Peer Effects in the Adoption of a New Employment Subsidy for Vulnerable Youths

By Claudio Mora-García and Tomás Rau*

Draft: October 29, 2016

Many social programs have low take-up rates and little is know about the factors determining this regularity. This paper studies the effects of peers on the adoption of a Youth Employment Subsidy for vulnerable youths in Chile. We focus on the effects of high school classmates and coworkers' adoption on one's adoption. Identification comes from a discontinuity in the subsidy assignment rule inducing exogenous variation in a neighborhood around the worker's wage eligibility cutoff. Using a comprehensive set of administrative records that include high school and matched employer-employee data, we find that coworkers strongly influence one's adoption of the subsidy while high school classmates do not. Peer effects are larger among older youths with about five years of working experience and within big firms. We also find that peer effects decrease with time, but remain significant, one year after program implementation. These results suggest that information diffusion is one channel explaining adoption in the short run, but more research is needed to understand steady state take-up level.

I. Introduction

Many social programs face low take-up rates and little is known about the channels explaining this regularity. In the U.S. take-up varies a great deal across means and non-means tested programs (Currie, 2004). While the State Children's Health Insurance Program (SCHIP) had a take-up rate of 8.1% to 14% (LoSasso and Buchmueller, 2002), the Child Care Subsidy Programs had a take-up rate of 15% (Administration for Children and Families, 1999). On the other hand, the Unemployment Insurance had a take-up rate of 83% (between 1980 and 1982), while the Medicaid had a take-up rate of 96% during 2002[1]. It has been

* Corresponding author: Tomás Rau, Pontificia Universidad Católica de Chile, Vicuña Mackenna 4860, Santiago, RM; phone, (56-2) 354-4326; fax, (56-2) 5532377; email, trau@uc.cl. Claudio Alberto Mora, Pontificia Universidad Javeriana; email,claudio_mora@javeriana.edu.co. We would like to thank Jeanne Lafortune, Francisco Gallego, and seminar participants at Pontificia Universidad Católica de Chile and SECHI (Viña del Mar) for valuable comments. We also thank Cristián Crespo and Nicolas Andrade from the Miniterio de Desarrollo Social, and Andrea López from SENCE, for facilitating data access. The usual disclaimer applies.

[1]Something similar occurs in the United Kingdom, where the Working Families' Tax Credit has a take-up rate of 72% (Currie, 2004), and the Income Support has a take-up of 64% (Cuclos, 1995). In Norway, Dahl, Loken and Mogstad (2014) show that the take-up of governmental paid paternity leave for eligible fathers jumped from 3% to approximately 35% in 1993 after the implementation of a reform promoting gender equality.

reported that the differences in take-up rates are due to the high costs of learning, application costs and stigma (Moffit, 1983; Blasco and Fontaine, 2012; Dahan and Nisan, 2011). Also, small benefits from applying (Anderson and Meyer, 1997; Riphahan, 2001), and behavioral issues on the profile of the payments (O'Donoghue and Rabin, 1999), have been indicated among other reasons.

This paper analyzes an alternative channel explaining the take-up of social programs which is the impact of peers in adoption through information sharing (Borjas and Hilton, 1996; Bertrand, Luttmer and Mullainathan, 2000; Duflo and Saez, 2001; Aizer and Currie, 2004; Dahl, Loken and Mogstad, 2014; Carneiro, Galasso, and Ginja, 2014; Figlio, Hamersma and Roth, 2015). We do this in the context of an employment subsidy in Chile targeted to vulnerable youths.

The Youth Employment Subsidy (YES) consists of a twofold monetary incentive for employed youths and their employers. It was implemented in July 2009 and is currently an ongoing program. For low-income workers, the YES subsidy can represent as much as four monthly wages in a calendar year (20%), and for an average worker about one additional monthly wage. Altogether, this social program has a very straightforward and private application process, that is done on the internet at any time and place. This further decreases the application cost, by providing an easy and accessible way to apply.[2] However, despite the low application costs and advertising campaigns carried out at the beginning of the program implementation, the percentage of eligible workers that are taking up the program has remained below 20%.

This paper estimates the causal impact of peers' adoption decision on one's decision. We explore the hypothesis that during the first months of YES implementation informational barriers have a significant role in explaining the low take-up rate of the program. Thus, peers' experience is a potential channel to overcome low adoption rates through information sharing that would translate into adoption. Hence, peer effects could be substantial, specially at the beginning of the program.

This article exploits a unique database that results from merging four different administrative records using a common identification number. Together, they provide information on individual socioeconomic characteristics of disadvantaged youth between 18 and 25 years, their employment history as dependent workers hired at private firms, and their history of school enrollment by grade and year between 2002 and 2012. It also contains a record of the monthly and annual YES payments made.

This rich dataset allows us to fix the networks several months before the introduction of YES, thus alleviating concerns about endogenous group membership. Hence, any changes in

[2]According to SUBTEL (2014), in 2009, 31.1% of Chilean households had any access to the internet from the house; during 2014, this amount has increased to 61.6%. According to CASEN 2011, almost 80% of youth Chileans between 18 and 25 years old had any access to the internet, and the average among the lower quintiles of income was around 65%.

group formation will be arguably exogenous to the introduction of YES. We then track peer effects among peers who were within one's network before the implementation of the YES program.

The dataset allows us to have access to different kind of networks. In particular, we focus on classmates and coworkers. To the best of our knowledge, this is the first paper in the take-up literature to study the effect of classmates on the adoption of a labor social program[3].We also analyze coworker networks because the literature has provided extensive evidence on the importance of them on several decisions. For example, at referral based models coworkers provide information about available jobs and unknown productivity of workers (Glitz, 2013); and Dahl, Loken, and Mogstad (2014) find important peer effects among coworkers in social program participation. However, we are not able to see other networks (for example, Dahl, Loken and Mogstad (2014) study family members, Aizer and Currie (2004) study neighbors at the zip code level, and Duflo and Saez (2001) study university departments).

The identification strategy exploits a discontinuity in the eligibility rule of the program that provides random variation around the worker's wage cutoff. In particular, eligible worker's monthly wage has to be below CLP $360,000. Hence, we use as an instrumental variable for network's adoption the fraction of peers adopting the YES in a very small window around the cutoff, following a similar strategy implemented by Dahl, Loken, and Mogstad (2014). Using a McCrary (2008) test for different dates and measures of earnings, we show that there is no manipulation in wages as may occur in an RD design. Hence the distribution of wages is smooth around the threshold. Moreover, most of the observable and pre-determined characteristics of the peers (age, sex, years of education, the size of networks, among others) do not change near the cutoff, except for the fraction of peers that are adopting the YES that is our instrumental variable for network's adoption. So the instrument is likely to be orthogonal to several variables and quasi-randomly distributed among the population, and hence independent of unobservable shocks at the group level (correlated unobservables).

