Final Exam

Foundations of Econometrics, Part II

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Instructions

This exam consists of 6 pages, including this one. Please make sure that you have all of them! If you did not receive all pages, let us know immediately. Ex-post claims about this will not be considered.
The duration of the Part I exam is **1 hour**. The exam consists of several questions with limited space to answer them. You can use the space at the end of the instructions as scratch paper (I won't read what you write there), no other paper will be distributed. **You must hand in all pages of the exam, even if you don't answer the questions in those pages.** Use the provided space only, and **do not write on the page margins!** Don't try to write too much. I expect answers that do fit in that space.

Grading system: All questions are worth the same. I will grade the exam up to 120%, but the maximum grade that you can obtain is 100%. For example, if you answer all questions well, or, in general, questions that give you between 100% and 120% of the points, you would obtain a grade of 10/10. If you answer questions well enough to amount less than 100%, then you would get the grade that corresponds with the % of correct answers that you provided (e.g. if you get 90%, then your grade is 9/10). In other words, you need to answer well 5/12 instead of 6/12 to pass and I am giving you points as bonus. This system is equivalent to having bonus questions that are worth up to 20%.

Make sure you identify and solve **easy questions first** before you attack harder questions. **Time is finite**: make sure to **distribute your time** wisely (e.g. it is not a good strategy to devote one hour on a single regular question because, even if you do it perfectly, it is only worth a small percentage of the final grade). The optimal strategy is to **answer all questions** (better than answering fewer questions more thoroughly addressed). Also, use clear writing and discuss what you are doing (especially when you use mathematical expressions). There will be no credit for correct answers if I cannot follow your arguments, but potentially partial credit for incorrect answers with a sensible approach. **Read the ques-**

tions carefully and answer to all what you are asked (no more than that, no less than that). **Please do NOT use red pen**. No phones, calculators, or auxiliary materials of any sort are allowed. **Respect the no-cheating code of honor**, we take cheating very seriously!

Good luck!

DON'T USE THIS SPACE FOR ANSWERS!

Student ID:..... **Program:**.....

1) The following table is included in the paper "Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment" by Paul Gertler, published in the *American Economic Review* in May 2004:

TABLE 1—PRE-INTERVENTION DESCRIPTIVE STATISTICS FOR THE MORBIDITY SAMPLE OF CHILDREN AGE 0–35 MONTHS AT BASELINE

Variable	Treatment	Control	p value for difference
Child was ill in last 4 weeks (=1)	0.330	0.323	0.771
Age	1.625	1.612	0.914
Male (=1)	0.511	0.491	0.091
Father's years of education	3.803	3.840	0.980
Mother's years of education	3.495	3.829	0.062
Father speaks Spanish (=1)	0.942	0.929	0.276
Mother speaks Spanish (=1)	0.935	0.917	0.443
Own house (=1)	0.923	0.917	0.465
House has electricity (=1)	0.644	0.711	0.091
Hectares of land owned	0.809	0.791	0.553
Male daily wage rate (pesos)	30.483	31.219	0.370
Female daily wage rate (pesos)	27.258	27.844	0.493
Sample size:	4,519	3,306	

Notes: This table reports descriptive statistics for the sample of children age 0–35 months at baseline before the intervention. The p values in the third column are for the test of the hypothesis that the means of the treatment and control groups are equal and are adjusted for inter-cluster correlation at the village level.

The table represents averages of different covariates for treated and control samples. The treatment is providing the conditional cash transfers implemented in the PROGRESA policy, and it was randomized across villages. The outcome is child's height. Given this information, do you think that comparing treated and control average heights provides a good estimate for the average treatment effect? Do you need to control by any regressors? Why?

2) Rita Almeida and Marta Faria, in their paper "The wage returns to on-the-job training: evidence from matched employer-employee data" published in the *IZA Journal of Labor and Development* in 2014, are interested in analyzing the effect of of on-the-job training on different outcomes such as worker productivity and wages. Whether a worker receives training is observable, but possibly not random.

