RA Task Written Response

Pryce Davies

November 2021

1 Maximum Likelihood

1.1 d

My estimation routine only took a few seconds using the optim function as there were only two parameters of interest and 1000 observations. Computing the mean optimum value and the standard errors via bootstrapping took around a minute. To shorten this time, I could use less iterations for my bootstrap, but ideally still more than 1000. I could also use the boot package in R, which is specially designed for calculating bootstrapped parameters and more efficient.

1.2 e

The standard errors are .1457 for α and .0747 for β

1.3 f

The average predicted value is .842. This is high, but makes sense given that 84% of the latent samples are higher than 0, and therefore assigned to 1 when converted to binary outcomes.

2 Working with Data

2.1 Part 1

The largest difficulty with merging the datasets was creating a unique ID and processing the date variable. Some entries in the date variable from the monthly-data folder were specified in YYYY-DD-M format rather than YYYY-MM-DD format. In order to clean these entries, I wrote a function that would read in a csv from the monthly-data folder, identify if it had the correct month in the months place, and replace the date with the correct date if it did not. I then compiled these cleaned files into one large dataset. By having the proper dates, I was able to create a unique id, the concatenation of the date and firm id, that allowed me to join this dataset with the other datasets.

2.2 Part 2

2.2.1 Comparing Eligible vs Non-Eligible Firms

	Eligible	Number_of_Firms	Revenue_vs_Wages	Num_Employees	Month_Sales	Hospitality	Retail	Fast_Food
1	0	22	585	119	733433	0.09090909	0.2272727	0.6818182
2	1	28	306	81	235667	0.14285714	0.5357143	0.3214286

First, I looked at the characteristics of eligible vs non-eligible firms of similar size at the beginning of the program (Jan 2013). In regression discontinuity design we are interested in how eligibility cutoffs impact the outcomes of otherwise similar firms. Therefore, I choose to focus on the subset of firms with between 60 and 140 employees to approximate comparing somewhat similar firms. I choose this first month to get a general idea of how the firms differed at the beginning of the relevant time period. I choose to look at Revenue/Wages as a proxy for profit, as it roughly adjusts for firm size (though not returns to scale). I also choose to look at sales, one of the main outcome variables of interest, as well as sector breakdown. There do seem to be some key differences between the groups, with Eligible firms seemingly having less sales and profit, and non-eligible firms being more likely to be in the fast food sector. Adjusting the comparison group to be smaller does not change these numbers too much. Note: For all tables, Eligible or Adopt = 1 means firm is eligible or adopted treatment and vice versa.

Since this data has both time and eligibility cut-offs, we can also model this data in the form of difference-in-differences. In other words, we are comparing the difference in the impact of the program between the non-eligible and eligible firms. In order to do this type of analysis, an important assumptions of this model is the parallel trends assumption, which guarantees the quasi-randomness of control and treatment groups. This means that the control and treatment group should exhibit similar trends in outcome variables prior to project implementation. In the figures below, I have plotted how revenue and sales have changed over time for the eligible vs non-eligible groups. Of note, this is the intent to treat group. As you can see, they have fairly similar trends prior to implementation (vertical line). Post-implementation, the slope and/or intercept of the eligible groups change significantly, demonstrating the impact of the program. Note: Ideally I would plot more bins, but the binsreg function did not handle me manually putting in bins well. Also, I tried to recreate these plots for the wage bill using binsreg, but again there were unexpected errors in my code. Therefore, I used the ggplot package for some of these graphs.