As a result, this rule induced quasi-experimental variation that comes close to an ideal experiment. So we implement a "partial population" approach (Moffitt, 2001; Dahl et al., 2014), where some peers within a group were quasi-randomly not given the opportunity to apply to YES while leaving all other peers within the same group available to apply. To do so, we start by selecting a subsample such that all groups have at least one peer whose wage is inside a narrow interval around the cutoff (we call this interval the window). We then define the fraction of eligible peers whose wage is inside and at the left of this window and use this as an instrumental variable for the endogenous peer effect. The idea is that for a

[3]Classmates may be particularly important since schools have shown to be highly relevant in determining labor market outcomes among individuals from a privileged background in Chile. Zimmerman (2015) finds that the admission to an elite program raises the number of leadership positions at firms that students hold by 50%, but gains are larger for students who attended one of nine elite private high schools and near zero for students who did not. In the context of our work, we find that classmates are also crucial among vulnerable youths in transmitting information about social programs.

given person, having more peers with wages just below the cutoff and inside that window is a quasi-random event because admissibility to YES was quasi-randomly distributed among them.

The first-stage results show that networks with a higher fraction of peers at the left side of the cutoff inside this small window were more likely to have adopted YES during the first month of implementation. For example, having a 10 pp higher fraction of coworkers to the left of the cutoff increased the fraction of coworkers with YES by 0.48 pp; and having a 10 pp higher fraction of classmates to the left of the window increased the fraction of classmates with YES by 0.32 pp (both significant at the 1%).

The reduced form estimates show that having a 10 pp higher share of coworkers with a wage below the threshold, and inside the window, increased the individual probability of adopting YES by 0.23 pp (significant at the 1% level). However, having a 10 pp a higher fraction of classmates with a wage below the cutoff, and inside the window, decreased the individual probability of adopting YES by -0.008 pp but not significant. Hence, a 10 pp a higher fraction of coworkers with YES implies a 4.8 pp higher individual probability of adopting YES (significant at the 1%); while the classmates peer effect is statistically nonsignificant from zero. We conclude that coworkers played a major role in determining participation in YES program during the early months, but the classmates were not relevant.

In analyzing the mechanisms that drive the peer effects, the second set of results show that an informational channel is a good candidate to explain the results. First, we look at peer effects during 2010, one year after the beginning of the program, finding that coworkers peer effects remain significant but decay over time, perhaps as more information is available in the market. Also, peer effects are more important among older youths over 21 years old, who have more working experience and the interaction with other coworkers are more likely to be stronger compared to younger youths. Peer effects are also higher among admissible peers within larger firms, and they are strong regardless of the school characteristics (either from vocational high school or the size of the school).

This paper makes some contributions to the current state of knowledge. First, this is the first article on program participation (to the best of our knowledge) studying a welfare program during the early stages of its implementation. Many papers have examined the take-up of social programs and have provided explanations for the low take-up rates by studying social programs that had been undergoing for several years (Bertrand, Luttmer and Mullainathan (2000), Duflo and Saez (2001), Aizer and Currie (2004), Dahl, Loken and Mogstad (2014), Carneiro, Galasso, and Ginja (2014), among others). Though there are many potential explanations for the low take-up rates, this literature has not been able to study social programs in the implementation phase, and peer effects during this period could behave differently due to information sharing. Second, our paper has the advantage of using a database with information on two kinds of networks –classmates and coworkers– that allow us to evaluate the importance of peer effects through different networks that may differ on the information sharing process. And third, this article is one of the first to track the evolution of peer effects for several months afterward, until September 2011. The previous literature has documented peer effects at one point in time.[4]

The paper is organized as follows. Section II describes the YES program and section III explains the identification strategy and estimation methods. Section IV describes the data used and section V analyzes the validity of the instruments. Section VI shows the results and section VII concludes.

II. The YES subsidy

The Youth Employment Subsidy (YES) is targeted to workers in the 18-24 age range belonging to the poorest fourth decile of the population as measured by the FPS proxy means test score. It considers wage bonuses for employees and employers, however, the application processes are independent, and workers and employers must apply separately through an online system. The implementation and administration of YES are in charge of the Service of Training and Employment (SENCE) from the Ministry of Labor and Social Welfare and, additionally, involves the Social Security Institute (IPS) and the Internal Revenue Service.

The program was launched in July 2009 with an advertising campaign supplemented by field visits of the Minister and the entire team of the Ministry of Labor and Social Welfare, some of them also had the participation of the Finance Minister. Advertising was placed on television, national and regional radio, national and regional newspapers, subway, public transportation in Santiago and their bus stops (Huneeus, 2010).

Eligibility Criteria. To be considered eligible for the YES, a worker must meet the following criteria:

1) to be between 18 and 24 years old;

2) to be a member of a family group belonging to the poorest fourth decile of the population, which is equivalent to having a vulnerability score equal or less than 11,734 points in the FPS;

3) to declare a gross annual income less than CLP$4,320,000 (US$8,640) [5] in the calendar year in which the benefit is claimed (or a gross monthly income less than CLP$360,000 (US$720) for those who request provisional monthly payments);

[4] In Chile, Carneiro, Galasso, and Ginja (2014) showed that information sharing is important in defining social program participation. They find that *Chile Solidario* participants, a program that provides information on social programs, increased their take-up of a family allowance for poor children (the *Subsidio Unico Familiar*, SUF) by 11%, relative to an average take-up of 65% among comparable non-participants. However, Carneiro, Galasso, and Ginja (2014) do not analyze how peer effects influence program participation.

[5] According to the law, all quantities in Chilean pesos are adjusted each year for the annual variation experimented by the Chilean Consumer Price Index.

4) to not be working in a state institution or a company with a state contribution higher than 50%[6]; and

5) to have social security contributions paid up to date.[7]

The benefit is maintained as long as the recipient continues to meet the above criteria. Beginning in April 2011, a worker who is 21 years or older must have obtained a high school diploma to access or keep getting the subsidy.

Application. The application process is simple and can be carried out at any time from any place because applications are made electronically on the internet. Workers and employers who want to apply must follow independent processes that require independent information. They must log into the website www.subsidioempleojoven.cl. Once entered the worker's ID number, it is immediately reported whether or not the requirements are met. In case the answer is favorable, a registration form can be filled and submitted. After submission, SENCE has a deadline of up to 90 days to respond the result of the application. The application status can be verified at any time by accessing the website of YES. If the person believes the system is displaying incorrect information, the person can contact SENCE from the same webpage.[8]

Paperwork. This application process means that paperwork is reduced to the minimum. Moreover, several procedures are simplified to the minimum, for example, it is not necessary to submit documents establishing the employment relationship of the worker with his employer because the information is verified internally. Nor is it necessary to submit documentation in case of medical license because that information is internally verified by the Social Security Institute (IPS).

Paperwork is necessary in very specific cases, but in all of them, the required documentation may be electronically uploaded to the website. For example, when the worker wants to appeal for suspended payments, or when there is a suspension of payment of the benefit due to unpaid health contributions. Appeals due to suspended payments can be entered electronically from the YES website.