For this reason, they use propensity score matching. In particular, they estimate their propensity score based on several worker (including education, gender, age, tenure with the firm, potential experience, marital status, occupation) and firm characteristics (including firm size, age, export intensity, foreign ownership, education of the workforce, degree of technological adoption). Are you convinced by this approach? Why?

3) Amazon is interested in estimating the effect of advertisement on purchases and they hire us for that purpose. They give us weekly data on sales and advertisement for that purpose. Our first approach is to classify weeks into high advertisement or not based on the number of ads displayed on that week. We call high advertisement weeks treated and low advertisement weeks controls. Do you expect the comparison of treated and control weeks to provide a valid causal effect? Why? Explain briefly, connecting the empirical context described and the assumptions discussed in the course.

4) Continuing with the Amazon example, we have now access to Amazon's app. Through the app, we can randomly vary the amount of ads that individuals receive displayed on their app, and we observe whether or not individuals watch the ads that are displayed in their apps. The more ads are displayed, the more likely that the individual sees the advertisement at least once (our treatment variable). We implement two different treatments. In the first one we randomly classify customers in two groups, and we double the number of ads displayed to the group

assigned to treatment. In the second one, we display a different (random) amount of ads to each customer. Briefly explain the different treatment effects that can be identified using these two experiments, how, and why.

5) Building on "Do corporate income tax cuts decrease labor share? Regression discontinuity evidence from China" by Bing Li and co-authors, published in *Journal of Development Economics* in May 2021, we are interested in analyzing how corporate income tax cuts decrease the firms' employment to output ratio. The following table summarizes the Chinese corporate tax system:

Table 1. China's corporate tax rate system, 2010–2013.

Group	Tax rate (%)	Firms	Main qualification requirements
1	25	Normal rate	Firms with no tax credits
2	20	Small- and micro-enterprises tax credit	For service firms: annual taxable income≤300,000, employees≤80, and asset≤100,000,000
3	15	High-technology firms tax credit	R&D intensity≥6% if sales<50,000,000; R&D intensity≥4% if sales≥50,000,000 & sales <200,000,000; R&D intensity≥3% if sales≥ 200,000,000
4	15	Offshore service outsourcing firms	Offshore service outsourcing revenue/total revenue≥ 50% in 21 cities
5	15	Western development tax credit	Located in China's western regions, and main business revenue from government-supported industry/revenue≥70%
6	0 or 12.5	Software and integrated circuit firm tax credit	For start-ups, exemption from tax for two years from the first profit-making year and a preferential tax rate of 12.5% for the subsequent three years
7	Miscellaneous	–	–

Notes: The monetary unit is RMB Yuan. In group 2, the threshold for manufacturing firms is different. In group 3, R&D intensity is defined as the ratio of R&D input to total revenue. There are several special cases not included in groups 1–6, which we label as the miscellaneous group 7.

We want to implement a regression discontinuity design based on group 4 from the previous table, that is, focusing on a sample of the selected 21 cities, the running variable is the offshore service outsourcing (OSO) revenue divided by total revenue. Would you implement a sharp or a fuzzy design? Why? What treatment effect will you identify?

6) Building on the 2012 *American Economic Review* paper by Petra Moser and Alessandra Voena entitled "Compulsory Licensing: Evidence from the Trading with the Enemy Act", we analyze the effect of compulsory licensing on domestic invention. During World War I, the US enacted TWEA, an act that allowed US firms to violate enemy-owned patents if they contributed to war effort. To measure the effects of licensing, they compare, before and after, the number of patents by domestic inventors across subclasses of (organic chemistry) technologies that were diferentially affected by TWEA. Chemical inventions in all these subclasses, however, were affected by increase in tariff barriers. Does a difference-in-differences approach (exposed to TWEA vs not) provide a consistent estimate in this case? Why? If the UK was exposed to the same tariff increase but did not implement TWEA, can you suggest a method to obtain a valid causal effect?