2.2.2 Comparing Adopting vs Non-Adopting Firms

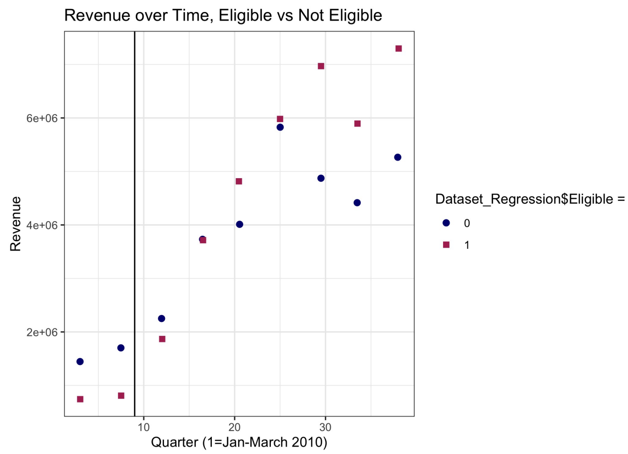
	adopt_t	Number_of_Firms	Revenue_vs_Wages	Num_Employees	Month_Sales	Hospitality	Retail	Fast_Food
1	0	32	510	105	511014	0.1250000	0.3125000	0.5625000
2	1	18	285	85	354542	0.1111111	0.5555556	0.3333333

This comparison is generally the most important comparison, as it looks at the differences between the treatment and control groups. For a difference-in-differences estimate, and also regression discontinuity estimate, to be unbiased the treatment and control groups should satisfy the parallel trends assumption. These figures verify this assumption.

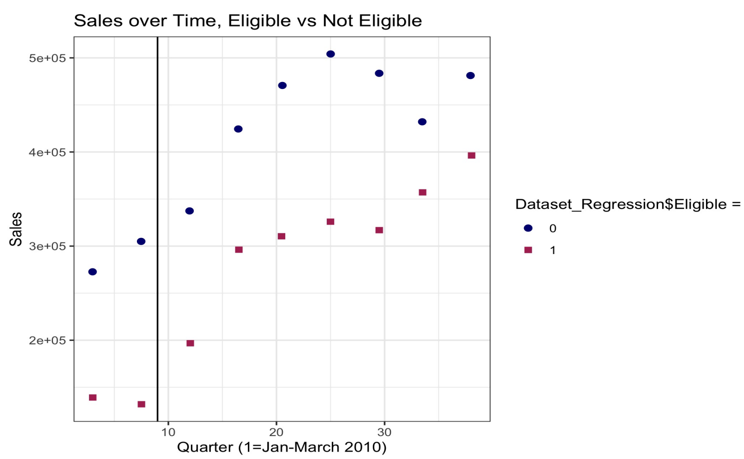
2.2.3 Comparing Adopting vs Non-Adopting Firms among Eligible Firms

	adopt_t	Number_of_Firms	Revenue_vs_Wages	Num_Employees	Month_Sales	Hospitality	Retail	Fast_Food
1	0	11	324	77	268369	0.1818182	0.5454545	0.2727273
2	1	17	295	83	214507	0.1176471	0.5294118	0.3529412

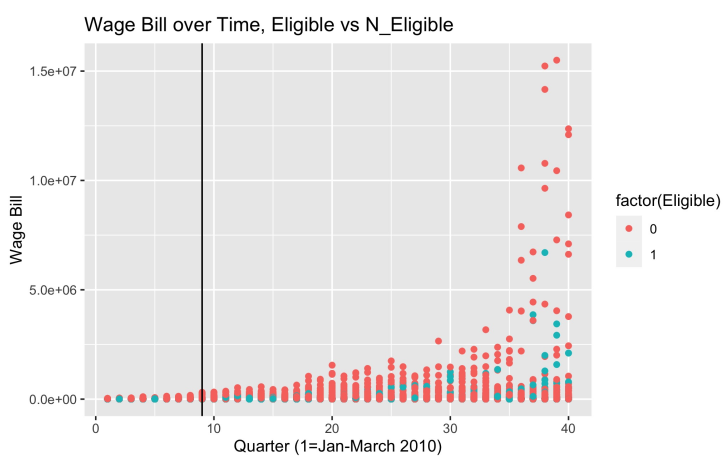
Lastly, this comparison looks at the characteristics of compliers vs non-compliers. This is useful as it allows us to analyze the external validity of our estimates for the impact of treatment. Importantly, we can also understand what populations were more apt to comply with treatment and therefore inform policy-making decisions.