Payments. If the application is successful, the bonus will begin to be paid within a maximum period of 90 days after filing the application. The benefit will be accrued from the

[6]The AFC database only includes private firms and not public enterprises. In Chile, firms, where the State has a stake exceeding 50%, are defined as public enterprises. Public corporations also contain companies created by law, those where the State is the owner or those where the State appoints most members of its Board. According to the National Securities and Insurance Agency (SVS), there are only 34 public enterprises whose names can be seen at http://www.svs.cl/educa/600/w3-propertyvalue-1066.html, and they employed a total of 40,239 persons during 2014, of all ages and socioeconomic conditions. Moreover, according to the Third Chilean Longitudinal Firm Survey carried out during 2013, only 22 of the 7,267 surveyed firms (0.3%) had a state contributing higher than 50%. For this reason, the empirical strategy will not take into consideration this point.

[7]Fortunately, workers that appear in the AFC database are only workers whose social security contributions are up to date.

[8]In the event that the website refuses the application option on the grounds of an FPS higher than the cut-off score, the worker can approach his municipality and ask for her FPS to be recalculated. However, this does not guarantee that the FPS score will be updated.

first day of the month following the date of the submission of the application.

Payment to the employee may be annual, being carried out during the second half of the next calendar year on which wages and employment income were earned. However, the dependent worker may opt for monthly payments upon submitting the application and can change his choice only once during the year.[9] In both cases, the payments are made on the medium indicated by the worker at the time of completing the application, either in cash or a bank deposit account.

For some workers, having YES means receiving more than an extra monthly wage. Table 1 shows the amount of the subsidy to which the worker is entitled to in a phase-in, plateau, and phase-out manner. Panel A shows, first, that for workers whose annual gross income is equal to or less than $1,920,000, the subsidy amounts to 20% of the sum of wages and taxable income. Second, for workers whose annual gross income is greater than $1,920,000, but not exceeding $2,400,000, the benefit will amount to $384,000 (20% of $1,920,000). Third, for workers whose annual gross income is greater than $2,400,000 but less than $4,320,000, the benefit will amount to $384,000 less the 20% of the difference between the sum of wages and annual taxable income and $2,400,000. If the worker opted for monthly payments, then the

Table 1—: Payment scheme for workers, by annual gross income and monthly gross income.

Panel A Annual Gross Income (AI)	Subsidy (YES)
AI$\leq$ \$1,920	$0.2 \times (AI)$
\$1,920<AI$\leq$\$2,400	\$384
\$2,400<AI$\leq$\$4,320	\$384-$0.2\times(AI$-\$2,400)

Panel B: Monthly Gross Income (MI)	Subsidy (YES)
MI$\leq$\$160	$0.2 \times (MI)$
\$160<MI$\leq$\$200	\$32
\$200<MI$\leq$\$360	\$32-$0.2\times(MI$-\$200)

Note: figures in thousands, CLP of 2009.

amount of the subsidy will follow a similar scheme to the annual one, which is described in Panel B of Table 1.

End of Payments. The worker ceases to be entitled to the subsidy for the months that the employer failed to pay (or paid late) the employees' social security contributions, or if the worker turns 21 years old and at that date has not received a high school diploma. Another reason is the failure to fulfill at least one of the eligibility criteria.

Two specific groups of workers have the right to request additional time to access YES. First, workers who have completed regular studies in a higher education institution and are

[9] According to Rau and Bravo (2015), in the 2009-2010 period about two-thirds of the workers opt for monthly payments.

between 18 and before 25 years old. Second, women can request an extension for each child born alive within the three months before she turns 25 years old.

Workers whose employment relationship ends while they were receiving the subsidy do not need to submit a new application at the time their new employment relationship begins. To the extent that they meet the requirements, they will keep the benefit, and it will be paid as soon as the pension contributions payments by the new employer are verified.

III. Identification Strategy

This section describes the problem of interest and explains the identification strategy used to quantify a causal peer effect. We follow closely Dahl et al. (2014) who start assuming that each network is composed of only two persons: 1 and his peer 2 (this assumption is relaxed later). Define y_{ig} as individual i's take-up decision within group g, which takes the value of 1 if i has YES and 0 otherwise. Then the system of simultaneous equations for peer effects is:

$$y_{1g} = \alpha_1 + \beta_1 y_{2g} + \gamma_1 x_{1g} + \tau_1 x_{2g} + \theta_1 w_g + \eta_{1g} \tag{1}$$

$$y_{2g} = \alpha_2 + \beta_2 y_{1g} + \gamma_2 x_{2g} + \tau_2 x_{1g} + \theta_2 w_g + \eta_{2g} \tag{2}$$

where x_{ig} are observable characteristics of individual i in group g, w_g are characteristics varying only at the group level, and η_{ig} are error terms. This model captures the idea that individual 2's choice is influenced by the choice individual 1 makes, and vice versa. It also allows individual 2's selection to depend on his own characteristics, the characteristics of individual 1, and common group-specific variables.

In this case, an individual's take up decision may be affected by the take-up decision of his peer, and the parameters β_1 and β_2 capture this endogenous peer effect (Manski, 1993). These equations have three problems (Manski, 1993; Moffitt, 2001; Dahl et al., 2014). First, there are correlated unobservables when not all relevant group-level (w_g) or individual characteristics (x_{1g}, x_{2g}) can be measured, leading to omitted variables bias in the estimated peer effect. Second, there is endogenous group membership because individuals choose which group to be part of, as a function of the choices and characteristics of the group. Finally, there is a reflection or simultaneity problem in which the decision of person 1 affects its peer 2 and viceversa, and as a result, the coefficients are not identified (Manski, 1993).

In our setting, the correlated unobservables emerge, for example, because advertising campaigns were made at the group level. Also, given the introduction of YES, workers could have moved to other employments or high schools where the adoption of social programs was higher or lower. Lastly, as a consequence of simultaneity, finding a significant peer effect does not mean that adoption by 2 is inducing 1 to adopt YES, but just that both made it at the same time.

A. Using quasi-random eligibility of peers

To correct for most of the issues mentioned above, we follow a "partial population" approach (Moffitt, 2001; Dahl et al., 2014), where for a given individual in a group we randomly vary which of his peers in this group can participate and see how this individual change his behavior. There is an exogenous variable which affects one individual directly but affects the other only through the endogenous social interaction.

To explain our approach, assume that individual 1 is always eligible to YES, and consider an experiment where the peer eligibility to the program, z_{2g}, is randomly varied for 2 only. Hence z_{2g} is either 1 or 0. Then equations (1) and (2) become:

$$y_{1g} = \alpha_1 + \beta_1 y_{2g} + \gamma_1 x_{1g} + \tau_1 x_{2g} + \theta_1 w_g + \eta_{1g} \tag{3}$$

$$y_{2g} = \alpha_2 + \beta_2 y_{1g} + \gamma_2 x_{2g} + \tau_2 x_{1g} + \theta_2 w_g + \lambda z_{2g} + \eta_{2g} \tag{4}$$

Since z_{2g} is randomly assigned to individual 2, it will be uncorrelated with x_{1g}, x_{2g}, w_g, e_{1g} and e_{2g}. And since the peer 1 is always eligible then the exogenous variable z_{2g} is excluded from the first equation. According to Moffitt (2001) this solves the simultaneity problem because it only affects y_{1g} through its effect over y_{2g}. Then the peer effect β_1 can be obtained by regressing y_{1g} on z_{2g} and scaling it by $\hat{\lambda}$. The next section explains how we implement this approach in our context.

