README: This was one half of my final year Advanced Econometrics project. It was marked highest in the cohort with a mark of 82/100. I answer the following question(s):

The legendary country of Albion consists of a northern and a southern state. To mitigate the effects of the current crisis, the northern state introduced a job retention scheme in 2020, which provided a loan worth 20% of the total payroll of the firm but had to be repaid within five years. All firms that applied for the loan received the loan but many firms did not apply. The southern state, concerned about the expense of such a scheme, did not introduce a job retention scheme.

The government would like you, firstly, to estimate a model to show which factors predict receipt of the loan (25%). Secondly, the government would like you to estimate the causal effect of the loan on firm employment (75%).

To allow you to conduct such an evaluation, you are given data from a random sample of 1,000 firms for the period 2018-20 (see Table 1 for a description of the variables in the dataset). 500 of these firms are located in the northern region and 500 are located in the southern region. Each of you will analyse a different sample (so your results will be different to those of your colleagues).

Table 1. Variable Descriptions

Variable	Description
id	Firm identification number
year	Year
employment	Number of employees at end of the year
loan	Dummy variable coded 1 if firms receives the loan
age	Age of firm at start of the year
leverage	Total liabilities divided by total assets at start of the year
industry	Categorical variable coded 1 if firm is in agriculture, 2 if firm is in manufacturing, 3 if firm is in services
state	Dummy variable coded 1 if firm is located in the northern region

1 Introduction

Governments across the globe have been challenged with offsetting the substantial effects of COVID-19 on their domestic economy. The ripples of this economic shock are particularly prevalent across labour markets, given the nature of the virus, and job retention schemes have accordingly come to the fore of global economic policy. Lam (2023) found that these schemes helped mitigate the rise of the unemployment rate, by about 3 percentage points, in Europe during the pandemic.

This study investigates the effectiveness of the northern government job retention scheme in the country of Albion, using a dataset of 1000 firms across the country from 2018-2020. Matching and difference-in-difference methods are utilised to elicit the contribution of the job retention loan to firm employment. Alongside this, a model showing which factors predict the firms receipt of the loan is devised.

2 Data

The sample consists of data gathered on 1000 Albionian firms in the timespan 2018-2020, with an exact split of 500 northern and 500 southern firms in the panel. It is worth noting that no such loan scheme was available to the southern firms in the timespan, and therefore all the firms receiving 'treatment' are northern (330 of the 500). Table 1 outlines the descriptive statistics of the variables between the untreated and treated group ***in the pre-treatment period***. Table 2 details the industry makeup of each group.

Table 1 (84) Variable	Obs. Un	Obs. Tr	Mean Un	Mean Tr	S.D. Un	S.D. Tr	Min Un	Max Un	Min Tr	Max Tr
Number of Employees	1,340	660	7.5821	10.485	2.8399	2.961	1	17	1	21
Age of firm	1,340	660	15.528	15.279	8.4155	8.438	1	31	1	32
Leverage	1,340	660	0.3914	0.3948	0.1988	.208	0	1.184	0	1.033
State	1,340	660	0.2537	1	0	0	0	1	1	1
Change in employment	670	330	0.1701	0.9515	2.9138	2.952	-10	9	-8	12

Table 2 (26)

Industry count	*Untreated* *(of which southern/northern)*	*Treated*
Agricultural	140 (115/25)	20
Manufacturing	338 (244/94)	128
Services	192 (141/51)	182
Total	670 (500/170)	330

The age and leverage of the firms across the two groups are similar. However, the mean number of pre-treatment employees is notably higher in the treatment group (10.4, relative to 7.5) alongside the change in number of employees - on average the treated group expanding and the untreated contracting their employee count. The makeup of industries is also not consistent between the two groups, with the modal industry of the treated group being services (55% of firms) vs manufacturing in the untreated group (50% of firms). In addition, whilst 87.5% of agricultural firms were in the untreated group, 44% of agricultural firms in the north did receive a loan. Table 3 provides descriptions of the variables.

Table 3 (86)

Variable	*Description*
Id	Firm identification number
Year	Year
Employment	Number of employees at end of the year
Loan	Dummy variable coded 1 if firms receives the loan
Age	Age of firm at start of the year
Leverage	Total liabilities divided by total assets at start of the year
Industry	Categorical variable coded 1 if firm is in agriculture, 2 if firm is in manufacturing, 3 if firm is in services
State	Dummy variable coded 1 if firm is located in the northern region

3 Methodology

This chapter is split into two sections. The first addresses the estimation methods of modelling the receipt of the loan ('probability model'), and the second details the estimation of the causal effect of the loan on employment ('causal model'). The casual models draw in some part from the probability model, specifically in the propensity-score method, so some pre-emptive references are made.