(a) Revenue over Time

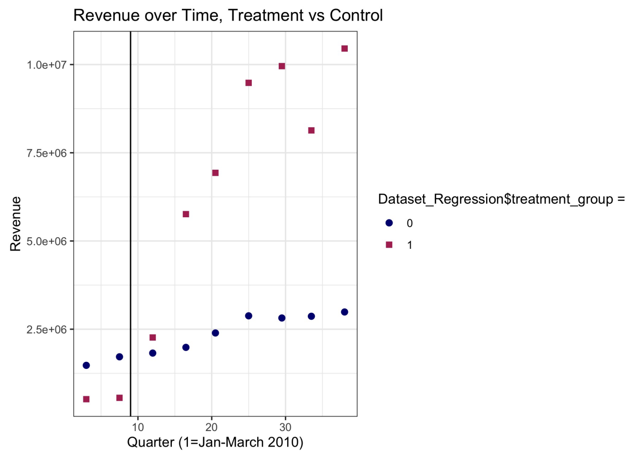


(b) Sales over Time

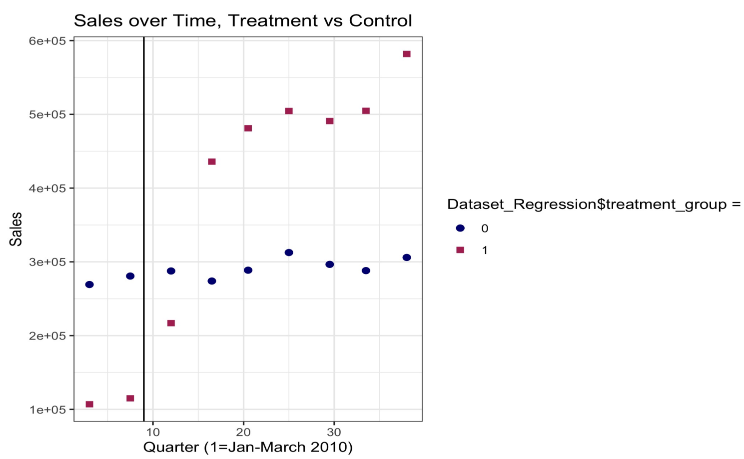


(c) Wages over Time

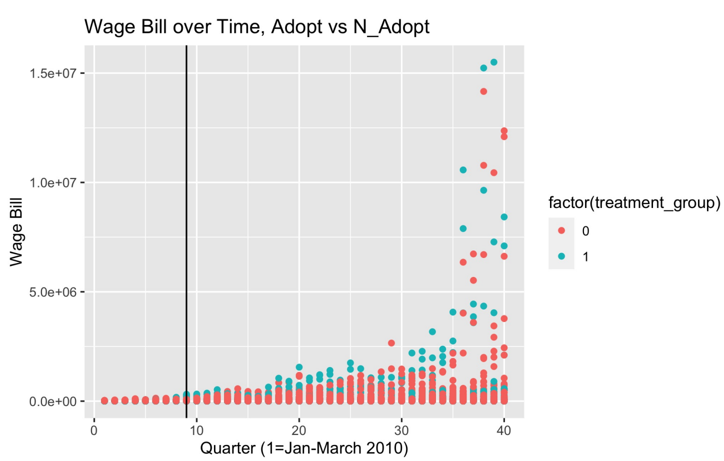
Figure 1: Eligible vs Not Eligible Characteristics over Time