B. Empirical Strategy

In our case, eligibility to the YES changes sharply based on the monthly wage due to the discontinuity in the assignment rule. As explained before, eligible workers must have a monthly wage below CLP $360,000. Thus, we get quasi-random variation in eligibility for YES for peers whose wage is inside a small window around the cutoff. This quasi-random variation occurs if individuals are not able to perfectly manipulate their wages to sort at the left of the cutoff. Thus, some of them will be at the left and some at the right of the cutoff.

The logic behind the instrument is that admissibility to YES in a particular period represents an exogenous random information shock within the group. The only way that 2's admissibility can affect 1's take-up decision is through 2's adoption of YES.

To construct the instrument, we define a narrow window of size Δ around the cutoff x_0, and compute the fraction of eligible peers in the window[10]. Formally,

$$\overline{z}^*_{2g} = \sum_{j \in N_1} \frac{1\left(wage_j \in [x_0 - \Delta, x_0]\right)}{n^*_1} \tag{5}$$

where $1(\cdot)$ is an indicator function and n^*_1 is the number of peers with wages inside the window $[x_0 - \Delta, x_0 + \Delta]$. The window size used is $\Delta = 60,000$. In Appendix C3 we study how sensitive are the results for the chosen window width as a robustness check.

To implement this approach we do something similar to Dahl et al. (2014) and restrict the sample to groups who have at least one peer with a wage inside the window, labeled 2 in equation (4), and the wages of all the other peers are outside the window, below $x_0 - \Delta$, labeled 1 in equation (3). Notice that z_{2g} only affects 2 in equation (4) and not 1 because the wages of all the peers in group 1 are below $x_0 - \Delta$, so they are all eligible. Hence, the instrument is constructed using information from 519,785 individuals, of which 86,404 individuals are in group 1 and this is the estimation sample.

There is one main difference with the approach of Dahl et al. (2014). While they have networks with only one peer in the window affecting adoption of many individuals, we have networks with one or more peers in the window affecting adoption of one person. But this can be easily taken into account by summing equations (3) and (4) among the members of the network and divide by the size of the group, see Appendix A for a derivation under these conditions.

Assume that each individual 1 has a specific peer's group N_1 of size n_1 (for example peers 2, 3, 4,...), and define $\overline{x}_2$ as $\sum_{j \in N_1} \frac{x_{jg}}{n_1}$, for any variable x. Then the peer effect can be identified by the following two-equation system:

$$y_{1g} = \alpha_1 + \beta_1 \overline{y}_{2g} + \tau_1 \overline{x}_{2g} + \gamma_1 x_{1g} + \eta_{1g} \tag{6}$$

$$\overline{y}_{2g} = \alpha_2 + \lambda \overline{z}_{2g} + \tau_2 x_{1g} + \gamma_2 \overline{x}_{2g} + \eta_{2g} \tag{7}$$

Since z_{2g} is orthogonal to all observed and unobserved covariates, their sum is also uncorrelated. Hence correlated unobservables can no longer bias the estimates. The last is true as long as each z_{ig} is determined independently.

We can estimate λ in a first stage regression given by (7). By estimating the following reduced form model, we can examine whether this quasi-random variation in the peer's eligibility (assigned the label 2) changes the individual take-up behavior of the coworker or

[10] This is not the same as the compliant subpopulation, the compliers are those peers whose wage is within the interval and adopt when they are admissible.

classmate (assigned the label 1):

$$y_{1g} = \alpha_1 + \pi \bar{z}^*_{2g} + u_{1g} \tag{8}$$

As is well known (Imbens and Angrist, 1994), under a set of assumptions, the Wald estimand can be interpreted as a local average treatment effect (LATE), the average causal effect of peer adoption on those whose treatment status can be changed by the instrument.

Subsection IV.C expands this approach a little further to allow for comparisons between network. And Subsection V.A we show that, first, the instrument given by equation (7) is randomly distributed among the population inside a very small window around the cutoff, then it is uncorrelated to other variables. Second, that the only way that the instrument affects individual take-up is through its effect on peer's adoption. Lastly, that individuals cannot manipulate the assignment variable. The next subsection explains how the peer effects in the two networks can be compared.

IV. Data

A. Data and Networks

The information sources come from four different administrative datasets that were merged using a unique identification number (ID): the Ficha de Protección Social (FPS),the Unemployment Insurance (AFC), the Chilean Student Registration (RECH), and the Youth Employment Subsidy (YES).

The FPS data includes information on socio-economic characteristics and economic vulnerability of families. This database contains the FPS score which is a proxy-means score that determines social program eligibility. It assigns one score for all household members going from 2,072 to 16,316 points, with a higher number implying a lower degree of vulnerability. We have access to a panel from December 2007 to September 2013 with a periodicity of March, September, and December in most of the years. This database contains information on an individual's date of birth, sex, an indicator if the person was born in Chile, educational level, *comuna* of residence[11], and the vulnerability score and its decile, but only for individuals between 18 and 25 years old.

The AFC database is a matched employer-employee data that contains information on all the workers who have ever contributed to the Chilean unemployment insurance (UI) system since it started in October 2002. These are formal dependent employed workers from the private sector (it excludes the public sector–see footnote 7). It includes information on each

[11] Information on the *comuna* of residence is coded in a way that does not allow to know where each person lives. We can only know that two persons live in different areas because their *comuna* of residence codes are different between each other.

employee's total taxable income in the last 6 and 12 months, and the number of months with taxable income in the last 6 and 12 months. It also contains a unique ID for each employer, so we can identify someone's employment history and compare it to someone's else. It also contains information about the firm, such as the number of employees, economic activity among other.

At the time the data was delivered, the AFC database was already merged to the FPS dataset described above by the Ministry of Social Planning. Notice that this "FPS+AFC" database provide valuable and disaggregated information on both employed and unemployed individuals, before and after the YES program began. It allows us to know if a worker meets YES eligibility criteria or not, the labor network of formal dependent workers, individual and mean peers characteristics, and characteristics at the level of the employer.

The RECH database contains nationwide information on the academic history of enrolled students at any educational institution (other than third-level) officially recognized by the Ministry of Education. The variables requested in this work includes the student's nameless and masked ID, a masked and nameless ID of the educational establishment where the student is registered (RBD), study plan[12] , grade and classroom codes. Data provided is between 2002 and 2012, for all individuals registered in those years. The RECH database allows identifying, very precisely, the classmates and schoolmates of a person, at different educational institutions, years and grades.

Finally, the YES database are annual administrative lists of all monthly and annual payments of YES made by SENCE. It contains a nameless and masked ID for YES recipient, an indicator of monthly or annual payment, and month and year of the subsidy. This database allows us to know when a worker begins to receive YES and if at any time t the worker is receiving YES or not. Unfortunately, the YES database used here does not contain information on the amount of YES payments, but it can be inferred using the subsidy assignment rules and the reported wage.

