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Evaluating the Impact of COVID-19 on Credit Rationing for Tunisian SMEs: A Conditional Difference-in-Differences Analysis

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Abstract: The COVID-19 pandemic has had a substantial impact on the global economy, particularly small and medium-sized enterprises (SMEs). This empirical study examines the effects of the pandemic on credit rationing for SMEs in Tunisia using a panel firm-level dataset spanning from 2014 to 2020. To analyze these effects, we employ the conditional difference-in-differences (CDiD) approach, which extends the commonly used difference-in-differences evaluation method. Our findings indicate that despite government support measures for SMEs, the COVID-19 pandemic has led to increased rates of credit rationing. We further explore heterogeneity in these effects based on criteria like corporate indebtedness and investment levels, identifying the most affected categories. Our results highlight that SMEs heavily reliant on suppliers, those with significant reliance on the banking system, and low financial resilience encounter more severe credit rationing compared to other groups. Additionally, credit rationing is more pronounced in the secondary sector compared to the tertiary sector.

Key words: Credit rationing, COVID-19 pandemic, Impact evaluation, Non-parametric matching, Conditional difference –in –differences, Tunisia

JEL classification: C14, C23, H43, G21, I10

1. Introduction

The COVID-19 pandemic has unleashed a severe impact on the global economy, prompting the implementation of broad-reaching measures such as lockdowns, travel restrictions, and corporate closures to contain the spread of the virus. Consequently, these policies have caused substantial disruptions to supply chains, a decline in consumer spending, and widespread job losses. Amidst this turmoil, small and medium-sized enterprises (SMEs) have emerged as one of the hardest-hit segments. The current crisis has inflicted significant damage upon SMEs, as they grapple with limited financial resources and face formidable obstacles in accessing funding, making it exceedingly challenging for them to navigate the prevailing economic downturn. Consequently, many SMEs have been compelled to make difficult choices, including employee layoffs and even permanent closures.

SMEs are pivotal in driving profit, job creation, livelihood development, innovation, and social stability (Baumol, 2009; Servon, 1999). Globally, they constitute approximately 90% of all businesses (ITC, 2021). However, financing obstacles impede operational efficiency and hinder the growth of enterprises (Beck and Demirguc-Kun, 2006, Beck et al., 2006; Beck, Demirgüç-Kunt and Maksimovic, 2004), and credit rationing appears as a major source of financing obstacles for SMEs (Gou Q et al., 2014).

Credit rationing stems primarily from the information asymmetry between lenders and borrowers (Adair and Adaskou, 2020). In this context, both banks and SMEs possess private information that is disregarded by the opposing party. SMEs hold private information regarding the characteristics of their projects, including risks and profitability, which banks fail to consider. Conversely, banks possess private information about their evaluation techniques and methods, which remains undisclosed to SMEs. Furthermore, banks and SMEs do not share a common objective, with banks prioritizing the borrower's repayment capacity while SMEs strive to maximize profitability. The presence of information asymmetry and/or a lack of alignment in goals give rise to a conflict of interest between the two parties.

Credit rationing can manifest in three forms: pure credit rationing, size-rationing, and self-rationing. Pure credit rationing involves denying credit to certain applicants while others with similar characteristics are approved. In this situation, even willing borrowers offering higher interest rates are unable to obtain loans, and despite sufficient loan supply, certain borrowers remain unable to secure loans at any interest rate (Stiglitz and Weiss, 1981). Size-rationing refers to granting smaller loan amounts than requested. Self-rationing happens when firms refrain from applying for loans due to perceived non-approval.

In this study, we assess the impact of the COVID-19 pandemic on credit rationing among Tunisian SMEs, focusing on size-rationing as we cannot verify the existence of the theoretical pure credit rationing because the respondent enterprises are not observationally identical; and we lack data on self-rationing because we lack data on firms that do not apply for a loan even though they needed to. Our empirical investigation draws on firm-level data from 626 Tunisian SMEs spanning from 2014 to 2020. To evaluate the pandemic effect, we use the semiparametric conditional difference-in-differences estimator (CDiD) (Heckman, Ichimura and Todd, 1998). In the first stage, we use propensity score matching and then use the conventional difference-in-differences (DiD) method to estimate average effects of treatment on the treated (ATT). In the analysis we implement kernel matching to match treated individuals to no-treated individuals in order to account for selection on observables. A conditional difference-in-differences estimator is used to control for time invariant unobservable characteristics. Our inference uses a bootstrap approach that accounts for the estimation error in the propensity score.

Our paper involves two further methodological innovations with respect to the current literature on credit rationing: Firstly, unlike previous research that relied on a binary indicator of credit rationing (whether a SME has been rationed or not), we adopt a different approach by incorporating the demand and supply for credit to calculate the rationing rate, providing a continuous measure of rationing. Secondly, this study stands as the first to assess the effects of COVID-19 on credit rationing rates using a Conditional difference-in-differences (CDiD) approach.

The article is structured as follows: Section 2 provides a review of the relevant literature on the impact of COVID-19 on small and medium-sized enterprises (SMEs) and the banking system. In Section 3, we outline the treatment approach and propose a methodology for measuring the outcome variable. Section 4 describes the data used in the study and discusses the empirical strategy applied. We provide detailed information on the data and present descriptive statistics. Additionally, we develop the microeconomic evaluation approach and explain its implementation. Section 5 discusses the empirical findings of the evaluation. Finally, we draw conclusions based on the overall analysis in Section 6. The numerous tables and figures are included in the appendix.

2. Previous literature on the impact of COVID-19 on SMEs and the banking system

The COVID-19 pandemic has had serious economic implications, affecting multiple sectors and economies worldwide, with the banking system and SMEs bearing the brunt (Levashenko and Koval, 2020; Cepel et al., 2020; Kraima and Boudabous, 2022), along with other sectors.

Small and medium-sized enterprises (SMEs) were particularly vulnerable to the risks posed by the pandemic. Many governments developed various strategies to provide both financial and non-financial assistance to mitigate these hazards. These measures included direct financing, financial guarantees, tax reliefs, and low-interest loans for operating capital.

According to data from the weekly U.S. Census Small Business Pulse Survey, around half of businesses had a major negative impact from the pandemic, with only 15-20% having enough cash reserves to support three months of operations (Bohn et al., 2020; U.S. Census Bureau, 2020). According to the research of Bartik et al. (2020), 43% of businesses experienced temporary closures and 40% drop in employment, particularly in the retail sector, emphasizing the financial vulnerability that small businesses suffer, with the majority having less than one month's worth of cash reserves.

According to Levashenko and Koval (2020), and Chen et al. (2020), SMEs are more vulnerable to pandemic-related hazards than large corporations. The authors emphasize the importance of implementing financial mechanisms such as direct financing and tax stimuli to support SMEs. They also underline the importance of non-financial support systems. Several research projects have been conducted to examine the impact of COVID-19 on various business sectors. Hudson (2020a) specifically points out that the travel and hospitality industries have been among the hardest hit by the crisis.

In their study involving a sample of 140 Tunisian SMEs, Kraima and Boudabous (2022) found a positive impact of indebtedness on the health crisis caused by COVID-19. SMEs are currently having difficulty servicing their loans, resulting in delays in timely repayments. Furthermore, the research findings indicate that the COVID-19 health crisis has given rise to additional obstacles that hinder the functioning of SMEs. These findings emphasize the relevance of government and partner efforts in assisting SMEs with cash flow issues

Based on a joint report by the African Development Bank (AfDB) and the International Labor Organization (ILO) released in February 2022, the COVID-19 pandemic had a significant impact on SMEs. The report reveals that 65% of SMEs experienced a substantial decline in turnover since the global outbreak in 2019. Furthermore, the study found that one out of every five businesses failed, and one out of every six downsized their employment.

During the COVID-19 pandemic, the banking system, which is a vital component of the economy, suffered severe hurdles, representing one of the most serious challenges faced by the financial services industry in over a century. Banks have faced a decline in demand, reduced incomes, and production disruptions, which have adversely affected their operations. The situation is compounded by staff shortages, insufficient digital readiness, and strain on existing infrastructure as firms deal with the effects of the pandemic on financial services.