3.1 Probability Model

The most prevalent binary choice models are that of probit, logit and Linear probability model (LPM). In order to assess robustness of results, estimations using all three methods are reported for the specified model equation. Given the internal inconsistency of the LPM – due to the lack of a link function that maps the predictor variables to the [0,1] interval – the LPM results are reported only as reference. In particular, as the dataset itself is homoscedastic, and LPM errors are heteroskedastic, the use of a LPM as a propensity scoring method may be problematic (specifically the confoundedness assumption may not hold).

Caution must be taken, given the nature of the dataset, in specifying the form and sample upon which the probability model is estimated. Across the 3000 firm/year observations, there are only 500 observations in which the receipt of the loan was *possible* – for northern firms in 2020. This has two implications: Firstly, the estimated sample must only include firms in the north[1]. Secondly, the model mustn't include regressors that are themselves influenced by the receipt of the loan. Specifically the regressors of leverage and employment in the year 2020 must be excluded: (1) The receipt of the loan directly has an impact on the liabilities of a firm, thus the leverage and (2) the purpose of the loan is to be spent on employee retention so is expected to influence the number of employees. Therefore, only the lags (i.e. year='18,'19) of these variables are present in the model.

The regression equation is specified as follows, where subscript "i,t,N" represents firm "i" in year 2000+"t" in the Northern state:

$$\begin{aligned} loan_{i,20,N} = {} & \beta_1 \Delta employment_{i,19,N} + \beta_2 employment_{i,18,N} + \beta_3 \Delta leverage_{i,19,N} \\ & + \beta_4 leverage_{i,18,N} + \beta_5 age_{i,18,N} + \boldsymbol{\beta_6} industry_{i,N} \qquad (1) \end{aligned}$$

Where $\Delta X_{i,19,N} = X_{i,19,N} - X_{i,18,N}$

For the two time-variant (non-age) variables, the 1st lagged *change* of the variable is included alongside the 2nd lagged *level* of the variable. Apropos to economic theory, firms that undergo greater rapid expansion are more vulnerable to the effects of macroeconomics shocks[2]. Capturing both the level and change incorporates both the size of the firms, and an approximation of the

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[1] Mechanically, in order for the maximum likelihood estimate to converge, 'state' variable must be excluded if using the full sample as it is a perfect predictor of non-treatment.

[2] See, for instance the works of Minsky (1975)

expansion path at the point of the crisis, which is likely to influence the decision to apply for a loan (whilst also mitigating correlation from including both years as levels).

Specifically, it is posited that firms with a greater number of employees (and those that have just increased the number) may be in a greater need/want of this loan. Whether it is hypothecated as intended to prevent layoffs through funding wages, or spent elsewhere, remains to be seen. The expected sign of the leverage variables is unclear. One may expect, given the aforementioned rate of expansion, that firms with growing leverage may be in a position of requiring economic relief. However, those firms with a larger debt to assets ratio may find taking on more debts (i.e. receiving the loan) as unviable. It is worth noting given the relatively few variables in my dataset, there are likely to be variables that predict the receipt of the loan that we do not observe. In particular, even if these omitted variables are uncorrelated with those in the model, the coefficients of those included are vulnerable to 'attenuation' bias towards zero (Mood, 2010).

3.2 Causal Model

The primary goal of this study is to investigate the causal effect of receiving the government loan on firm employment. Whilst the overall sample is randomly selected, the selection into the treatment is not (that is, for the northern firms). As such, one ***cannot*** assume that this stochastic independence between treatment (receiving the loan) and the outcome (firm employment) shown by (2) is true:

$$\text{Loan}_{i,20} \perp (\text{employment}^0_{i,20}, \text{employment}^1_{i,20}) \tag{2}$$

Where employment^j_i refers to the potential number of employees of firm "i" having received (j=1) or not received (j=0) the loan.

For instance, if the firms who receive the loan have common *confounding* factors (e.g. being new start-ups, low leverage, predominantly being in one industry), and these factors have an effect on the firm's employee numbers in 2020 regardless (*or not*[3]) of if treatment had happened, then one may misattribute the causal effect of the loan to what is actually being caused by the confounding variables. If the relationship described in (2) were true, the means of the treated and untreated group post-treatment could be compared to estimate the average treatment effect:

$$ATE := E\left(emp_i^1 - emp_i^0\right) = E\left(emp_i^1 \middle| Loan_i = 1\right) - E\left(emp_i^0 \middle| Loan_i = 0\right)$$

$$= E\left(emp_i \middle| Loan_i = 1\right) - E\left(emp_i \middle| Loan_i = 0\right)$$

Since $E\left(emp_i^j \middle| Loan_i = 1\right) = E\left(emp_i^j \middle| Loan_i = 0\right)$ for j={0,1} due to the assumption (2) therefore

[3] If concerned by only the average treatment effect of the treated (ATET) then this doesn't matter, but if the interaction of these confounding factors and receiving the loan *also* has an effect on the outcome (on top of just the confounding differences) then this must be considered for the ATE.