Table 2 presents descriptive statistics on the main variables used over different samples. Column (1) uses the full sample, column (2) uses a sample of workers with YES at some time, and column (3) uses a sample of employees without YES at some time. The sample consists of youths with a mean age of 21 years old; a small fraction are migrants, and it is well balanced between men and women with an average *FPS* of 8,256 points (there is no comparison point at the national level). It contains youths with a mean wage of CLP$223,053 which is lower than the national mean wage of CLP$355,771 according to CASEN (2011), and the occupancy rate is 51% which is lower than the average national occupancy rate of 55% for the period 2010-2012. The mean years of education are 11, and they are higher than the national mean of 9.5 years of schooling during 2012 (INE, 2012). Only 76% had an FPS

[12]The study plan refers to Educación Parvularia, Enseñanza Básica, Educación Especial, Enseñanza Media Humanista Cientí ica, Enseñanza Media Técnico Profesional and Enseñanza Media Artística

score lower than 11,734, and only 45% have a wage lower than the $360,000 threshold; as a result only 33% are eligible to apply to YES. Column (4) shows that the sample of people with YES is, on average, quite different from the sample of individuals without YES, providing evidence of potential self-selection into YES participation. However, the most interesting fact of all is that the fraction of classmates with YES among people with YES is 13%, making it 3% higher than the fraction of classmates with YES among people without YES which is 10%. The same thing happens with the fraction of coworkers with YES, which is 17% among people with YES and 10% among people without YES. This suggests that there could be some social multiplier in the adoption of YES, and this is precisely what the next section attempts to quantify.

B. Sample Selection and Networks

The rich dataset allows us to focus on individuals that can be tracked before the introduction of YES so we can construct coworkers and classmates network before the subsidy started. Hence, we can fix the groups of coworkers and classmates at a point in time and track them several months after the introduction of YES. Defining networks this way allows us to isolate any changes induced by the introduction of the YES over the composition of the groups. Any changes in group membership which happen after the introduction of YES are either a causal result of the randomization or orthogonal to changes in the randomization (Dahl et al., 2014). Defining groups before the introduction of the subsidy lessen concerns with endogenous group membership because it does not create any bias.

We start by fixing individuals at a point in time by taking a cross-sectional cut of the FPS-AFC database for December 2008. We choose this date because it is the closest to the YES announcement in March 2009. Then, we construct the measure of coworkers and our measure of wages. Two individuals i and j are coworkers if they are working for the same employer.

Separately, we append all the RECH datasets and keep only individuals that graduated from twelve grade (known as *cuarto medio*) before 2009, the year when the subsidy started. Two persons i and j are classmates if they were enrolled in twelve grade, in the same educational institution and the same year. As a result, they are classmates of the same generation, not only schoolmates.

We finally merge this two datasets together and then merge it with the YES payments database.

An important issue is the presence of missing values. Hence we follow an "individual-deletion procedure (IDP)", where individuals whose characteristics are not seen are deleted from our database[13]. Then, the coworkers of an individual are defined according to the last

[13]This is an important point, according to Sojourner (2013) this can render biased and inconsistent estimates.

employer before the introduction of YES.

Since the AFC database contains information on the labor history of only formal dependent workers, the information on the networks could be limited if the workers maintain a stronger relationship with other dependent informal or independent workers.

C. *Comparing Networks*

We are interested in comparing which network influence more the individual adoption decision: coworkers or classmates. To make this comparison, the endogenous variable $\overline{y}_{2g}$ will have to be modified to account for both networks. It is easy to realize that the fraction of peers with YES is composed of the fraction of coworkers with YES and the fraction of classmates with YES, as long as two assumptions hold. First, the correct reference groups must be the coworkers and classmates only; and, second, there must be enough independence between the two groups (the validity of this assumption will be assessed in Subsection VI.A) [14]. Define l_net$_2$ as $\frac{\sum_{j\in C_1} y_j}{c_1}$ the fraction of 1's coworkers with YES with a wage inside the window, and s_net$_2$ as $\frac{\sum_{j\in S_1} y_j}{s_1}$ the fraction of 1's classmates with YES with wage inside the window, where the group of 1's coworkers is C_1 of size c_1, and the group of 1's classmates is S_1 of size s_1. Then the structural equation becomes:

$$y_{1ls} = \alpha + \beta_1 \text{l\_net}_{2l} + \beta_2 \text{s\_net}_{2s} + \nu_{1ls} \tag{9}$$

Since each network can have a different impact over the individual take-up decision, there are two endogenous variables and two instruments. Now the first stage equations are:

$$\text{l\_net}_{2l} = \gamma_1 + \lambda_{11}\text{z\_lnet}_{2l} + \lambda_{12}\text{z\_snet}_{2s} + \xi_{2ls} \tag{10}$$

$$\text{s\_net}_{2s} = \gamma_2 + \lambda_{21}\text{z\_lnet}_{2l} + \lambda_{22}\text{z\_snet}_{2s} + \xi_{2ls} \tag{11}$$

where z_lnet$_2 = \frac{\sum_{j\in C_1^*} z_j}{c_1^*}$ and z_snet$_2 = \frac{\sum_{j\in S_1^*} z_j}{s_1^*}$ are the fraction of coworkers and classmates whose wage is at the left of the cutoff and inside the small window, respectively. And C_1^* and S_1^* are the groups of 1's coworkers and classmates whose wage are inside the small window of size Δ around the x_0 cut-off, respectively.[15] Equations (9), (10), and (11) can be augmented to include covariates X_i at both the individual and group level.

Equation (9) is estimated by 2SLS, with first stages given by (10) and (11). Since any shock common to the group creates spurious peer effects, standard errors are clustered using multi-way clustering at the (grade-school-year x workplace-year) according to Cameron,

[14] Under these assumptions, $\sum_{j\in N_i} D_j = \sum_{j\in C_i} D_j + \sum_{j\in S_i} D_j$ because $N_i = \{C_i, S_i\}$ of size $n_i = c_i + s_i$. As a result, $\overline{Y}_{-ig} = \frac{\sum_{j\in N_i} D_j}{n_i} = \frac{\sum_{j\in C_i} D_j}{n_i} + \frac{\sum_{j\in S_i} D_j}{n_i}$. Instead of dividing by n_i we divide each term by c_i or s_i.

[15] Then $C_i^* \equiv \{j \in C_i : FPS_j \in [x_0 - \Delta, x_0 + \Delta]\}$ and similarly $S_i^* \equiv \{j \in S_i : FPS_j \in [x_0 - \Delta, x_0 + \Delta]\}$.

Gelbach and Miller (2006). See Appendix Figure C1 for Kernel density estimates and Table C1 for other descriptive statistics. The next section addresses the problem of potential manipulation and violation of the exclusion restriction.

V. Testing identification assumptions

A. Manipulation of Eligibility Rules

The implicit assumption of our identification strategy is that individuals cannot perfectly manipulate the assignment variable (McCrary, 2008; Lee and Lemieux, 2010), which is the peers' wage. We look for discontinuities of the wage distribution at the cutoff, for monthly and yearly wages. We implement this approach for several dates, before and after the YES implementation. If individuals cannot perfectly manipulate their wage, the aggregate distribution of the assignment variable should be continuous around the cutoff value.