In response, central banks worldwide have reacted proactively, intervening to stabilize markets and displaying their commitment to using all available measures (Korzeb and Niedzióka, 2020; DemirgucKunt and Ortega, 2020). DemirgucKunt and Ortega (2020) underscored the substantial strain placed on banking systems globally due to the global crisis and the expected countercyclical lending role of banks, which have varied implications depending on the pre-crisis characteristics and vulnerabilities of each system. Korzeb and Niedzióka (2020) found that major banks in Poland demonstrated resilience during the pandemic-induced crisis. However, the crisis is expected to impact the financial system by increasing the number of non-performing loans and requiring write-offs.

3. Treatment description in Tunisia

The first case of COVID-19 in Tunisia was confirmed in March 2020. COVID-19 had a significant economic impact in Tunisia. The country is heavily reliant on sectors such as tourism, manufacturing, and services, all of which have been severely harmed by travel restrictions and reduced consumer spending. This has resulted in job losses, lower earnings and economic difficulties for businesses, particularly SMEs (Kraima and Boudabous, 2022).

The National Institute of Statistics in Tunisia (NIS), in collaboration with the International Finance Corporation (IFC), launched a survey in Tunisia, revealing that despite a slight recovery after decontamination, 82.3% of companies reported a decline in turnover in July compared to 88.8% in April. Consequently, we chose turnover as the basis for assessing the impact of COVID-19 on companies. To establish a clear criterion, we relied on a governmental decree[1], which stipulates that for a company to be considered affected by COVID-19, their turnover must have decreased by at least 25% in March 2020 compared to March 2019, or by 40% in April 2020 compared to April 2019. This threshold aligns with the peak of the pandemic, and since our data is annual, it is reasonable to consider a 40% decrease as the minimum rate to determine the COVID-19 effect.

Using this information, we can establish a binary treatment strategy for COVID-19, where we define the variable "COVID" to indicate whether a company has been affected by COVID-19. Specifically, a company is classified as "COVID-affected" if its turnover fell by at least 40%, and as "COVID-unaffected" if its turnover decreased by less than 40%.

The objective of this study is to examine the impact of COVID-19 on the credit rationing rate, which serves as the outcome variable. Since the required data for the credit rationing rate is unavailable, it needs to be estimated. To determine the rationing rate for each firm i in year t, we first compute the total credit requested and total credit offered. Because both demand and supply of credit are not directly observable, we adopt a methodology similar to Steijvers (2008), Adair and Fhima (2013), and Adair and Adaskou (2020) that involves a disequilibrium model. This strategy effectively addresses the limitations of previous techniques like proxies or surveys (Kremp and Sevestre, 2013). If a company's requested credit amount exceeds what the bank is willing to provide, it is classified as partially or totally rationed (Adair and Fhima, 2013).

The disequilibrium model involves three simultaneous equations[2]. These equations cover the credit demand, credit supply, and the equilibrium equation. The demand for credit is driven by several firm-specific characteristics that indicate its financing needs, including reliance on suppliers, sales reflecting activity levels, tangible and intangible assets, as well as internal resources such as cash flows and returns on assets. On the other hand, the supply of credit is influenced by the firm's ability to repay its debts, which is determined by factors such as firm size, age, and sector of activity that reflect the associated risk. The presence of collateral also affects the availability of credit. These combined factors shape lenders' willingness to extend credit to the firm. The third equation is an equilibrium equation that equates credit demand to credit supply.

Following the estimation of the model, we can obtain the fitted values for both the amount of credit requested and the amount of credit offered. These fitted values represent the predicted values of a dependent variable based on the estimated parameters of the regression model. The rationing rate, based on Steijvers' (2008) definition of rationed firms where the credit value requested exceeds the available supply, can be calculated using the following formula:

$$Credit\ rationing\ rate = \frac{Credit\ amount\ requested - Credit\ amount\ granted}{Credit\ amount\ requested}$$

4. Data and empirical strategy

4.1 Data and descriptive statistics

In our analysis we focus on the impact of COVID pandemic (the treatment) on credit rationing rate (the outcome variable) in 2020. Our empirical analysis draws on seven SMEs-level datasets derived from the Central Balance Sheet Data of the Central Bank of Tunisia (BCT). The sample ranged from 2014 to 2020 and includes 626 unlisted, privately owned enterprises that were not part of a group of firms (independent). According to the updated definition of SMEs, the firms in our sample have net fixed assets larger than or equal to 10 million Tunisian dinars.

Our choice of this sample of SMEs was driven by several factors. Firstly, SMEs play a crucial role in the Tunisian economy, contributing significantly to employment generation, innovation, and overall economic growth. According a European Bank for Reconstruction and Development (EBRD) report from 2020, there are over 80,000 SMEs in Tunisia, accounting for over 40% of the GDP and employing more than half of the population. Moreover, these SMEs account for around 90% of the private sector (Bellakhal and Mouelhi, 2020).

Second, compared to larger corporations, SMEs often encounter distinctive challenges such as limited access to financial resources and rely heavily on external financing, such as bank loans, to sustain their operations and growth. As a result, they are more susceptible to changes in credit conditions and are particularly sensitive to disruptions in the financial system.

Our data set includes information on both sales and financial resources. We also have information on the number of years the company has been in business. We have data on the ability of firms to deal with anticipated financial restrictions, as well as the degree of reliance on suppliers due to outstanding loan obligations. We also have information on loan amounts and collateral secured through the banking system, as well as companies' ability to repay their short-term obligations.

Table 1 (in the Appendix) displays the variable definitions as well as descriptive statistics measured at the occurrence of the COVID pandemic in 2020 separately for treated and non-treated units. Treated units are on average, smaller and older firms with a comparatively lower capacity to handle anticipated financial constraints. These units demonstrate reduced reliance on suppliers due to outstanding debt obligations, have received more loans from the banking system, possess less collateral, exhibit a higher ability to fulfill short-term commitments (such as loans), display a significantly higher interest coverage ratio indicating a lower risk of loan default, and are more concentrated in the secondary and tertiary sectors.

<Insert Table 1 here>

We also observe a raw difference credit rationing rates of around 0.859% between treated and non-treated units. These are merely preliminary descriptive statistics, and the discrepancy could be explained by both the treatment and differences in key variables. Indeed, estimating causal effects accurately is typically challenging when experimental designs are not feasible due to the influences of covariates or confounders from selection bias (Rosenbaum and Rubin, 1983). We will return to this point later on in subsequent sections when discussing the identification strategy, and describing causal effects of the treatment.

4.2 Evaluation Approach

In order to estimate causal effects, we base our empirical analysis upon the potential outcome approach to causality, also known as the Roy (1951) - Rubin (1974) model (refer to the survey by Heckman, LaLonde, and Smith, 1999). We will focus on the most prominent evaluation parameter, which is the average treatment effect on the treated[3] (ATT) in the binary treatment case. The ATT is given by the following equation:

$$E(Y^1|D=1)=E(Y^0|D=1)$$

Where Y^1 is the treatment outcome (individual receives treatment, $D=1$) and Y^0 is the non-treatment outcome (individual does not receive treatment, $D=0$) and D denotes the treatment dummy. Our outcome variable of interest is a continuous variable, the credit rationing rate. The observed outcome Y for any individual i can be written as: $Y=DY^1+(1-D)Y^0$.

The treatment effect for each individual i is then defined as the difference between her potential outcomes: $\beta_i=Y_i^1-Y_i^0$. Since it is impossible to observe both potential outcomes for the same individual at the same time, the fundamental evaluation problem arises. The evaluation problem consists of estimating the counterfactual outcome in the non-treatment situation, $E(Y^0|D=1)$, which is not observed for the treated individuals ($D=1$). Thus, identifying assumptions are needed to estimate $E(Y^0|D=1)$ based on the outcomes for non-treated individuals ($D=0$).