$$\widehat{ATE} = \overline{emp^{1}_{\square}} - \overline{emp^{0}_{\square}}$$

is a suitable approximation for the average treatment effect. If one can control for the set of confounding variables $\boldsymbol{Xi}$ that selection is based on, then conditional stochastic independence (3) can be assumed (the *unconfoundedness* assumption):

$$\text{Loan}_{i,20} \perp (\text{Employment}^{0}_{i,20}, \text{Employment}^{1}_{i,20}) \mid \boldsymbol{Xi} \quad (3)$$

Which implies $E\left(emp_i^j \mid \boldsymbol{X_i}, Loan_i = 1\right) = E\left(emp_i^j \mid \boldsymbol{X_i}, Loan_i = 0\right)$ for j={0,1} so unbiased estimates of the ATE can be estimated as above when both are conditioned on $\boldsymbol{Xi}$. The propensity-score theorem shows that the equivalent relation to (3), call it (4), that conditions on a propensity-score p($\boldsymbol{Xi}$) rather than the vector $\boldsymbol{Xi}$, also implies conditional stochastic independence (see Rosenbaum and Rubin (1983)).

The most common methods that build upon assumptions (3) and (4) respectively are nearest neighbour matching (NNM) and propensity score matching (PSM). The two approaches bring differing benefits in this specific context. Given the multi-dimensional, multi-datatype observed covariates vector, finding suitably close-in-distance firms across the treated and untreated group is not guaranteed, therefore propensity scoring offers a more pragmatic metric (avoiding the 'curse of dimensionality'). However, using NNM based on Mahalanobis distance allows the relevance of the individual covariates on the outcome variable itself to be represented through weighting or exact matching, whereas the propensity score does not[4].

In order to find method robust results, given these caveats, both matching approaches are explored and compared. These two methods are combined with Difference-in-Differences (v2) estimation. Given the availability of pre/post-treatment period cross-sections, DiDv2 attempts to control for unobserved fixed (or compounding) effect confounding variables by 'removing' the difference in trends between the two groups. The two methods, alongside a reference OLS method, are detailed next in this chapter.

Method 0 (OLS):

An ordinary least squares regression on

$$\begin{aligned} employment_{i,20} &= \alpha_{\square} + \beta_0 loan_{i,20} + \beta_1 \Delta employment_{i,19} + \beta_2 employment_{i,18} \\ &+ \beta_3 \Delta leverage_{i,19} + \beta_4 leverage_{i,18} + \beta_5 age_{i,18} + \boldsymbol{\beta_6} industry_i \\ &+ \beta_7 state_i \end{aligned}$$

performed as reference. Whilst state can be explicitly controlled for, the pooling of periods means difference in differences can't be exploited across control groups, thus there is greater potential for time-invariant omitted variables to confound estimates.

[4] as the PS bears no direct reflection on the outcome variable itself, just the propensity to be treated.

Method 1 (NNM):

Using nearest-neighbour matching, an individual firm from the treated is matched (with replacement) with a firm from the untreated group, based upon the covariates as specified in the probability model equation (1)[5]. The average effect of treatment effect (ATE) is estimated using the difference *in differenced* (i.e. DiD v2) employment variable in 2020:

$$\beta_{ATE} = \frac{1}{N}\sum \widehat{\nabla Y}^1_{i,20} - \widehat{\nabla Y}^0_{i,20} \qquad (5)$$

With $\widehat{\nabla Y}^1_{i,20} = \begin{cases} \Delta Y_{i,20} - \Delta Y_{i,19} & if\ D_i = 1 \\ \Delta Y_{j,20} - \Delta Y_{j,19} & if\ \ D_i = 0 \end{cases}$ and $\widehat{\nabla Y}^0_{i,20} = \begin{cases} \Delta Y_{j,20} - \Delta Y_{j,19} & if\ D_i = 1 \\ \Delta Y_{i,20} - \Delta Y_{i,19} & if\ D_i = 0 \end{cases}$

Where $\Delta Y_{i,T} = Y_{i,T} - Y_{i,T-1}$

Firm "i" from the *treated* group is matched with firm "j" from the *untreated* group based on Mahalanobis distance. Y = "emp" as the outcome variable and D = "loan" the treatment variable, N is the sample size.

The DiDv2 estimation is incorporated in the summand of the matched estimator, evaluating in the treated (Di=1) case:

$$\frac{1}{N_{tr}}\sum \widehat{\nabla Y}^1_{i,20} - \widehat{\nabla Y}^0_{i,20} = \frac{1}{N_{tr}}\sum((\Delta Y_{i,20} - \Delta Y_{i,19}) - (\Delta Y_{j,20} - \Delta Y_{j,19}))$$

$$= \left(\frac{1}{N_{tr}}\sum \Delta Y_{i,20} - \frac{1}{N_{tr}}\sum \Delta Y_{i,19}\right) - \left(\frac{1}{N_{tr}}\sum \Delta Y_{j,20} - \frac{1}{N_{tr}}\sum \Delta Y_{j,19}\right)$$