In Figure 1, we present the density of the wages following the McCrary (2008) approach and perform the density test. The results show a smooth density function of the wages at the cut-off. In Appendix C2 we show densities for other dates and other measures of wages.

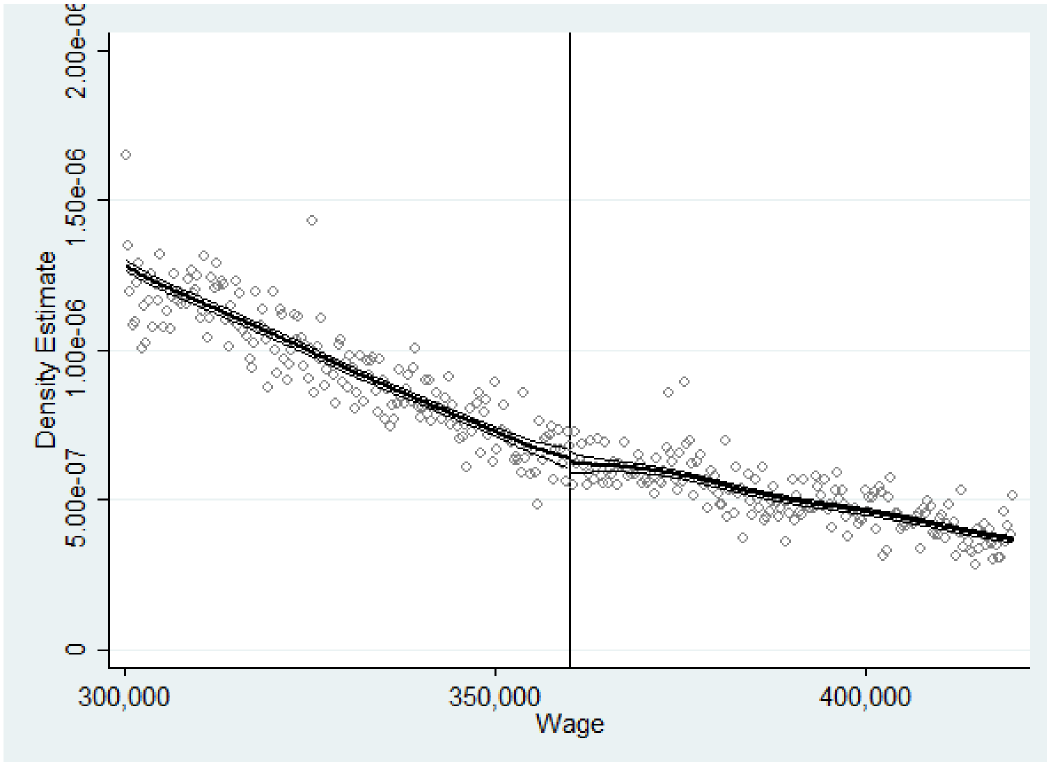
B. Exogeneity of the instrument

The validity of the instrument depends on the absence of correlation between the residual and the instruments themselves (clean instruments). Thus, the instruments operate through a single known causal channel and induce random assigned throughout the population. In other words, firms and high-schools with a low fraction of peers at the left of the cutoff and inside the window would not have been different on average from firms and high-schools with a high fraction of peers on the left of the cutoff and inside the window.

Since we are in a case of exact identification, we cannot perform a Sargan test for the null hypothesis of clean instruments. Instead, we regress a variety of characteristics at the classmates and coworkers levels on both instrumental variables with results reported in Table 3. These regressions are similar to equations (10) and (11) but instead of using s_net_{2s} and l_net_{2l} at the left side, we take the mean of characteristics specified in each row at the level of the classmates in column (1) and coworkers in column (2). As we can see, most of the coefficients are small and not significant, especially for coworkers network. This suggests that the instruments were quasi-randomly assigned among groups.

Although, there are some differences regarding age, sex and employment duration of classmates, these coefficients are small. For example, having a 10% more classmates with a wage inside and at the left of the window implies classmates 0.97 years older.

Figure 1. : Density Function of wages, during December 2008



Notes: This figure was constructed using the McCrary (2008) test and it shows density estimates of the probability density function for monthly wages and for different dates, around the selected window of width $60,000 CLP. To construct this figure, first we kept only wages inside the selected window and save the bandwidth used by the McCrary (2008) code to construct the graphs, for each date. Then, using the full sample of wages and the saved bandwidth, the graph was created by limiting the domain to wages within the window.

VI. Results

Table 4 shows the main results for 2009, the first year of implementation. All regressions in this table include as control variables the individual's age, age^2, sex, FPS score, Ln(wage), years of education and the *comuna* of residence. A *comuna* is the lower and primary administrative division of Chile, and corresponds to what in other countries is known as a municipality. The *comuna* is a division with local government purposes only, because in Chile the state government only extends at the regional and provincial level. This is interesting because despite the YES program was managed by the state government institution SENCE, some municipalities could have characteristics that made them more or less interested in participating in YES. The reason to add these fixed effects is to control for such unobserved heterogeneity at the *comuna* levels.

Column (1) shows the coefficients from an OLS regression of equation 9 in a linear probability model (LPM). It shows that the individual probability of adopting YES increases by 0.24 pp and 0.73 pp, given a 10 pp increase in the fraction of classmates and coworkers with YES inside his network, respectively. However, these peer effects are subject to several known problems such as correlated unobservables and a reflection problem. The next columns try to solve these problems and to establish causality.

Columns (2) and (3) show first stage estimates of equations (11) and (10), respectively. They show the causal effect of the fraction of classmates and coworkers with a wage lower than the cut off and inside the 60,000 window on the fraction of classmates and coworkers with YES, during 2009. These columns shows a very strong first stage for the coworkers with an F-statistic above 50, but the classmates first stage is not as strong, and has an F-statistic of only 7.5.[16] The coefficients show that having a 10 pp higher fraction of coworkers inside and at the left side of the small window increases the fraction of coworkers with YES in 0.48 pp, and having a 10 pp higher fraction of classmates inside and at the left side of the small window increases the fraction of classmates with YES in 0.32 pp.

Column (4) shows reduced form intention to treat (ITT) estimates. It shows that having 10 pp more coworkers with a wage inside and at the left of the window had an average causal effect of increasing the individual probability of adopting YES by 0.23 pp, a point estimate that is significant at the 1% level. However, having a bigger fraction of classmates with a wage inside and at the left of the window had no impact over the individual decision to adopt YES.

Finally, column (5) shows the Wald 2SLS estimates of equation (9) using columns (2) and (3) as first stages. These coefficients measure the average causal peer effect of the coworkers and classmates' adoption on individual adoption. Unlike column (1), this column shows no evidence that the classmates peer effect is different from zero. However, it also shows a positive average causal peer effect of coworkers on individual adoption during 2009. Having 10 pp additional coworkers with YES increases the individual probability of adopting YES by 4.8 pp. This is a sizable peer effect when compared to other works. For example, it is almost four times the effect reported by Dahl, Loken and Mogstad (2014), which may be expected since the features of the YES application process that make it simple and cheap to apply. Hence, admissible workers have a lot to gain from finding out information concerning the existence of YES and its application process.