(a) Revenue over Time

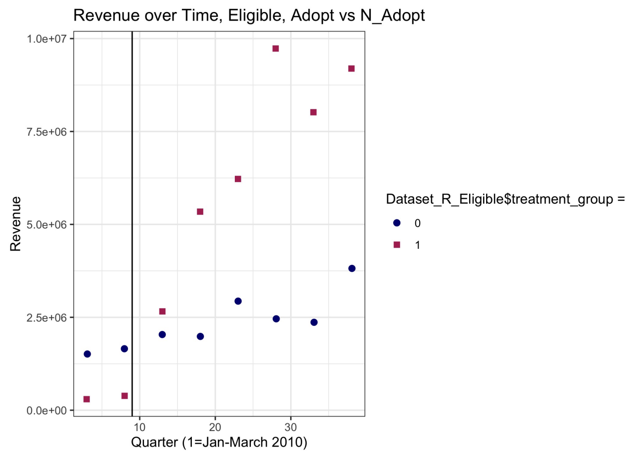


(b) Sales over Time

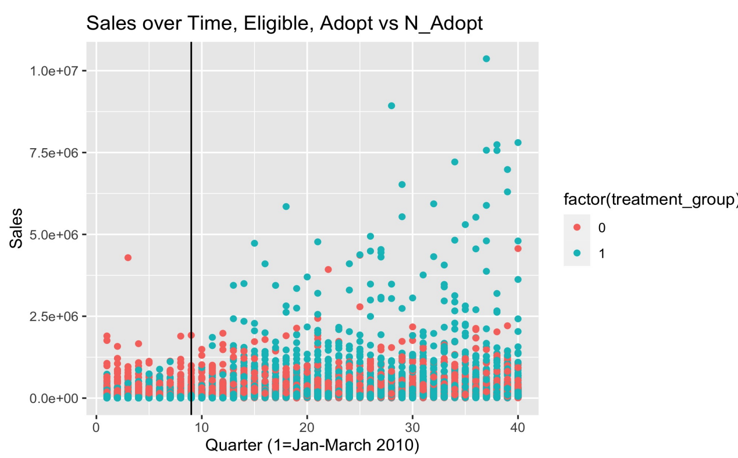


(c) Wages over Time

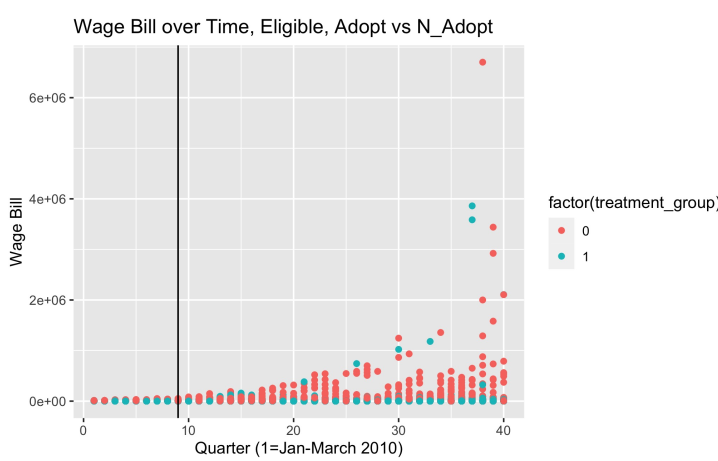
Figure 2: Adopt vs Not Adopt Characteristics over Time