$$= \left(\frac{1}{N_{tr}}\sum Y_{i,20} - \frac{1}{N_{tr}}\sum Y_{i,19}\right) - \left(\frac{1}{N_{tr}}\sum Y_{i,19} - \frac{1}{N_{tr}}\sum Y_{i,18}\right) - \left[\left(\frac{1}{N_{tr}}\sum Y_{j,20} - \frac{1}{N_{tr}}\sum Y_{j,19}\right) - \left(\frac{1}{N_{tr}}\sum Y_{j,19} - \frac{1}{N_{tr}}\sum Y_{j,18}\right)\right]$$

$$= \left(\mathbb{E}\left[Y^1_{i,20}\middle|D_i = 1\right] - \mathbb{E}\left[Y^0_{i,19}\middle|D_i = 1\right]\right) - \left(\mathbb{E}\left[Y^0_{i,19}\middle|D_i = 1\right] - \mathbb{E}\left[Y^0_{i,18}\middle|D_i = 1\right]\right) - \left[\left(\mathbb{E}\left[Y^0_{j,20}\middle|D_j = 0\right] - \mathbb{E}\left[Y^0_{j,19}\middle|D_j = 0\right]\right) - \left(\mathbb{E}\left[Y^0_{j,19}\middle|D_j = 0\right] - \mathbb{E}\left[Y^0_{j,18}\middle|D_j = 0\right]\right)\right]$$

$$= \widehat{ATET}$$

In the treated case (N_{tr} is number of treated), where the common trends assumption is employed to get from line 4 to 5, which is discussed in greater depth shortly. An equivalent

[5] Note *matching* on (ΔX_{t-1}, X_{t-2}) and (X_{t-1}, X_{t-2}) is equivalent as the same information is encoded.

derivation for estimating the ATE of the *untreated*, thus the whole summation approximates β_{ATE} (one can also find estimates for the β_{ATET} as above).

Method 2 (PSM):

A propensity score p($\boldsymbol{Xi}$) is generated for each firm in the population of interest (see following discussion) using the logit probability model. Each firm is grouped into four evenly spaced strata based on this score:

$$Q1 = (0,0.25)\ Q2 = [0.25,0.5)\ Q3 = [0.5,0.75)\ Q4 = [0.75,1)$$

Difference in differences 'v2' is taken, using the difference in trends between 2018 and 2019 for each stratum as an indicator for the difference in the trend the following year. The average treatment of the treated is weighted by the number of treated firms in each stratum.

<u>Assumptions and Caveats</u>

There are a number of key assumptions in this context that guide the employment of each method 1 and 2, particularly in relation to the population of interest and the observed variables Xi in our dataset.

Firstly, the matching methods rely on confoundedness given the Xi variables in our dataset - which are fairly limited. In the case of omitted variables that have a fixed or constantly compounding effect (i.e a linear trend) *over time,* employing the DiDv2 estimation controls for these confounding variables. The DiDv2 assumption we must make is that the firms (either matched or in the same stratum) in the treated and untreated group would've stayed on the *same respective trend* they were on before the treatment period (2020). This brings us to the other key consideration, regarding the population of interest.

Blasé inclusion of the 'state' variable, treating as any other, is problematic given the estimation methods rely on the overlap assumption[6]. Therefore, there are two options for sample selection: either include the full sample or only the 500 northern firms. There may be fundamental differences between the northern and southern firms (in other words, *additional* omitted variables that are no longer constant across the population given the population has 'expanded'), that have an effect on employment and the (hypothetical) counterfactual treatment circumstances. In which case, one should restrict the process to only between northern firms, as both common trends and unconfoundedness would otherwise be violated. However, this could effect a counter issue: If there are omitted variables *within* the north that are correlated with the treatment decision, and have an effect on employment, the unconfoundedness assumption is broken. This *con*foundedness could be significantly reduced by including the southern firms, as then the correlation between these omitted variables and the treatment groups are significantly reduced (as the southern firms have no choice of a loan). However, that is only the case if the south and north are sufficiently 'similar', or else the previous issue arises. This leaves a question of which

[6] That is $0 < \Pr(Loani = 1|Xi = x) < 1$ for all possible values of x. However $\Pr(Loani = 1 \mid Statei = 0) = 0$.

assumption (north/south similarity vs a proper set of Xi for northern unconfoundedness) is more likely, and therefore which one does more ‘harm’ than ‘good’ to the estimates.

For example, if northern firms are all already subsidised in some way (assume subsidies have an effect on employment) and southerners aren’t, introducing the southern firms would significantly weight the untreated group to “subsidy=0” (that we don’t observe and can’t control for), and therefore would confound results. Suppose now that there’s a similar mix of subsidised firms in both north/south, but that subsidy=0 is correlated with the treatment in the north. Then including the southern firms in the untreated group significantly reduces the correlation/confounding. One may be tempted to throw out the untreated northern firms, but this would be an additional arbitrary decision to be made, given the unknown distribution of the unobserved variables in each region.