A. Robustness Checks

Table 5 summarizes several robustness checks. In row A we present the baseline results and row B shows the results the first robustness check that considers running the baseline

[16] The F statistics reported are Angrist and Pischke (2009) that allows to test if one of the endogenous regressors is weakly identified.

regression without any control variables. As can be seen, the coefficients from the first stages change very little. This confirms that the instrumental variable was quasi-randomized in the window around the cut off and hence, this assures that it is independent of other covariates.

Then row C considers an LPM with one endogenous and one instrument at the time; it shows that the coefficients from the first stages change very little; hence both instrumental variables are independent of each other. However, the estimated coefficient for the classmate's first stage in row C compared with the baseline results shows that the the classmates and coworkers networks are somehow related from each other, so someone's classmate can be the same time his coworker.

Notice that the 2SLS coefficient of the classmates in row C is closer to one. Boozer and Cacciola (2001) show that the instrumental variables estimator is algebraically exactly equal to 1 for the empirical endogenous peer effects regression, without covariates, where the researcher uses characteristics of the *full* group in the sample as either an instrument or regressor[17]. Row B should in part reduce this concern because of none of the coefficients in column (5), row B equal 1. Remember that the sample is restricted to individuals with information on the fraction of his classmates and coworkers with YES and with information on both the individual and group controls. However, moreover, if Boozer and Cacciola's argument is the driving the results then the reduced form and first stage coefficients from row D should be the same, and they are not. Finally, row E shows that the results are not driven because of the LPM. This row shows coefficients form a non-linear probit model. The reduced form follows a probit model; the first stages are OLS, and the second stage is an instrumental variables probit model calculated following Newey (1987). The coworkers peer effect remains significant.

Row F adds fixed effects at the high school level. High schools with a higher fraction of classmates at the left of the window could have many unobserved characteristics; such as worst classmates, teachers, and infrastructure; that makes them more susceptible to apply to YES, and this could violate the exclusion restriction. Row F shows that, despite the inclusion of the fixed effects, the first stage remains very strong and unchanged; moreover, the second stage coefficients do not deviate too much from the baseline results.

Row G adds controls at the group level. The group level controls are the mean characteristics of the individuals inside the window, the characteristics are age, age squared, sex, vulnerability score, and years of education. We do not include wage due to the payments scheme.

Another potential concern is causality or the reflection problem. If everyone inside a network is adopting YES at the same time, then, a significant peer effect could be found even though peers are not necessarily talking about YES adoption or interacting among them in

[17]See Appendix B.

the application process. It just happens that all of them are applying at the same time. Table 6 presents evidence aiming at reducing this concern by looking at the effect of early adopters during 2009 over the individual probability of adopting YES during 2010. While the first stages remain the same, we change the dependent variable, and the results in this table show that 10 pp increase in the fraction of coworkers with YES 2009 increased the individual probability of adopting YES during 2010 by almost 3.3 pp.

B. Mechanisms

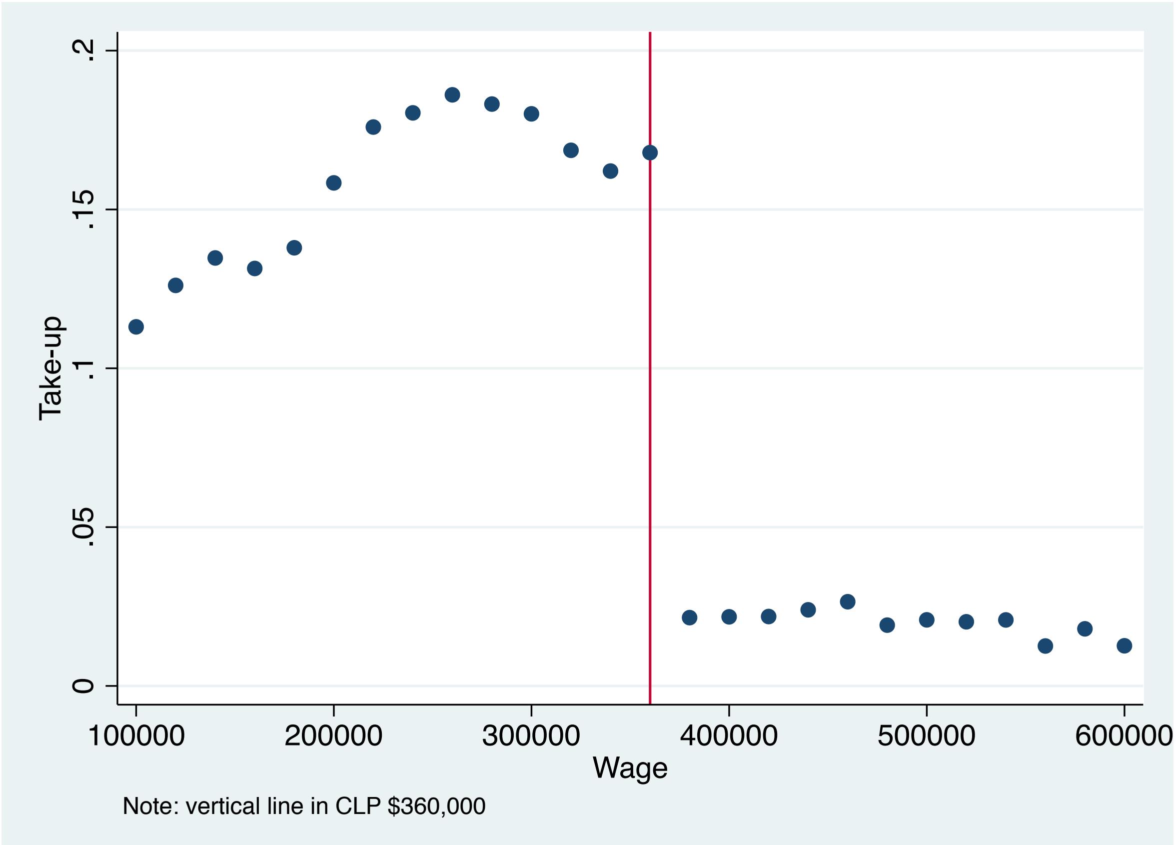
Before explaining the potential mechanisms through which the peer effects are operating, it is important to mention what is driving the first stage. Figure 2 shows the mean take-up rates of individuals inside wage bins CLP $ 20,000 wide in March 2010. As can be seen, the probability of adopting YES is quite uniform across wages, and is not concentrated in just a small interval. As a result, the fraction of peers at the left of the small window should be a good predictor of who is applying to YES.

What is driving the peer effects? Table 7 shows results using subsamples of small and large firms and schools. Small firms are those with less than 50 workers, and small schools are schools with less than the median size of the schools, 54 students. See the footnote at the end of the table for a detailed description on how this table was constructed. This Table shows that the coworkers peer effects are important regardless of the size of the school. However, peer effects are mostly concentrated in firms with over 50 workers. Irrespective of the first-stage, the reduced form results are not biased, and they show no significant peer effects among small firms.