We implement the semiparametric Conditional difference-in-differences (CDiD) approach to control for unobserved time invariant selection effects. Conditional DID was first introduced by Heckman, Ichimura, Smith and Todd (1998), where they use a hybrid model combining a difference-in-differences (DID) estimation and matching. In the first stage, they apply propensity score matching[4] to match treated individuals to non-treated individuals in order to account for selection on observables[5]. The difference-in-difference (DiD) approach[6] is then used to estimate the average effects of treatment on the treated (ATT). The basic idea of a conditional difference-in-differences estimator that controls for time invariant unobservable characteristics is to compare outcome changes conditional on matched samples rather than whole samples of treated and non-treated units. The CDiD method makes DiD more robust by incorporating control variables to match treatment and control group units, thus reducing bias (Fredriksson and De Oliveira, 2019). It effectively addresses issues arising from various identifying assumptions in the conventional DiD method. This approach specifically tackles the potential violation of the crucial parallel trend assumption, while also ensuring that no compositional changes take place in both the treated and control groups (Dette and Schumann, 2020; Fredriksson and De Oliveira, 2019).

The CDiD relies on several identifying assumptions to assess casual effects. Some of these assumptions are standard assumptions that apply to all micro-econometric causal studies and are not specific to matching or DID. One of these assumptions is that any general equilibrium effects must be ruled out, which means that treatment participation by one individual cannot influence the outcomes of other individuals. This assumption is known as the stable-unit-treatment-value-assumption (SUTVA), and it states that there should be no spillover effects between the treatment and control groups, as the treatment effect would be lost (Duflo, Glennerster, and Kremer, 2008).

The next assumption concerns the conditioning variables X because the main behavioral assumptions are supposed to hold conditional on some covariates X. To make sure that this conditioning does not destroy identification, it is assumed that the control variables should be exogenous, unaffected by the treatment. This assumption is called EXOG (exogeneity). This assumption will be discussed further in the next section after discussing the control variables selection. Additionally, we should check whether there are no changes in the composition of the two groups over time. This means that the characteristics and makeup of both groups remain consistent in the pretreatment and post-treatment periods. This is critical because compositional changes could potentially result in an underestimate of the treatment impact. We can acquire more precise and trustworthy estimations of the treatment's impact if the groups are similar before and after the treatment. These two assumptions will be discussed further in the next section after discussing the control variables selection.

4.2.1 Selection on Observables and Matching

In the evaluation problem taking the mean outcome of non-treated individuals as an approximation of the counterfactual is not recommended, because treated and non-treated individuals generally differ even in the absence of treatment. This is known as selection bias, and a good illustration comes from the labor market studies, where motivated individuals have a higher probability of attending a training programme and have also of landing a job. One proposed solution to the selection problem is the matching strategy. It comes from the statistical literature and is closely related to the experimental context. Propensity score matching method aims to mimic the random assignment mechanism by choosing units as similar as possible to the participants to constitute the comparison group. Its core idea is to identify in a large group of untreated individuals, those who are similar to the treated individuals in all relevant pre-treatment characteristics X. Following that, differences in outcomes of this well selected and hence adequate control group and of treated individuals can be attributed to the programme. As the number of characteristics determining selection increases it becomes more and more difficult to find comparable individuals (known as the “curse of dimensionality”), Rosenbaum and Rubin (1983) propose a single index for matching (propensity score matching (PSM)). This index which reflects the probability of receiving the treatment, may produce consistent estimates of the treatment effect in the same way as matching on all covariates X. This single index summarizes all relevant information from the covariates X. Matching on this index is similar to matching on the covariates X, in that the distribution of X should be the same for treated and non-treated individuals for any given value of the index.

4.2.1.1 Covariates selection

The matching strategy builds on the Conditional Mean Independence Assumption (CIA), which states that there are no systematic differences between treated and non-treated individuals in terms of unobserved characteristics that may influence both the treatment and the outcomes[6]. As a result, the CIA asserts that there is no systematic difference in potential non-treatment outcomes between the treated and control groups conditional on the observed covariates X.

$$E(Y^0|D=1,X) = E(Y^0|D=0,X)$$

Thus, to estimate the expected non-treatment outcome for treated individuals with observable characteristics X, it sufficient to take the average outcome for untreated individuals with the same characteristics *X*.

What is crucial for implementing matching is the selection of relevant variables X that genuinely satisfy this condition. According to Heckman, Ichimura and Todd (1997) omitting important variables can seriously increase bias in resulting estimates. Only variables that influence both the decision to participate in the treatment and the outcome variable should be included. It should also be clear that only variables that are unaffected by the treatment (or the anticipation of it) should be included in the model. Heckman, LaLonde, and Smith (1999) further state that data for treated and untreated individuals should come from the same data sources. The more accurate and informative the data, the easier it is to justify the CIA and the matching procedure. Some randomness is also required to ensure that people with identical characteristics can be observed in both states (Heckman, Ichimura, Smith and Todd, 1998).

The CIA is plainly a very strong assumption, and the matching estimator application is critically dependent on its plausibility. According to Blundell et al. (2005), the plausibility of such an assumption should always be evaluated case by case. Hence, economic theory, prior research experience, and information about the institutional environment should lead the researcher in specifying the model (Sianesi, 2004; Smith and Todd, 2005).

In our study we restrict analysis to two variables that satisfy the aforementioned conditions: *TRADECREDIT*, and *DEBTS*. The information for both treated and untreated units is derived from the same data source. Moreover, we ensure that the control variables satisfy the exogeneity assumption (EXOG) and confirm the absence of compositional changes in the treatment and control groups over time, as they are not influenced by the treatment (Fredriksson and De Oliveira, 2019; Aragon and Rud, 2013). A classic example of compositional changes arising from a widely researched healthcare reform in Massachusetts in 2006, aimed at ensuring healthcare coverage for almost all residents (Long, Yemane, and Stockley, 2010), would be if individuals with poor health move to Massachusetts (from a control state to the treatment state). The health reform impact would then likely be underestimated (Fredriksson and De Oliveira, 2019). To assess these assumptions, we use a regression model to test these assumptions, with each of the two covariates serving as the dependent variable in an expression 2-style regression. Any significant effect (the interaction term between treatment and time dummies) would indicate a potentially troublesome (Aragon and Rud, 2013). Estimation results are displayed by Table 2 in the Appendix. Because we are operating within the framework of a natural experiment (an experiment over which the researcher has no control), some randomness is guaranteed in our study. Furthermore, the selected variables satisfy the balancing condition, which is required for the matching technique to be a viable method. Based on all arguments presented above, we argue that the CIA holds in our application.

<Insert Table 2 here>

4.2.1.2 Choosing a Matching Algorithm

Propensity score matching (PSM) estimators differ not just in how the neighborhood is defined for each treated individual and whether the common support problem is handled, but also in the weights assigned to these neighbors. Matching estimators differ in terms of the weights assigned to comparison group members. The most widely used method in the literature is nearest neighbor matching, which uses the outcome for the closest control unit as the comparison level for the treated unit (Lechner, 1998; Heckman, LaLonde, and Smith, 1999). In this case, the weight is 1 for the nearest neighbor to the concerned treated unit and 0 for all other non-treated units that are not the same as the concerned treated unit.

However, there are alternative matching estimators that incorporate weights different from 1. In this research, we use a nonparametric kernel regression to estimate the expected non-treatment outcome of treated units with specific characteristics, as described by Pagan and Ullah (1999). This involves specifying the weight function based on a kernel function with the distance in terms of individual characteristics as its parameter. Kernel matching has certain potential advantages over the nearest neighbor matching. The asymptotic properties of kernel-based approaches are simple to investigate and it has been shown that bootstrapping[9] provides a consistent estimator of the sampling variability of the estimator (Heckman, Ichimura, Smith and Todd, 1998; Ichimura and Linton, 2001).