The following assessment is made to determine whether a full or just northern sample should be used: For each propensity score strata DiDv2 trends of the northern untreated firms are evaluated separate from the southern (all untreated) firms. If both groups of firms deviate from their existing trend the same signed amount, then this is a good indication that the north and south are ‘similar’ in characteristics (i.e. no additional omitted variables are introduced that affect employment in 2020), and therefore can be included to help offset potential time-variant confoundedness *within* the treatment space (the north).

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4.1 Probability Model Results

Tables 3 and 4 present the results of the probability model across each estimation method. The former shows the regression coefficients, and the latter shows the average marginal effects.

Table 4

Regression	Logit		Probit		Linear	
loan	Coef.	SE	Coef.	SE	Coef.	SE
Change in employment L1	.361***	.053	.207***	.03	.051***	.007
employment L2	.708***	.07	.409***	.037	.098***	.007
age L2	-.078***	.016	-.045***	.009	-.011***	.002
Change in leverage L1	-.678	1.395	-.434	.79	-.121	.191
leverage.L2	.939	.621	.603*	.356	.165*	.09
Manufacturing Dummy	-.738*	.431	-.416*	.252	-.084	.066
Services Dummy	.06	.435	.04	.255	.026	.067
Constant	-4.329***	.635	-2.52***	.357	-.117	.084
Count	0.798		0.798			
Pseudo r-squared	0.334		0.335		0.3462 (adj R-sq)	
Chi-square	214.195		214.600			
Akaike crit. (AIC)	442.841		442.435		475.299	

**** p<.01, ** p<.05, * p<.1*

Table 5

Average Marginal Effects	Logit		Probit		Linear	
loan	dy/dx	SE	dy/dx	SE	dy/dx	SE
Change in employment L1	0.0498***	0.00602	0.05***	0.006	.051***	.007
employment L2	0.0977***	0.00530	0.098***	0.005	.098***	.007
age L2	-0.0107***	0.00201	-0.011***	0.002	-.011***	.002
Change in leverage L1	-0.0936	0.1925	-0.104	0.189	-.121	.191
leverage.L2	0.129	0.0849	0.144*	0.084	.165*	.09
Manufacturing Dummy	-0.104*	0.0570	-0.102*	0.058	-.084	.066
Services Dummy	0.00779	0.0572	0.009	0.059	.026	.067
Constant	0.0498***	0.00602	0.05***	0.006	-.117	.084

Note: dy/dx for factor levels is the discrete change from the base level.

The average marginal effects are very similar across all three estimation methods. In the logit regression, the second lag of leverage just falls out of statistically significance (p=.103). Additional specifications of the variables found no higher r-squared or count fit-scores than these. As posited, a greater number and expansion of employees in previous years, as well as being a younger firm, is associated with a greater likelihood of applying for the loan. Whilst the change in leverage in 2019 is statistically insignificant, interestingly a higher leverage in 2018 is associated with an *increased* likelihood of applying. This affirms that higher leverage positions, potentially more financially vulnerable, see the further increase in leverage justifiable. Manufacturing was the only statistically significant predictor of the industry dummy variables (Agriculture was also insignificant when manufacturing was excluded). Other specifications of the model were regressed upon, all of which fell below the R-squared value for the specification listed. The R-squared and Count fit-values can be seen in particular in the density of non-treated north and treated predictions of the logit regression. These histograms were very similar across all three.

Figure 1 (logit)

