national.
Distance to ports	A variable capturing the distance from any of the nearest two ports to the city centre where the firm is located.

Source: Authors' compilation based on RNE.

Table A2: Sample *t*-test for difference between treatment and control group

	Treated	Control
Firm survival	0.9911	0.9845
Log of average employment	0.0084	0.0070
Log of net job creation	-0.0003	-0.0003
Log of wages (per worker)	0.0046	0.0059
Log of sales	0.0007	0.0015
Log of sales per worker	0.0001	0.0001
Log of export values	0.0013	0.0018
Log of profits per worker	0.0000	0.0000

Source: Authors' calculations based on RNE.

Table A3: Indicators of market concentration

Gini, Theil's entropy index, and Thiel's decomposition by treatment				
	GINI	GE(0)	GE(1)	GE(2)
2005	0.94533	4.35877	2.81526	16.16555
2006	0.93121	4.15509	2.61318	14.26525
2007	0.93743	4.43142	2.68034	15.1502
2008	0.92082	4.29535	2.40485	11.13297
2009	0.92128	4.08576	2.43914	11.96958
2010	0.92602	4.26168	2.53751	13.98232
	Between GE(1)	Within GE(1)	Between GE(2)	Within GE(2)
2005	0.05812	2.75714	0.0496168	16.11593
2006	0.06834	2.54484	0.0587316	14.20652
2007	0.1156	2.56474	0.0973379	15.05287
2008	0.10392	2.30093	0.08647	11.0465
2009	0.12832	2.31081	0.1092649	11.86031
2010	0.16404	2.37347	0.1377527	13.84457
	Control GE (1)	Treated GE (1)	Control GE (2)	Treated GE (2)
2005	2.823694	2.267654	15.12076	6.041585
2006	2.581956	2.298398	13.05273	6.724458
2007	2.569899	2.527124	12.75224	8.651509
2008	2.291784	2.374496	9.4886	7.440495
2009	2.301099	2.376101	9.80148	8.054179
2010	2.356615	2.501273	10.89955	9.322504

Source: Authors' calculations based on RNE.

Table A4: Indicators of product concentration

Gini, Theil's entropy index, and Theil's decomposition by treatment				
	GINI	GE(0)	GE(1)	GE(2)
2005	0.91884	3.57383	2.53157	16.72343
2006	0.92318	3.64043	2.58605	16.81002
2007	0.92229	3.66823	2.55396	15.28146
2008	0.93196	3.82093	2.84752	25.48601
2009	0.92217	3.68656	2.62719	18.51141
2010	0.92532	3.79645	2.65414	19.10999
	Between GE(1)	Within GE(1)	Between GE(2)	Within GE(2)
2005	0.0018	2.52978	0.00189	16.72154
2006	0.00002	2.58604	0.00002	16.81
2007	0.00195	2.55201	0.00186	15.2796
2008	0.00713	2.84039	0.00656	25.47945
2009	0.00287	2.62418	0.00302	18.50855
2010	0.01018	2.64396	0.00928	19.10071
	Control GE (1)	Treated GE (1)	Control GE (2)	Treated GE (2)
2005	2.53672	2.47872	17.42568	11.98901
2006	2.59353	2.53629	16.95802	15.83962
2007	2.56319	2.47047	15.44652	13.2552
2008	2.87848	2.53394	26.10005	14.15124
2009	2.64373	2.49276	18.86863	14.48558
2010	2.65729	2.54283	19.02241	15.63286

Source: Authors' calculations based on RNE.