4.2.1.3 Kernel matching

Unlike other matching algorithms that use only a few observations from the comparison group to construct the counterfactual outcome of a treated individual, Kernel matching (KM) is a non-parametric matching estimator that uses weighted averages of all individuals in the control group to construct the counterfactual outcome. As a result, one major advantage of this strategy is the lower variance obtained by using more information. A drawback of this strategy is that possibly observations are used that are bad matches. As a result, appropriate implementation of the common support condition is crucial for KM. This condition asserts that matching is only feasible when there are individuals with similar propensity scores in both treated and control groups. The criterion implies eliminating treated units that have no units with similar PSM in the control group.

When applying KM, the kernel function and bandwidth parameter or smoothing constant (a positive valued smoothing parameter that would typically tend to 0 when the number of samples tend to ∞) must be selected. There are various kernel functions that can be employed for density estimation, including Epanechikov, biweight, triangular, Gaussian, and rectangular kernels. However, the choice of kernel function for estimation appears to be rather unimportant in practice, as it has no effect on estimation accuracy (DiNardo and Tobias, 2001). Furthermore, the bias of density estimation using the kernel estimator is independent of sample size and only depends on the bandwidth parameter choice (Silverman, 1986).

What is considered as more important is the choice of the bandwidth parameter (Silverman, 1986; Pagan and Ullah, 1999), with the following trade-off arising: High bandwidth-values result in a smoother estimated density function, resulting in a better fit and a decreasing variance between the estimated and the true underlying density function. In contrast, a large bandwidth may smooth away underlying features, resulting in a biased estimate. Thus, the bandwidth choice involves a trade-off between a small variance and an unbiased estimate of the true density function.

Choosing an appropriate bandwidth for a kernel density estimator is of crucial importance, and the objective of the estimation may be an influential factor in the selection procedure. In many cases, it is sufficient to subjectively choose the smoothing parameter by looking at the density estimates produced by a range of bandwidths. One can begin with a large bandwidth, and gradually reduce the amount of smoothing until one gets a ”reasonable” density estimate. However, there are situations where several estimations are needed, and such an approach is impractical (Herawati et al., 2017).

Various methods have been proposed to select an optimal bandwidth for accurate estimation. These methods include the Scott (Nrd) bandwidth method, Silverman's Long-Tailed distribution (Silverman-LT), Silverman's rule of thumb (Nrd0) bandwidth method, Unbiased Cross Validation (UCV) bandwidth method, and Sheater-Jones (SJ) bandwidth method (Herawati et al., 2017). Each method determines the optimal bandwidth based on specific conditions, such as the data's distribution nature (normal, symmetric, unimodal, skewed, long-tailed), enabling precise estimation for different data characteristics.

When a large number of estimations are required as part of a larger global analysis, an automatic approach is required. Various methods have been proposed to select an optimal bandwidth for accurate estimation. These methods include the Scott (Nrd) bandwidth method, Silverman's Long-Tailed distribution (Silverman-LT) method, Silverman's rule of thumb (Nrd0) bandwidth method, Unbiased Cross Validation (UCV) bandwidth method, and Sheater-Jones (SJ) bandwidth method (Herawati et al., 2017). Each method determines the optimal bandwidth based on specific conditions, such as the data's distribution nature (normal, symmetric, unimodal, skewed, long-tailed), enabling precise estimation for different data characteristics.

Given that our study involves selection variables with highly skewed and long-tailed distributions, we have determined that the Silverman's Long-Tailed distribution (Silverman-LT) method is the most suitable for our data. Its formula (Rizzo, 2008) is given by:

$$h = 0.79(IQR)n^{-\frac{1}{5}}$$

Where, IQR is the inter quartile range (Q3 - Q1) and n is the sample size. The Silverman-LT bandwidth method would give the best density curve estimation among other methods. This implies that the density curve of the Silverman-LT bandwidth method would best approximate the real data distribution (PDF) curve.

4.2.2 Identification of causal effects with DID and conditional DiD

While the matching strategy addresses selection bias due to observed characteristics, selection bias generated by unobserved characteristics requires a different approach. We allow the credit rationing model's permanent unobserved effects to influence treatment selection. Unobserved characteristics, for example, could be due to differences in managerial abilities, leadership characteristics, employee motivation and satisfaction levels, informal social networks within an organization, including relationships, collaborations, and communication patterns, and so on.

The difference-in-differences (DiD) estimator can be applied when selection effects are additively separable and time invariant (Bergemann, 2005; Hoderlein et al., 2011). It is then possible to simply examine the before-after change in the outcome variable. The proper implementation of the DiD strategy relies on two key identifying assumptions: the 'Ashenfelter's dip and parallel trends assumption.

The Ashenfelter's dip[7] (Ashenfelter, 1978) or "fallacy of alignment" (Heckman, LaLonde, and Smith, 1999) refers to the phenomenon in which the anticipation of a treatment may lead to a temporary change in the behavior of the applicants. Anticipation effects encompass two components: the ex-ante effect and the ex-post effect (Malani and Reif, 2015). The ex-ante anticipation effect refers to the average effect on the pre-treatment outcome when a permanent treatment is implemented in the current period. The ex post anticipation effect, on the other hand, can be defined as the impact on the outcome at the time of treatment occurrence, based on individuals' expectations regarding whether the treatment will continue to occur in the future.

Examples of ex-ante anticipation effects causing behavioral changes are mostly described in the context of active labor market programs, where it is often observed, that shortly before participation in a labor market program the employment situation of the future participants deteriorates disproportionately; for instance unemployed people may lower their job search effort when they anticipate participation in a training program in the near future (Bergemann et al., 2009).

In our study, we argue that anticipatory effects could potentially lead to the emergence of Ashenfelter's Dip[8]. It is important to stress that our analysis is limited to ex-ante anticipatory effects because we only have data for the treatment period. One rationale for (ex-ante) anticipatory effects could be that small and medium-sized businesses adjust their demand for investment credits in anticipation of an economic downturn if the COVID epidemic strikes Tunisia soon, as has happened in previous COVID-affected countries. Likewise, banks may adjust the availability of these credits if they anticipate challenges in debt repayment by enterprises in certain sectors if Tunisia is hit by COVID in the near future.

One important issue in estimating models with anticipation effects is that researchers may not know how many periods in advance agents expect treatment. To address this, a common approach in empirical microeconomics literature is to estimate a "quasi-myopic" model, incorporating anticipatory terms for a finite number of periods (Malani and Reif, 2015; Mertens and Ravn, 2011; Autor et al. 2006; Ayers et al. 2005; Finkelstein, 2004; Acemoglu and Linn, 2004; Lueck and Michael, 2003).

Within the scope of our study, the COVID pandemic (the treatment) struck Tunisia in 2020, which implies that we should investigate these anticipation effects shortly before the treatment occurs, say in 2019. Indeed, the COVID pandemic strike is regarded as a natural experiment that first struck the planet in 2019, therefore if there are any probable anticipation effects that may affect individual behavior prior to treatment can only occur during this period and not before.

We estimate the average effect on pre-treatment credit rationing rates resulting from the implementation of COVID-19 in 2020. Our analysis reveals no evidence of Ashenfelter's Dip. Detailed results are displayed in Table 3 in the Appendix.

<Insert Table 3 here>

The next assumption is the "common trend" assumption, which is the key assumption of the DiD approach. It asserts that the differences in the expected potential non-treatment outcomes over time (conditional on X) are unrelated to whether people were in the treated or control group during the post-treatment period. It means that if the treated had not received the treatment, both subpopulations defined by $D = 1$ and $D = 0$ would have experienced identical time trends conditional on X. The common trend assumption gives the intuition of the identification of the treatment effect. As the non-treatment potential outcomes share the same trend for treated and untreated individuals, any deviation in the trend of the treated observed outcomes from the trend of the non-treated observed outcomes will be directly attributed to the effect of the treatment and not to differences in other characteristics of the treatment and control groups.
Because the treatment group is only observed as treated, the assumption is fundamentally untestable. One can lend support to the assumption, however, through the use of several periods of pre-treatment data, showing that the treatment and control groups exhibit a similar pattern in pre-treatment periods. If such is the case, the conclusion that the impact estimated comes from the treatment itself, and not from a combination of other sources (including those causing the different pre-trends), becomes more credible. A certain number of pre-treatment periods is highly desirable and certainly a recommended "best practice" in DiD studies.