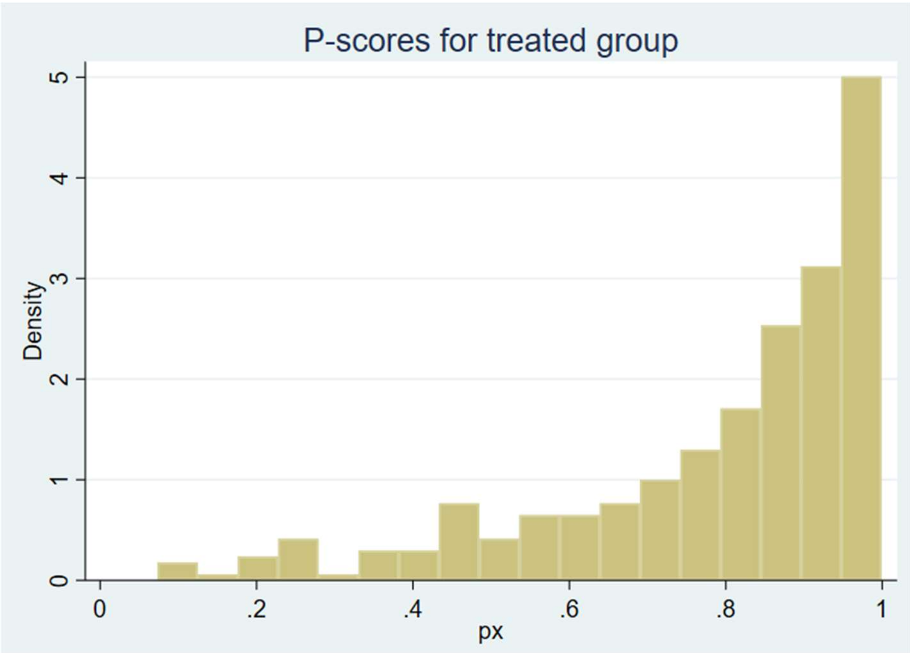
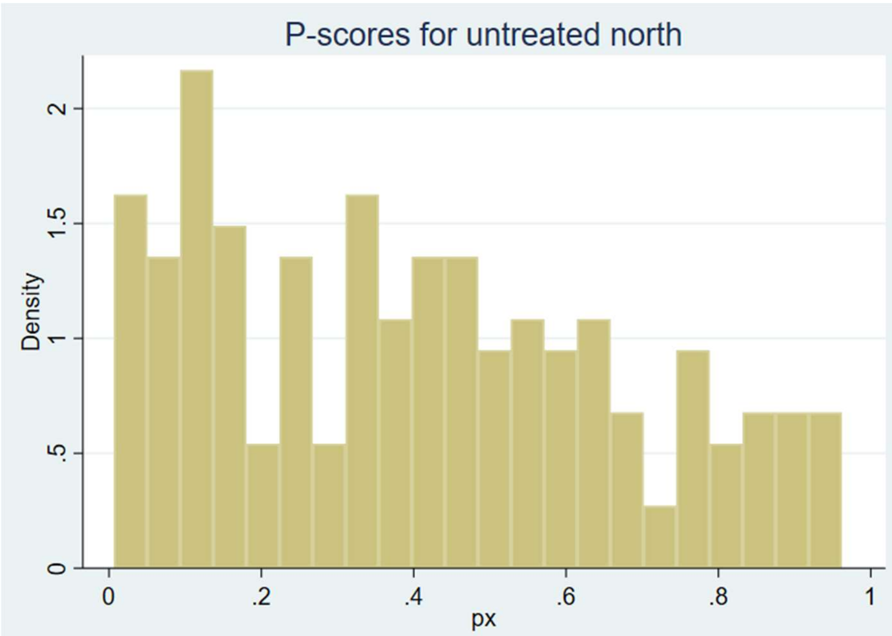


Figure 2 (logit)



4.2.1 Causal model Pre-results

Comparing untreated north and untreated south trends across strata. Number of observations in brackets.

