Pre-analysis plan for Reference Dependent or Independent Labour Supply among Street Paper Sellers

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Abstract

We investigate whether labour suppliers behave as though they have reference-dependent preferences. The present pre-analysis plan discusses how the variables are coded, how we will deal with missing values, and the specification of the estimation equations. We also conduct a power analysis.

Introduction

Dynamic labour supply models suggest that work hours should increase in response to temporary wage increases. This occurs because workers shift labour and leisure over time, choosing to work more when wages are higher and to enjoy more leisure when the opportunity cost—the wage they forego—is lower. However, a permanent wage increase will trigger an offsetting income effect, as the rise in lifetime wealth allows workers to afford more leisure (Farber, 2015). We investigate whether labour suppliers behave according to the dynamic labour supply models or if they have reference-dependent preferences to attempt to reach specific income targets, which aligns with the predictions of prospect theory (Kahneman and Tversky, 1979). Our participants are sellers of the Swedish street paper *Situation Stockholm*. Our main focus is how the workers respond to transitory income shocks.

The intervention

Our group of participants is sellers of the Swedish street paper *Situation Stockholm* (henceforth "*Situation Sthlm*" or "the paper"). Sellers buy copies from the paper's office for 40 SEK each and sell them for 80 SEK. The paper is published monthly, except for one summer month. Sellers who have unsold copies when a new issue comes out may return the copies to the paper's office and get 40 SEK per copy back.

In an intervention directed at sellers of *Situation Sthlm*, we reward sellers with ten SEK extra per copy they sell. To eliminate the possibility of cheating (e.g., sales between sellers where the issues can later be sold again), only sales made through an electronic payment method known as Swish qualify for the reward. Swish sales already comprise the vast majority of all sales, and enable us to verify that the buyers are not other sellers, as well as to exclude buyers of multiple papers of the same issue. Sellers know these rules and should expect their sales behaviour to be monitored. Participating sellers also sign a contract that promises not to cheat. Participants do not know that they are in a study, having been told that the editorial office of the paper has simply received a donation.

A coin flip randomizes sellers. In concrete terms, sellers looking to buy copies of *Situation Sthlm* are asked if they would like to flip a coin to determine if they will receive a ten-crown reward per copy sold. through Swish. If the coin comes up heads, he or she gets the reward. Otherwise, he or she gets the reward the next issue.

Data

We use data from the editorial office of *Situation Sthlm* on all sales by individual sellers. For electronic sales, we have access to real-time sales data; for payments in cash, we will know that the issue was sold in the period between when the seller picked it up and when s/he went back to potentially return

unsold magazines (a period that may cover several days or weeks). In addition to the sales data, we also have access to the age and gender of the sellers.

Coding of variables

In this section, we present the variables that we will use in the analysis. We start with the outcome variables and continue with the covariates and the variables used to study heterogeneous effects.

Main outcome

Total number of sold papers of the September 25 issue: Sales can be made in cash or through the electronic system Swish. On average, about 80 percent of sales are made via Swish. Our primary outcome measure is the total number of sold papers of the September 25 issue, including cash and Swish sales.

Other exploratory outcomes

Log of Total number of sold papers of the September 25 issue

Total number of sold papers of the September 25 issue via the electronic Swish system

Total number of sold papers of the September 25 issue via cash

Hours worked selling the September 25 issue: We can approximate the daily number of hours worked by our measure of electronic sales since we know the exact time of day for electronic sales. This measure of working hours is not an exact measure of working time, and the intervention might affect the share of sales via the electronic system. For sellers with a high share of electronic sales our measure will be better and we will consider this outcome variable for the full sample as well as for a restricted sample of sellers with a large share of electronic sales during the previous three months. We will also restrict the hours-worked analysis to days with at least two electronic sales.

Number of days worked selling the September 25 issue: We can approximate the number of days worked via electronic sales data, similar to how we estimate hours worked.

Control variables

Our control variables are administrative data given us by the editorial office of the paper and are all measured before the intervention starts.

12 lagged issues of past number of sold papers.

Female: Equal to one if the seller is a woman.

Age: Continuous variable of age.

Variables to measure heterogeneity

Total number of sold papers during the last 12 months

Total number of sold papers during the last month

High seller: A dummy equal to one for sellers who have sold more than the median number of the previous three issues of the paper.

Age

Estimation strategy

We first regress *Total sales of the September 25 issue* on treatment status; i.e. a dummy variable equal to one if the individual was randomly assigned the lower price, and a set of controls, $\boldsymbol{X}_i$:

(1) $$Y_i = \alpha + \beta_1 T_i + \beta_2 \boldsymbol{X}_i + \varepsilon_i$$

The vector **X** comprises the covariates listed above (Lags of the dependent variable, Female, and Age). The regression is estimated with OLS and robust standard errors.

To test for balance, we will regress our main treatment variable on the control variables described above both individually and together. We will judge whether the randomization worked by conducting an F-test of whether the control variables jointly predict treatment status.

We will present results from estimations without control variables. However, our main estimation will be one where optimal controls are chosen from the total list of controls using a post-double LASSO selection approach by Belloni, Chernozhukov, and Hansen (2014). The LASSO selection approach selects those variables that are correlated with both treatment and the outcomes, which may improve precision in the estimates (especially including the lagged values of sales), and it also helps to correct for imbalances across groups. We will also present the results using an event plot where we estimate equation (1) for each period. We will then test for balance for each individual lag and for the lags jointly.

If we have missing values on explanatory variables, we will code the variables as zero and include dummy variables controlling for missing status so that we do not lose observations. To make the models fully saturated, we partition the covariate space and add control variables as indicator variables rather than using their multi-valued codes (Athey and Imbens, 2017). If cells are too small, with less than 5 percent of the observations, adjacent cells are combined. When using interaction terms and in tests of balance, we will retain the continuous coding of the variables.

Exploratory analyses

Exploratory heterogeneity analyses

We will investigate whether sellers differ in their labour supply behaviour. Are some sellers reference-dependent and others optimizers? Following findings from previous research (Farber, 2015), there is evidence of more reference dependence among the inexperienced and less among the experienced. We will explore this issue by comparing experienced sellers to inexperienced ones using the variables described in "Variables to measure heterogeneity".

Exploratory analysis using the October issue

The control group for the September issue will be treated in October and we will test whether we observe similar effects for them. We do not expect that there are long run effects of the lower price for the September issue. Under that assumption, we can also use the early treated group as a comparison.

Power

The sample will contain around 150 individuals, half assigned to treatment and half to control, assuming no or almost no sellers opt out of the intervention. We know from data from previous years that the lags have very strong predictive power for the outcome, amounting to an R-square of 0.71, so we consider that. At the conventional significance level of 0.05, an R-square of 0.7, and a power of 0.8, our sample size would allow for a minimum detectable effect of 0.25 standard deviations. Assuming only an R-squared of the controls of 0.5 and a sample of 130 individuals, the minimum detectable effect will be 0.35.

Archive

The pre-analysis plan is archived before any post-intervention data are collected. We archive it at the registry for randomized controlled trials in economics held by The American Economic Association: https://www.socialscienceregistry.org/ on September 16, 2024.

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