In this analysis, we used six pretreatment data periods, and found that the treatment and control groups exhibit different patterns in two pretreatment periods 2014-2015 and 2018-2019, while trends in the remaining periods are parallel (this is depicted in Figure 1 of the Appendix). Dette and Schumann, 2020) emphasize that one strategy to addressing the failure of the identifying assumption of parallel trends is to use a conditional difference-in-differences (CDiD) approach based on matching methods on observable covariates (e.g. Heckman, Ichimura, and Todd, 1997, Heckman, Ichimura, Smith, and Todd, 1998 or Abadie, 2005). This approach uses pretreatment differences in the outcome variable after matching (the matched samples that are statistically similar in terms of their selected observed characteristics) to control for remaining unobservable differences. Indeed, in conditional DiD, the conditional independence assumption (CIA) for matching and the common trend assumption (CTA) for DID are replaced by the "conditional parallel trend assumption" (Callaway and Sant'Anna, 2019), which implies that unobservable individual characteristics must be invariant over time for units with the same observed characteristics, implying that the CIA assumption is relaxed.

<Insert Figure 1 here>

4.2.3 Estimation Procedure

Following the discussion of identification issues, we turn to the estimation of causal effects. We apply probit-estimation to estimate the propensity scores, which represent the predicted probability of receiving the treatment for each treated and non-treated unit. We perform nearest neighbour matching on the propensity score and impose a 1% caliper (where the caliper restricts matches to be sufficiently close) to ensure common support (Sianesi, 2004). According to the economic and empirical literature, in order to select the final specification, we must test several specifications with different sets of selected observable covariates and then choose the best one based on the matching quality, as well as on a variety of economic indicators such as variable significance and a lower value of pseudo-R^2.

To evaluate the matching quality, that is, whether the matching procedure balances the distribution of observable characteristics between treated and non-treated units, we use statistical tests. Table 4 in the Appendix displays the various quality measures. For a good match or balance we should look at the t-test testing the statistical significance of the difference between the two estimates of the treated and non-treated group. A good match implies that the estimates difference is not statistically significant at the standard 5% level, and the standardized percentage difference – or bias – between the means in both groups should be less than 5%. Furthermore, we should get an information on the similarity of variances in the treated and the comparison group (Caliendo and Künn, 2011).

Before matching, we can see in specification 1, that the selected observable variables *DEBTS* and *TRADECREDIT* have a mean that is significantly different between treated and non-treated at the 5% level or less, as well as a percentage bias higher than 5%. The matched sample, on the other hand, shows no significant differences, and the percentage bias for both variables is less than 5%. This indicates that matching was successful. Because a t-test provides no information regarding bias reduction, we also report the Median absolute standardized bias (MASB), which decreases from 11.3% before matching to 2.7% after matching. A MASB of less than 3% to 5%, according to Caliendo and Kopeinig (2008), generally suggests that matching was successful as the means and variances of all the matching variables are balanced. Overall, matching on the estimated propensity score balances the selected observable covariates in the matched samples very well (in fact better than the kernel versions we tested).

In specification 2, the selected variables *CASHFLOW* and *FINANCINGCOST* show no significant mean differences between treated and non-treated groups after matching, and the median absolute standardized bias (MASB) decreases from 21.9% before matching to 1.8% after matching. However, after matching, there is a substantial difference in variances between the treatment and control groups at the 10% level. This implies that matching is also successful here, but when compared to specification 1, this latter has a better balance as it meets all matching quality criteria and the resulting pseudo-R^2 from propensity score estimation is rather low when compared to specification 2. Tables 4 in the Appendix displays the results of the probit-estimation for both specifications.

<Insert Table 4 here>

Let us briefly discuss the main components that influence the selection into treatment. In particular, variables such as *BEBTS* and *TRADCREDIT*, turn out to be among the most important variables for the selection into treatment. These variables generally reflect the extent to which the firm relies on external sources of financing, such as suppliers and the banking system. Firms relying heavily on these sources may face heightened vulnerability to the adverse impacts of COVID-19, resulting in larger losses compared to their counterparts.

Furthermore, we test the sensitivity of our findings using alternative matching algorithm, specifically, nearest neighbor matching with and without a caliper. Our investigation shows that our results are not sensitive - the selection of the matching approach had no significant influence on the estimated treatment effects. As a result, we only present the results based on kernel matching. In addition, we present the distribution of the estimated propensity scores in Figure 2 in the Appendix.

<Insert Figure 2 here>

As we can see, the data tends to cluster around a central value with a slight bias right, where most people have a predicted probability of receiving the treatment between 0.1 and 0.2, but a few individuals with significantly higher probability of receiving the treatment contribute to the skewness. This suggests that treated individuals have a somewhat higher probability on average of being credit rationed than non-treated. Furthermore, the estimated propensity score distribution of treated individuals overlaps the region of the estimated propensity score's the distribution of non-treated individuals completely (see Figure 3 in the Appendix); therefore, the overlap assumption is fulfilled.

<Insert Figure 3 here>

In the next step we estimate the average treatment effects on the treated (ATT). We implement kernel matching based on the estimated propensity score to increase efficiency and enable bootstrapping. Bootstrapping takes account of the sampling variability in the estimated propensity score to calculate the standard errors of the estimated treatment effects. We specifically use an Epanechnikov Kernel with a bandwidth of 0.17 calculated using the Silverman-LT formula. All the bootstrap results reported in this research are based on 100 replications.

5. Results

The Conditional DiD specifically estimates the impact of the COVID pandemic (treatment) on credit rationing (the outcome variable). We find positive COVID effects during the evaluation period, which are significant. To be more specific, shortly after the COVID strikes, small and medium-sized businesses affected by the COVID pandemic have a 1.7% higher probability of credit rationing than those who are not affected by COVID. This result appears to be somewhat surprising at first glance, given that the government has advocated for a scheme to assist enterprises that have suffered financial losses as a result of COVID. One potential explanation is that enterprises with high losses are more inclined to demand larger amounts of credit to compensate for their losses and revive their activity in order to maintain their market share. However, for the banking system, these enterprises become risky borrowers since they may not recover quickly enough from their recession to repay their loans.

In what follows, we investigate the treatment's heterogeneity affects, following Caliendo and Künn (2010), to determine which groups the treatment is most important for and which groups it is less relevant for.

5.1 Effect Heterogeneity - Effects for Subgroups:
The following section delves deeper into effect heterogeneity. This is particularly valuable in determining the types of people who are most affected by the COVID epidemic.
To answer the question of who is most affected by COVID, we perform the full estimation procedure, which includes propensity score estimation and conditional DiD, on different subgroups of our sample based on two categories of observable variables: the first is related to the firms' indebtedness. It includes the variables *TRADECREDIT*, *DEBTS*, and *CASHFLOW*, whereas the second category is concerned with the amount of investment made by enterprises. It is made up of the variable *SECTOR 2*. The data are summarized in Table 6 in the Appendix, with the first column displaying the effects for the whole sample. The first row depicting the average treatment effect on the treated (ATT), and the next four rows indicate the number of individuals in the control/treated groups before and after matching.

<Insert Table 6 here>

First of all, consider the results stratified by the variable *TRADECRDIT*, which measures firms' dependence on suppliers due to outstanding debt obligations. We divided the sample into two groups: highly supplier-dependent debtors (those who have a large reliance on suppliers and have *TRADECREDIT* levels greater than the first quartile, 1.30e+08 million Tunisian dinars), and minimally supplier-dependent debtors (those who have *TRADECREDIT* levels less than 1.30e+08 million Tunisian dinars). It is clear that highly supplier-dependent debtors are more affected by the COVID epidemic and so have more credit rationing than others; the effect is approximately 0.2% greater than for weakly supplier-dependent debtors. This is mostly due to the fact that highly supplier-dependent debtors are more inclined than others to request higher loans. Banks may then perceive heavily supplier-dependent debtors as risky and curtail their loans.