Figure 3

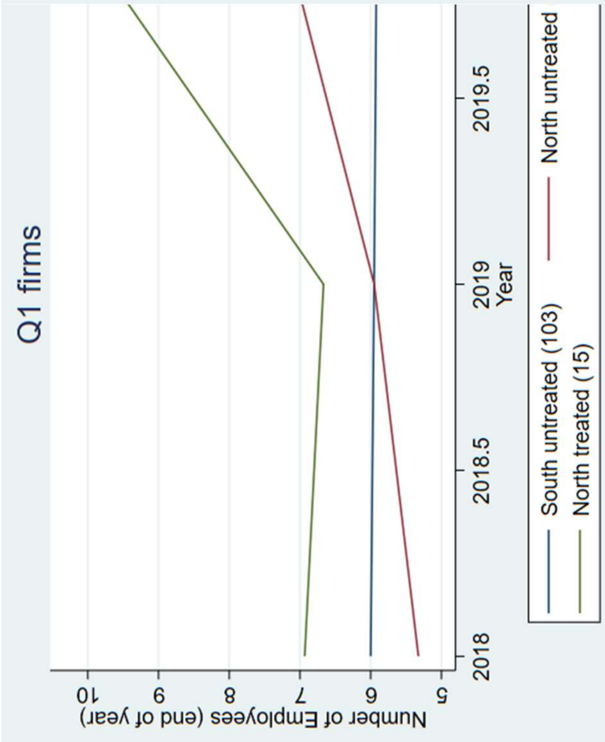


Figure 4

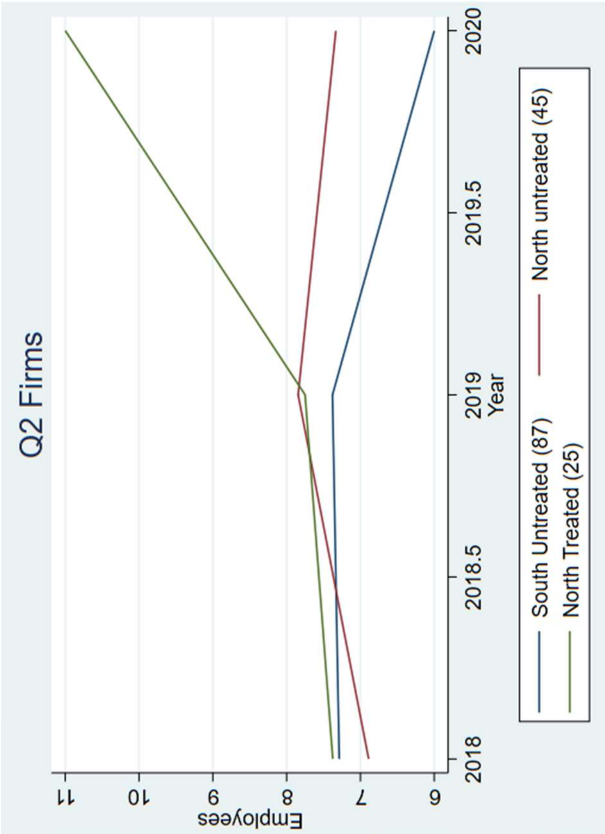


Figure 5

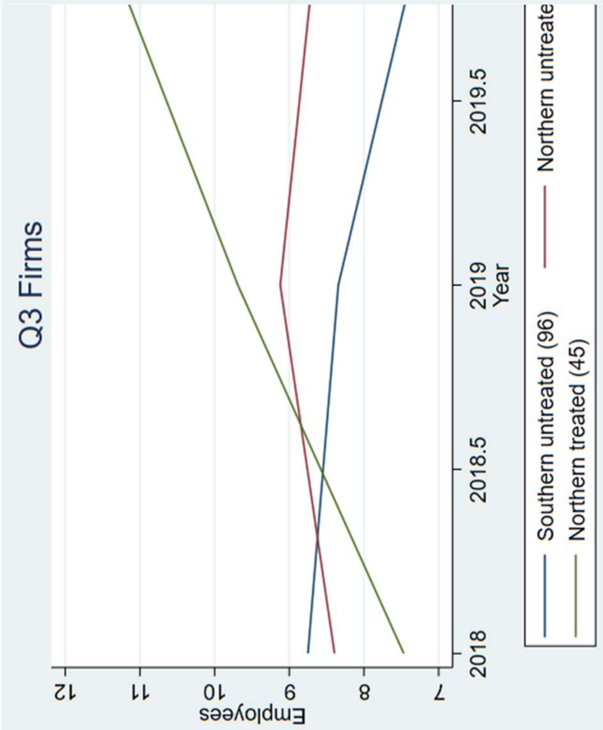


Figure 6

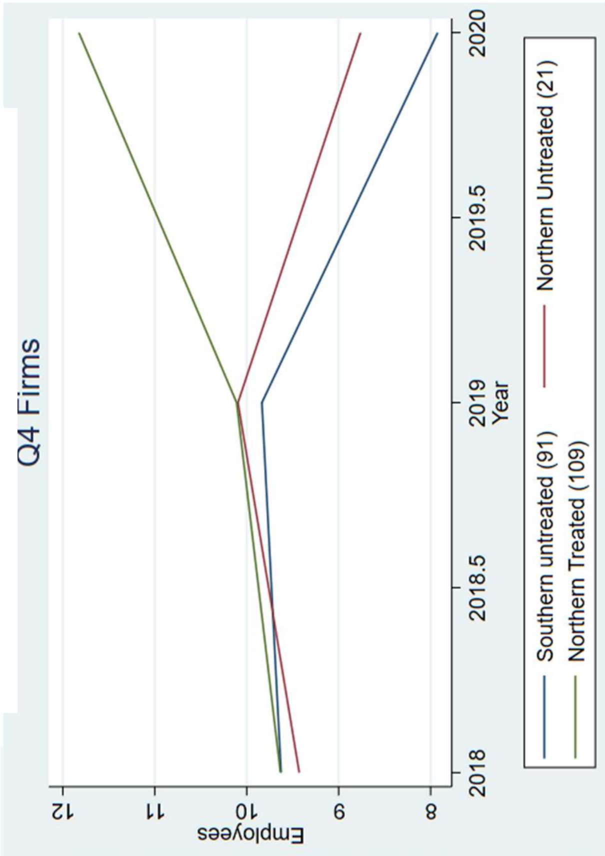


Table 6 Deviation from previous year trend	North Untreated	South Untreated	Difference in deviations
Q1	0.725	0.009709	0.715291
Q2	-1.46667	-1.47126	0.004598
Q3	-1.24242	-0.76042	-0.48201
Q4	-2.0007	-2.12088	0.120875

In all four strata, there is minimal difference in trend deviations between the north and southern untreated groups. Untreated northern firms were with fewer employees on average in 2018, but in all strata the untreated northern firms overtook the southern firms by 2019. *Relative to this previous trend,* both groups across all the strata saw a very similar decline in employee numbers (versus if they'd have kept employing *at the same rate*). This gives a strong indication to include the southern firms into the comparison group, showing no evidence of unobserved additional omitted time-varying variables across regions that influencing the pandemic's effect on employment.