(a) Revenue over Time



(b) Sales over Time



(c) Wages over Time

Figure 3: Eligible Firms, Adopt vs Not Adopt Characteristics over Time

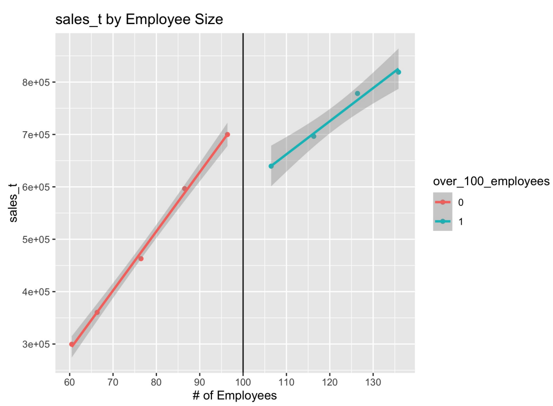
2.3 Part 3

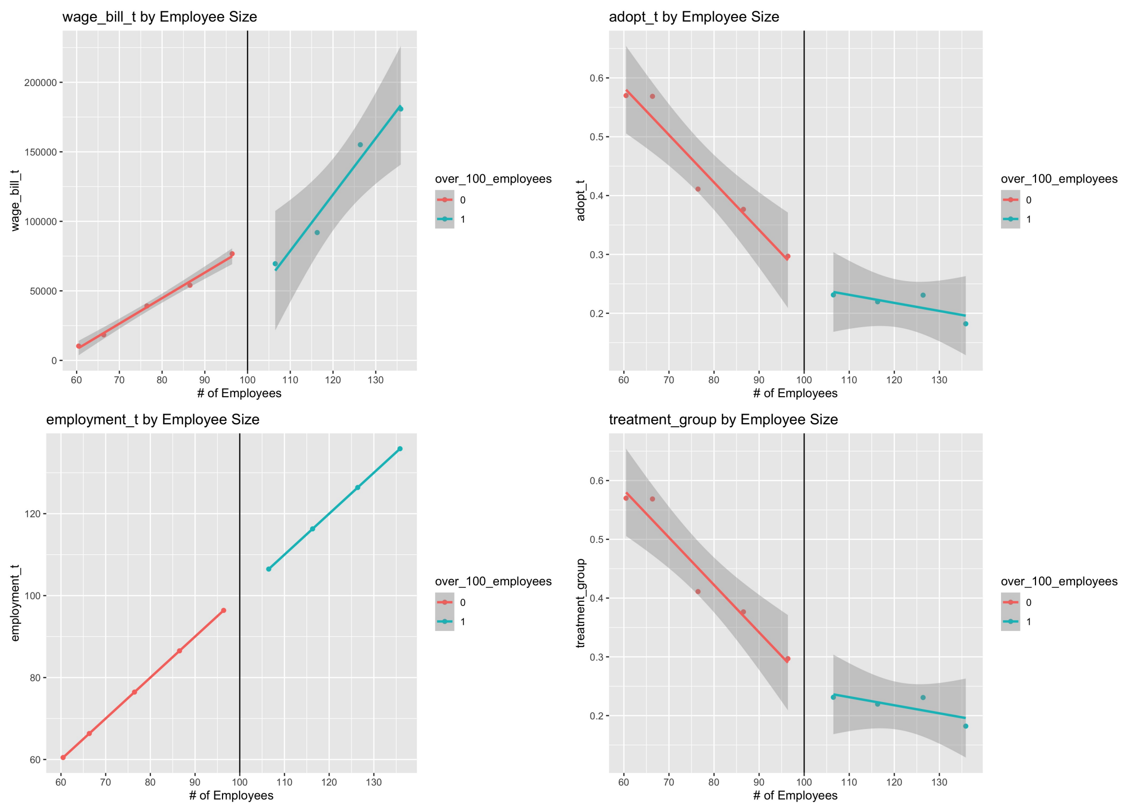
2.4 a

The Regression Discontinuity Design strategy attempts to quantify the causal impact of an intervention by comparing observations close to the treatment cutoff score. In other words, the treatment cutoff can be seen as creating two quasi-random groups close to the cutoff score that only vary in their assignment to treatment. Therefore, comparing the outcomes of these two groups allows for an unbiased estimate of the treatment effect.

2.5 b

These figures generally confirm our hypothesis that the eligibility cut-off generates a discontinuity in outcomes. Importantly, this seems to be a fuzzy discontinuity as being eligible for the program does not necessarily mean a firm will be treated. This is shown by the chart comparing number of employees to adoption of program. Firms that are eligible are more likely to adopt, but the percent that do is far from 100%. Therefore, this discontinuity likely underestimates the true difference in outcomes between treated and non-treated groups. Obviously, the graph of employment vs employment is linear with slope 1 and the graph for treatment group is the same as the graph for adoption.