Second, we look at the results stratified by the variable *CASHFLOW*, which reflects the firm's ability to deal with impending financial restrictions. We split the sample into two groups based on the first quartile of the variable *CASHFLOW*: the first is less financially resilient (comprised of enterprises with *CASHFLOW* less than the first quartile, 4.26e+07 million Tunisian dinars), whereas the second is highly financially resilient (comprised of firms with *CASHFLOW* greater than 4.26e+07 million Tunisian dinars). The less financially resilient category faces more severe credit rationing due to their perceived higher risk and lower loan repayment capabilities, with approximately 0.8% more impact than highly financially resilient firms.

Third, we explore stratified results based on the variable, *DEBTS* which relates to debts obtained from the banking sector. This variable gauges the extent of the firm's dependence on the banking system for financial support. The sample is divided into two groups based on the first quartile of the variable *DEBTS*: the first is less dependent on the banking system (comprised of firms with *DEBTS* less than the first quartile, 8.80e+07 million Tunisian dinars), whereas the second is highly dependent (comprised of firms with *DEBTS* greater than 8.80e+07 million Tunisian dinars). The findings imply that firms that rely heavily on the banking system face more credit rationing than other firms. This finding may look counterintuitive at first because borrowing heavily from banks can be regarded as a sign of trust and favorability. One would wonder why banks would limit loans to their most valuable customers and risk losing them. Closer research reveals, however, that firms strongly reliant on the financial system, like those in other industries, are likely to be greatly harmed by the epidemic. To recover from their losses and resume their operations, these firms may need a larger credit line during a crisis than they would in more stable periods. Unfortunately, the banking system may be unable to meet their requested amount, resulting in credit rationing for these firms. Furthermore, the effect is around 0.8% greater than for enterprises with less reliance on the banking system.

Finally, firms from the secondary sector (which typically includes industries such as manufacturing, construction, utilities, and energy production) have more credit rationing than firms from the tertiary sector (which encompasses various service-based industries, such as retail, hospitality, finance, healthcare, education, and professional services). The effect is approximately 0.3% more than for secondary sector than for tertiary sector.

The more severe credit rationing of the secondary sector can be attributed to several factors. Firstly, the sector's significant investments in machinery, equipment, and infrastructure expose lenders to increased risk. As capital intensity rises, lending conditions become more stringent, resulting in less credit availability. Furthermore, the longer production cycles in the secondary sector, compared to the tertiary sector, pose challenges in meeting working capital requirements. Lenders may be hesitant to provide long-term financing or working capital loans, contributing to credit rationing. Moreover, the secondary sector's susceptibility to economic fluctuations and market volatility raises lenders' risk assessments, further limiting loan availability. Unlike the tertiary industry, which frequently relies on intellectual property or intangible assets, the physical assets of the secondary sector might make it difficult to get the requisite collateral, leading to credit rationing. Finally, the limited access to alternative sources of financing, such as venture capital, crowdfunding, or angel investments, which are more prevalent in the tertiary sector, exacerbates credit rationing in the secondary sector.

6. Conclusion

The global economy has been significantly impacted by the COVID-19 pandemic, particularly affecting small and medium-sized enterprises (SMEs) to a greater extent. In this research, we evaluate the impact of COVID-19 on the fluctuation of credit rationing rates among SMEs. Our study uses panel data encompassing 626 Tunisian SMEs, covering the period from 2014 to 2020, sourced from the Central Balance Sheet Data of the Central Bank of Tunisia (BCT).

We use the semi-parametric Conditional difference-in-differences (CDiD) methodology to effectively estimate the average treatment effect on the treated (ATT). This approach allows us to control for unobservable time-invariant selection effects. Additionally, we delve into the heterogeneity of the treatment's effects to identify which groups are most affect by the pandemic.

Our findings reveal that despite governmental support measures for small and medium-sized enterprises (SMEs), the COVID-19 pandemic has led to a rise in credit rationing rates. We have examined the heterogeneity effects based on various criteria, including corporate indebtedness and investment levels. Our findings suggest that SMEs heavily dependent on the banking system for financial support, along with those having a high reliance on suppliers, and have limited financial resilience, experience more pronounced credit rationing compared to other groups. Moreover, our observations indicate that companies operating in the secondary sector face higher levels of credit rationing than their counterparts in the tertiary sector.

However, it is important to recognize certain limitations and potential future research avenues. Firstly, while our study focused primarily on analyzing a continuous outcome, it is worth emphasizing that the same analytical framework can be applied to a binary outcome, indicating whether an enterprise experiences credit rationing or not. In such circumstances, investigating the transitions between these two states can reveal further insights, especially when state dependence is present in credit rationing. Bergemann et al. (2005) demonstrated that using transition rates, rather than unconditional employment rates, was more relevant and informative in assessing the dynamic effects of training programs on employment in East Germany. Therefore, investigating transition rates in the context of credit rationing can yield valuable insights into the dynamics of this phenomenon and contribute to a more comprehensive understanding of its implications.

Secondly, Due to data limitations, our study was constrained to a single treatment implementation period. To enhance the analysis, it is recommended to extend the study by including additional periods, such as 2021 and 2022. This extension will allow us to investigate how the treatment effect may vary across different time periods. By incorporating multiple time periods, we can use the time-varying difference-in-differences (TVDD) approach, which offers a more comprehensive understanding of the treatment effect over time compared to traditional DiD models that assume a constant treatment effect throughout the study period. The TVDD methodology highlights the dynamic nature of interventions and allow researchers to assess the heterogeneity and evolution of treatment effects over time, allowing for a more comprehensive study.

Overall, our findings may provide some useful insights into policy recommendations. Our research clearly indicates that the tertiary sector faces less severe credit rationing than the secondary sector. This shows that the tertiary sector was less vulnerable to COVID-19 compared to the secondary sector, with relatively less disruptions and shutdowns, and many of its components successfully maintaining online operations. These findings highlight the need of policymakers prioritizing tertiary sector support, particularly in terms of financing service-oriented enterprises. Emphasizing sectors like technology, software, healthcare, and other service-based industries, as observed in technology hubs such as Silicon Valley in the United States and Silicon Fen in the United Kingdom, can lead to significant benefits. These sectors have consistently displayed resilience and substantial growth potential, making them highly attractive areas for investment and policy focus.

Notes

1. Tunisian Government Decree No. 2020-308, issued on May 8, 2020, outlines the criteria for determining affected companies and the conditions for their eligibility to benefit from the provisions of the decree-law of the Head of Government.

2. The details of this model, including the variables used in these equations, are extensively described in Sayari (2023).

3. The ATT primarily focuses on the treated individuals, it can provide valuable insights into the treatment effect for individuals with similar characteristics in the population. This suggests that

individuals with similar characteristics in the population may also experience a similar treatment effect if they were to receive the treatment. However, caution should be exercised when generalizing the ATT to the entire population.

4. See Section 4.1 for a definition of propensity score matching and an explanation of how it works.

5. This means that we select from the non-treated pool a control group in which the distribution of observed variables is as similar as possible to the distribution in the treated group.

6. The conventional DID incorporates insights from cross-sectional treatment-control comparisons and before-after studies for a more robust identification. First consider an evaluation that aims to estimate the effect of a (non-randomly implemented) policy ("treatment") by comparing outcomes in the treatment group to those in a control group using data from after the policy implementation. Assume there is a difference in outcomes. The basic idea behind the DiD identification strategy is to compute the difference of the mean outcomes of treated and controls after the treatment and subtract the outcome difference that existed before the treatment (conditional on a given value of covariates X). DID can effectively differentiate "time effect" from "program-treated effect" and identify a mean causal effect when the identification assumptions are met. In particular, the parallel trends assumption, which is a prerequisite for DID analysis and states that the growth trajectories of the treatment and control groups should be as comparable as possible in the pretreatment period (s).