Table 7 shows how well the NN matching (method 1) has balanced the covariates between the untreated and treated in those that matched, relative to the raw untreated and treated groups. Most of the covariates are reasonably well balanced, with a standardised difference close to 0 and variance ratio close to 1. The exceptions are the two lagged employment variables, which is unsurprising given the notable difference of distribution across the raw groups (see also table 1).

Table 7 Matching balance	Standardized differences		Variance ratio	
	Raw	Matched	Raw	Matched
l2employment	0.866785	0.183348	.8864087	1.130853
ldemployment	0.269486	0.049385	1.019976	1.394457
l2age	-0.02967	-0.01032	.9984942	1.095961
l2leverage	0.02939	-0.00393	.8814574	1.113709
ldleverage	-0.05078	0.00114	1.073055	1.185054
industry	0.629363	0.039761	.7593521	1.002779

4.2.2 Causal model Main Results

Table 7

Employmen t	Coeff.	Std. err.	z	p-value	[95% conf.	interval]
ATE (Method 0 OLS, state factored)	2.928948	.2407535	12.17	0.000	2.456502	3.401394
ATE (Method 1 NN)	2.502	0.391449	6.39	0.000	1.734775	3.269225
ATET (Method NN)	3.190909	0.603294	5.29	0.000	2.008475	4.373344
ATET (Method 2 PSM)	3.087665	- (calculated manually)	-	-	-	-

The average treatment effect estimate of NNM is below that of the OLS regression, implying that receipt of the loan would, on average across the whole country, allow 2.5 more people to stay employed than if the loan had not been received. However, the extent of the additional assumptions required for the ATE to be valid (beyond that of ATET) mean I do not emphasise this result. The two methods calculating ATET provide similar results, estimating the average effect of the loan on employment *for those that received it* as around 3.1-3.2 employees retained. This similarity between methods offers additional robustness verification.

Conclusion

Using a combination of matching and difference in difference techniques, I found method robust results implying the average casual effect of the loan on those who received it was retainment of just over 3 additional employees relative to no loan being received. Whilst these were method robust, the limited amount of data on each firm means various assumptions had to be employed. In particular, if there were time-variant factors we did not observe (for instance, the southern firms having a separate scheme) the results would lose validity as this would violate the key assumptions. A prediction model for the receipt of the loan was also devised, finding a greater number and expansion of employees in previous years, as well as being a younger firm, is associated with a greater likelihood of applying for the loan.

Section A Bibliography

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Applied Econometrics Question B

B1) Check for non-stationarity of the oil prices and then investigate the descriptive statistics (including higher moments) of the returns. How would you compute the confidence intervals for the moments? What is your inference?

The time period selected is from 31st January 2021 to 31st January 2023. Relative to the ongoing Russian-Ukraine war which formally began in February 2022, the sample includes data a year either side. In particular, the start date is drawn a year after the beginning of the Covid-19 pandemic, as prices returned to that of pre-pandemic by this timeframe.

B1.1 Non-stationarity

Stationarity is a property of a time series or stochastic process Y_t that is characterised by the distribution of Y_t being independent of time (Stock and Watson). In particular, this means the joint distribution $(Y_1, Y_2, Y_3, \dots, Y_t) = (Y_{1+m}, Y_{2+m}, Y_{3+m}, \dots, Y_{t+m})\ \forall\ t, m$. In the opposite case, that is *non*-stationarity, time series are typically characterised by either *deterministic* or *stochastic* trends (Stock and Watson).

A time series with a deterministic trend (being *trend stationary*) can be seen in equation (1).

$$Y_t = \alpha + \beta t + u_t \tag{1}$$

Assuming u_t itself is white noise, removing this deterministic trend means the remaining residual series (i.e. u_t) is stationary stochastic.

$$(Y_t - \alpha - \beta t) = u_t$$

A time series that exerts a stochastic trend (being *difference stationary*), can be seen for example in an AR(1) model:

$$Y_t = c + \varphi_1 Y_{t-1} + u_t \tag{2}$$

or in operator form $$(1 - \varphi_1 L)Y_t = c + u_t$$

If $|\varphi_1| < 1$ (i.e. the root $\frac{1}{|\varphi_1|} < 1$), the process is stationary (Stock and Watson, Appendix 15.2). However, if $|\varphi_1| = 1$ then the characteristic equation has a root at $\mathrm{L} = 1$ (a.k.a. having a *unit root*) and this means the process has a stochastic trend and is not stationary. The same condition generalising to the AR(p) case ($\mathrm{p} \in \mathbb{Z}^+$), where all the roots of the characteristic must be outside the unit circle for stationarity.

In the case where there are *d* unit roots, it can be shown that differencing the equation *d* times leads to a stationary process (denoted an I(d) series). In the I(1) case:

$$\Delta Y_t = Y_t - Y_{t-1} = c + \delta Y_{t-1} + u_t \qquad \text{(Where } \delta = 1 - \varphi_1\text{)}$$

This form sets up the "Dickey-Fuller" hypothesis test of δ, with hypotheses