2.6 c

This model is structured to report the difference in the outcome between non-eligible firms during the treatment period and all other firms. I believe it is structured this way to show if not being eligible for the program negatively impacts firms relative to being eligible for the program. In this way, this is a version of the intent-to-treat effect, rather than the LATE, as the estimate includes non-compliers.Importantly, this model is also a parametric estimator, where instead of looking at only data close to the cut-off, we include a term, in this case lagged employment, to control for the difference in eligibility score.Therefore, in this model, τ is the estimate of the impact of being non-eligible for treatment on the outcome of interest. X is likewise the estimate of the impact of having one more employee in t-1 on the outcome of interest. Lastly, this model specification does not say whether to look at the whole time frame or only the time period after the program start. For the outcome variables sales, revenue, and wages, I looked only at observations after the program started. This is because we are interested in the impact of the treatment on these outcomes, and looking at observations before the treatment occurs would simply weaken our coefficient. For the outcomes adopt and treatment group, I included observations before the treatment began. This is because there is very little change in firms adoption after the treatment has begun, meaning the firm fixed effects would "capture" most of this variable. Including observations before allows us to see how being eligible for treatment (or not) influences ones chances of adoption. Of note, the last outcome variable, treatment group, is expected to be trivial as it stays constant across firms, and is therefore captured by firm fixed effects.

2.7 d

Clearly, this program positively impacted businesses. Compared to eligible firms, and controlling for size and firm and time fixed effects, non-eligible firms had, on average, 210,000 fewer monthly sales, 2.9 million less revenue, and 39,450 higher monthly wage bill. All of these coefficients had a p value of .01 or lower. As expected, the coefficient on lagged employment was positive for the sales and revenue outcomes. Surprisingly, the coefficient on lagged employment when the outcome was monthly wage and employment is not significant. It is possible that firm and time fixed effects capture most of the information on wages and employment (as firms tend to have similar employee size over time), rendering lagged employment non-significant. Lastly, the coefficient on non-eligibility is negative when the outcome is adoption, as expected, and is 0 when the outcome is treatment, as treatment is captured entirely by firm fixed effects. Of note, since employment is non-binary,but adoption is binary, the coefficient on lagged employment in this model cannot be interpreted.

Table 1: Change after Program Implementation in Business Outcomes of Non-Eligible Firms

	Dependent variable:					
	Monthly Sales	Monthly Revenue	Monthly Wage Bill	Monthly Employees (at time=t)	Adoption of Intervention	Treatment Assignment
	(1)	(2)	(3)	(4)	(5)	(6)
Not Eligible x Post Implementation Dummy	−209,382.300***	−2,915,327.000***	39,446.560***	58.279***	−0.209***	−0.000***
	(17,843.840)	(411,311.400)	(14,217.540)	(0.881)	(0.006)	(0.000)
Employment in t-1	9,344.586***	110,007.800***	−116.212	−0.008	−0.0001	−0.000***
	(179.863)	(3,978.322)	(143.311)	(0.009)	(0.0001)	(0.000)
Observations	8,400	11,900	8,400	8,400	11,900	11,900
R^2	0.492	0.271	0.295	0.757	0.786	1.000

Note: *p<0.1; **p<0.05; ***p<0.01

2.8 e

As mentioned before, this model only captured the average intent-to-treat effect. This is because some eligible firms did not receive treatment and some non-eligible firms did (non-compliers). Therefore, our coefficients do not capture the impact of the treatment (adopting the intervention) in an unbiased manner. To capture this LATE, one could conduct a difference-in-differences with a dummy=1 if adopt-t = 1. Another issue with the model is the specification of the lagged employment term. By not including squared or cubic employment terms, the model does not capture changes in firm outcomes resulting from returns to scale, making our estimate less accurate.