7. All the variables that affect simultaneously D and Y are observed.

8. The Ashenfelter's Dip phenomenon was first discovered when evaluating the treatment effects on earnings (Ashenfelter, 1978). Later studies showed that the same phenomenon can occur in labor markets as well (Heckman, LaLonde, and Smith, 1999.; Heckman and Smith, 1999; Fitzenberger and Prey, 2000).

9. Abadie and Imbens (2008) show that the bootstrap is generally not suitable for nearest neighbor matching due its extreme non-smoothness. And the absence of evidence supporting the asymptotic linearity of the estimator.

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Appendix

Table 1. Empirical variable definitions and summary statistics

		Treated		Non-treated	
Variable	**Description**	**Observations**	**Mean (Std. Dev., Median)**	**Observations**	**Mean (Std. Dev., Median)**
Observable characteristics					
AGE	Years in operation	686	28.0204 (15.304, 23)	3,696	21.115 (10.5237,19)
ASSETS	Firm size (Steijvers, 2008)	686	2.96e+09 (3.76e+09, 1.40e+09)	3,696	3.03e+09 (3.12e+09, 1.76e+09)
CASHFLOW	Sum of net income, depreciation, and provisions	685	7.96e+07 (4.89e+08, 4.80e+07)	3,695	2.58e+08 (4.11e+08, 1.38e+08)
COLLATERAL	Sum of net tangible assets and net financial assets	681	8.35e+08 (1.64e+09, 1.73e+08)	3,610	8.79e+08 (1.28e+09, 3.54e+08)
DEBTS	Firm's current total credit amount	626	8.97e+08 (1.69e+09, 2.36e+08)	3,417	7.04e+08 (1.18e+09, 2.69e+08)
FINANCINGCOST	Proportion of financial costs to total debts	610	0.2662 (1.6607, 0.11027)	3,254	10.058 (324.280, 0.1301)
INTERESTCOV	Operating income to financial expenses ratio	654	34.6314 (363.624, 1.603)	3,447	15.877 (123.371, 3.2)
LIQUIDITY	Current assets to current liabilities ratio	686	4.514 (44.842, 1.3017)	3,691	13.951 (389.415, 1.284)
ROA	Net income to net assets ratio	685	-0.00562 (0.216, 0.0185)	3,693	0.0602 (0.115, .0468)
SALES	Measures the level of activity	681	2.59e+09 (3.41e+09, 1.23e+09)	3,674	3.12e+09 (3.58e+09, 1.81e+09)
SECTOR 1	Dummy: 1 if firm operates in the primary sector (including, agriculture, forestry, fishing, mining, and oil extraction); 0 otherwise	686	0 (0,0)	3,696	0.0322 (0.176,0)
SECTOR 2	Dummy: 1 if firm operates in the	686	0.40816 (0.492,0)	3,696	0.3939 (0.488,0)

	secondary sector (including industries such as manufacturing, construction, and utilities); 0 otherwise				

Table 1/Continued

		Treated		Non-treated	
Variable	**Description**	**Observations**	**Mean (Std. Dev., Median)**	**Observations**	**Mean (Std. Dev., Median)**
SECTOR 3	Dummy: 1 if firm operates in the tertiary sector (including retail, hospitality, healthcare, finance, education, and transportation); 0 otherwise	686	0.5918 (0.4918,1)	3,696	0.5738 (0.494,1)
TRADECREDIT	Accounts payable	665	7.09e+08 (1.19e+09, 2.67e+08)	3,629	8.16e+08 (1.10e+09, 3.92e+08)
Treatment					
COVID	Dummy: 1 if the firm is affected by COVID-19 pandemic; 0 otherwise	682	1 (0,1)	3,696	0 (0,0)
Outcome variable					
RATIONING RATE	Credit discrepancy ratio: (credit requested - credit granted) / credit requested	583	0.01608 (0.03146, 0.00805)	3,140	0.00749 (0.0326,0)

Source: The author's calculations.

Table 2. Exogeneity (of) /changes (in) the selected covariates *DEBTS* and *TRADECREDIT*: Ordinary Least Squares estimates of the average treatment effects on the treated (ATT) of COVID-19 on *DEBTS* and *TRADECREDIT*

Independent variable	***DEBTDS***	***TRADECREDIT***
*Time dummy*Treatment dummy*[a]	1.68e+07 (2.53e+08)	-1.68e+08 (1.30e+08)
CONSTANT	6.73e+08*** (2.07e+07)	7.97e+08*** (1.92e+07)
R-squared	0.0066	0.0028
No. obs	4,043	4,294

Notes:

[a]This is the interaction term between the time and treated dummy. The time dummy variable signifies the initiation of the treatment at a specific point in time. It is equal to 1 when the treatment started, typically in 2020; and 0 otherwise. The treatment dummy is equal 1 if the unit is affect by COVID-19; and 0 otherwise.

Inference: Robust standard errors are in parentheses and, *,**, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Source: The authors' calculations.

Table 3. Ashenfelter's dip: Ordinary Least Squares estimates of the average treatment effect on the treated (ATT) in the pretreatment periods

	Lagged (RATIONING)
Independent variables	
*Time dummy*Treatment dummy*[b]	0.0052
	(0.0032)
CONSTANT	0.00763***
	(0.0006815)
R-squared	0.0054
No. obs	3,722

Notes:

[b]The time dummy, treatment dummy, and the interaction term between the time and treatment dummies are the same as in Table 2.

Inference: Robust standard errors are in parentheses and, *,**, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Source: Authors' calculations.

Table 4. Matching quality measures: Indicators of covariate balancing, before and after matching, by set of selected covariates.

	t-test (significance in mean/variance differences) before	**t-test (significance in mean/variance differences) after**	**Percentage of Bias before**	**Percentag e of Bias After**	**Probit ps-R^2 before**	**Probit ps- R^2 after**	**Median absolute standard ised bias (%) before**	**Median absolute standard ised bias (%) after**
Set of selected observable covariates								
Specification 1: (*DEBTS* and *TRADCREDIT*)	Mean and variance differences significant	Mean and variance differences insignificant	*DBETS*: 13.2 *TRADECREDIT*: -9.4	*DBETS*: -2.6 *TRADECREDIT:* -3.1	0.006	0.000	11.3	2.3

Table 4/Continued

	t-test (significance in mean/variance differences) before	t-test (significance in mean/variance differences) after	Percentage of Bias before	Percentage of Bias After	Probit ps-R^2 before	Probit ps- R^2 after	Median absolute standardised bias (%) before	Median absolute standardised bias (%) after
Set of selected observable covariates								
Specification 2: (*CASHFLOW* and FINANCINGCOST)	Mean and variance differences significant	-Mean differences insignificant -Variance differences significant	*FINANCINGCOST*: -4.3 *CASHFLOW*: -39.5	*FINANCINGCOST*: -0.1 *CASHFLOW*: 3.4	0.027	0.0002	21.9	1.8

Source: Authors' calculations.

Table 5. Probit-estimation estimates

	Specification 1	Specification 2
Selected covariates		
DEBTS	1.44e-10*** (3.22e-11)	-
TRADECREDIT	-1.34e-10*** (4.73e-11)	-
FINANCINGCOST	-	-0.0664 (0.04689)
CASHFLOW	-	-1.21e-09*** (1.35e-10)
CONSTANT	-1.723*** (0.0567)	-1.4517*** (0.04934)
Pseudo R^2	0.006	0.031

Notes: Propensity scores estimated with 1% caliper; standard errors are in parentheses and *,**,*** indicate significance at 10%, 5%, and 1% levels, respectively.

Source: Authors' calculations.

Figure 1: Violation of parallel trends assumption

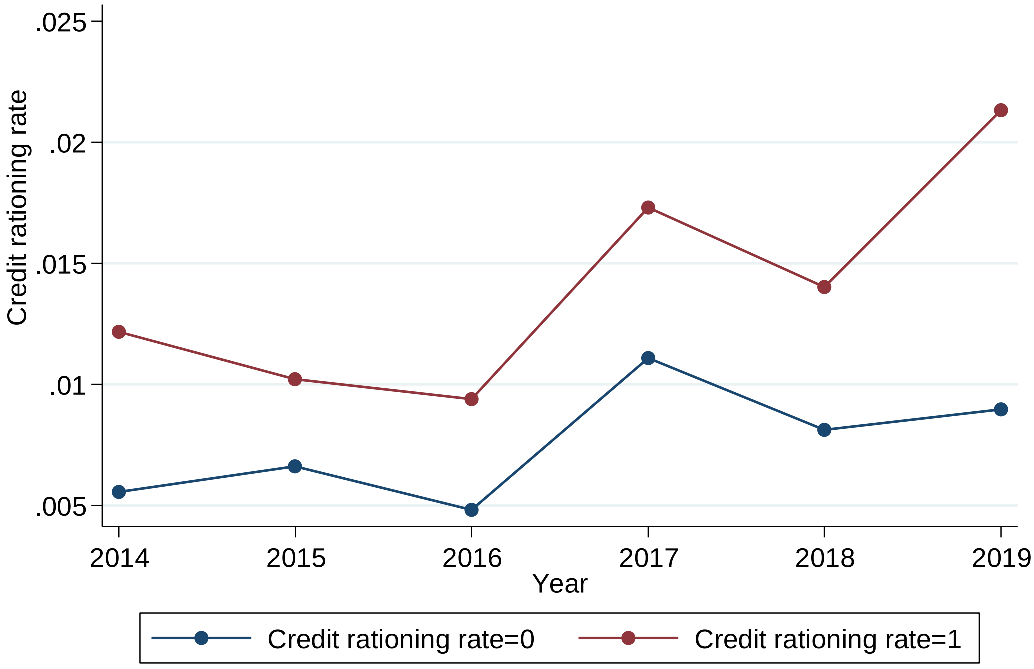

Figure 2: Distribution of Propensity Score Index

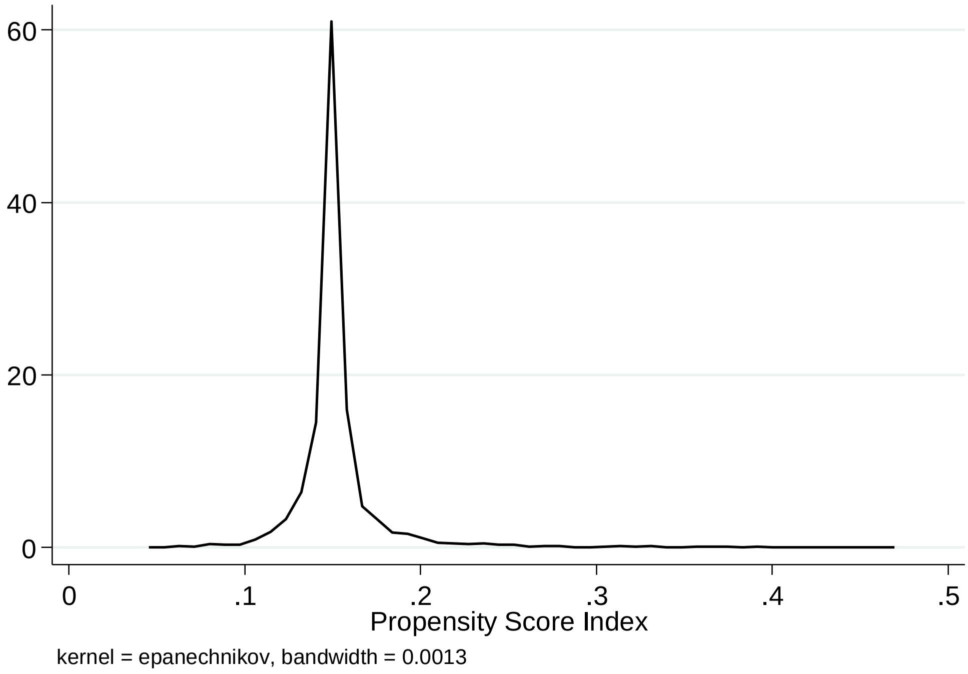

Figure 3: Overlap of Distributions of Propensity Score Index for Treated and Non-treated Units

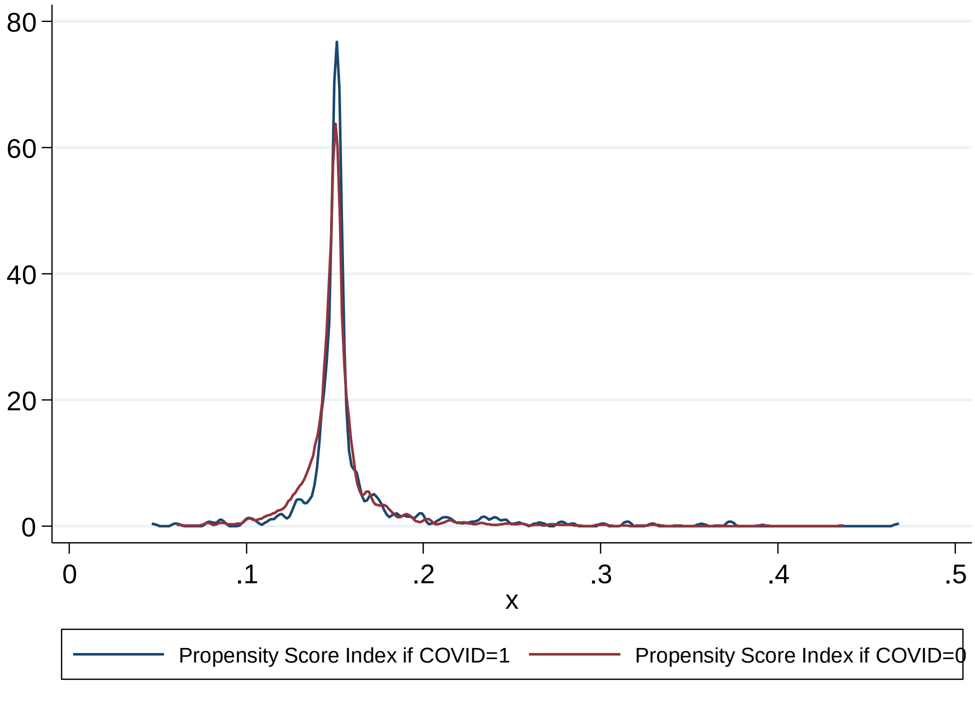

Table 6. Causal Effects of the COVID-19 pandemic

	Whole sample	Subgroup: ***TRADCREDIT*** **< Q1=1.30e+08 million Tunisian dinars**	Subgroup: ***TRADCREDIT*>** **Q1=1.30e+08 million Tunisian dinars**	Subgroup: ***DEBTS*<** **Q1=8.80e+07 million Tunisian dinars**	Subgroup: ***DEBTS*>** **Q1=8.80e+07 million Tunisian dinars**	Subgroup: ***CASHFLOW*** **<Q1=4.26e+07 million Tunisian dinars**	Subgroup: ***CASHFLOW*** **>Q1=4.26e+07 million Tunisian dinars**	Subgroup: Secondary sector	Subgroup: Tertiary sector
ATT[c]	1.7*** (0.005)	1.6** (0.005)	1.8** (0.007)	1.3** (0.006)	1.8*** (0.006)	1.3* (0.007)	0.5* (0.003)	1.9*** (0.004)	1.6* (0.008)
Control before	2692	554	2127	650	2029	524	2168	1091	1509
Control after	448	73	361	87	360	91	348	179	253
Treated before	498	150	347	110	387	195	302	209	289
Treated after	85	23	55	17	68	72	13	32	53

Notes:

[c]Depicted are average treatment effects on the treated (in percentage) as the difference in outcome variables between treated and non-treated small and medium-sized enterprises. Standard errors are in the parentheses and are based on *bootstrapping* with 100 replications.

Inference: *,**, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Source: Authors' calculations.