On the other hand, the results in section VI show significant peer effects from coworkers, in this context, these peer effects are likely to be due to informational benefits that get translated into adoption. One information channel is the social program itself and its information campaign at the beginning of the program. However, knowledge about the existence of the program would not automatically translate into adoption due to its unknown application procedure, admissibility rules, and other details of how it works, especially for a recently launched welfare program. For example, during November 2014 the Minister of Labor did a media campaign encouraging the 122,000 beneficiaries of YES and other women employment subsidy to claim USD$37,029,382 of unclaimed payments. The accumulation of unclaimed payments is sometimes due to ignorance of how YES works. As explained before, a person does not lose the benefit when she changes job, but the payments are held until she becomes eligible once again, payments resume automatically. With all, unclaimed payments for some individual cases summed up to USD$2,067[18]. In total, during this date, 1,800 youth workers

[18]"Bono Trabajo Mujer y Subsidio al Empleo Joven: llaman a cobrar más de 21 mil millones", 24horas.cl, November 3, 2014; and "Ministra de Trabajo llama a cobrar bonos Trabajo Mujer y Empleo Joven", 24horas.cl, November 2, 2014

Figure 2. : Mean take-up rate by wage, March 2010.



Notes: This figure plots mean take-up rate of individuals inside wage bins CLP $20,000 wide in March 2010. The sample is restricted to youths between 18 and 25 years with a score in the FPS below than 11,734. The vertical axis measures the fraction of people with YES and the horizontal axis measures the wages.

may claim amounts higher than USD$1,378[19].

Another channel of information is knowledge about the benefits and costs of participation, including a stigma cost. In this setting, the initial information set about these is quite limited. However, the admissibility requirements allowed some networks to quasi-randomly have more/fewer peers around a small window of the cut off while leaving everyone else intact. The results show that this exogenous increase in the fraction of admissible peers that are allowed to apply translated into a higher adoption of YES inside each network, hence reducing uncertainty about the costs and benefits.

With the current data, it is not possible to explain what these youths are talking about, whether about the benefits, costs, application procedure, etc. and the type of information

[19] "Bonos y subsidios Sence: llaman a cobrar $21 mil millones no reclamados", 24horas.cl, October 24, 2014

that is being transmitted inside each network. Classmates could be talking more about YES existence while the coworkers can provide more details about its costs and benefits. Despite this, there are several subsamples where one explanation is more likely to hold than the other. The first one is among older people with higher working experience that have spent more time building stronger ties with their coworkers. Hence they are more likely to keep a closer interaction or appeal to them when they need information about social programs. Table 8 tests if peer effects among older workers are stronger. The results show that the coworkers are important and similar in both young and old workers, but the peer effect is significant only for those with over five years of experience.

Another subsample where peer effects are expected to be larger is among individuals that were admissible before the introduction of YES. Panel D shows that peer effects are important only among people that were admissible during December 2008, but are not relevant among non-admissible individuals.

C. Other dates

Section VI shows that adoption by the coworkers had an important effect in determining participation in YES during 2009. But, does this effect hold over time? There is no reason to believe that peer effects will remain constant over time, they may increase or decrease over time, and could shift in importance between one network or another. Table 9 shows results of peer effects six months after YES was implemented. The results in Column (5) show that peers effects decrease a year later in 2010, when a 10 pp increase in the fraction of coworkers with YES increased the individual probability of adopting YES by 3 pp, and the classmates remain insignificant, even when the first stages remained strong. This variation over time provides interesting evidence on the behavior of the peer effects and how they adapt over time.

Peers could be more important during the early stages of YES implementation for several reasons. First, during this period information about the existence of YES is scarce, and other details of its application procedure and costs and benefits, but afterward many people learn about it, and peers become less relevant. Second, they are similar to a herd behavior of a trending topic where the peer effect is important at the beginning, when advertising was undergoing, but as the advertising decreases, the amount of time devoted to YES in the conversations also does so. Third, peers shift in importance and get depreciated over time, and since the classmate's definition is fixed here, they can become less important as people get older, but this does not necessarily mean that people stopped learning about the existence of YES and other details of its application procedure and its costs and benefits. Fourth, Bravo and Rau (2013) finds that admissibility to YES had an impact on formal employment but this effect decays over time. As a result, adopting YES several months after its implementation becomes less relevant. Altogether, peers help to solve a lack of information, but once the information is available, they lose importance.

VII. Conclusions

This paper estimates the effect of peers in the decision of adoption of a Youth Employment Subsidy (YES) in Chile. We follow an instrumental variable approach exploiting the exogenous variation induced by a discontinuity in the eligibility rule. To deal with endogenous group membership, the networks are defined before the introduction of the program, and, as a result, all changes in the groups' composition can be taken as orthogonal to the introduction of the YES.

The results show that during 2009, coworkers played a crucial role in determining participation in the program, but classmates were not relevant. A 10 pp higher fraction of coworkers with YES implies a 4.8 pp higher individual probability of adopting YES (significant at the 1%); while a larger fraction of classmates with YES does not affect the individual probability of adopting the program.

There are several explanations for the existence of peer effects, but the most likely one is due to an informational channel on the existence of the program and the unknown application costs, benefits and other details of YES. Given the particular case of YES, were application costs are extremely low, this information translated into adoption, and this explains the significant peer effects during the early years of its implementation. However, the effects decay over time. One explanation is that coworkers help to solve a lack of information, but once the information is available, they lose importance.

This hypothesis is sustained by the second set of results. They show that coworkers peer effects are important regardless of the type of education the person graduated from (either from a vocational or normal academy). Moreover, peer effects are more important among older workers with more working experience, especially if they were working in a large firm. These results are interpreted as providing evidence that an informational channel is the most important driver of the peer effects because people with more working experience had had the opportunity to build stronger relations within the job.

When the early adopters of YES are distributed according to several criteria, the results show that the highest increase in take-up is achieved when the early adopters are distributed only among small schools. As a result, if the government had chosen to target small schools with advertising, and especially small schools, instead of firms, the take-up rate of YES nowadays could have been higher.

Future work can exploit a methodology similar to the one employed in this work to study peer effects in other means-tested social programs. For example, program eligibility in the *Chile Solidario* (CS) program is an also a discontinuous function of the FPS. Carneiro, Galassoy, and Ginja (2014) find that CH participants increased take-up by CS recipients of a family allowance for poor children (the *Subsidio Unico Familiar*, SUF) because CS provided information of other social programs. This allows studying take up rates of YES and other social programs among ineligible peers from recipients of CS. On the other hand, Bravo and Rau (2013) finds that employment and participation rates increased among the eligible population of YES in the first six months of implementation. Once again, this result can be exploited to study what happens with the employment and participation rates inside networks who had at least one peer inside a small window around some cutoff value.

Further research can exploit the YES payments scheme of a phase-in, plateau, and phase-out in a regression kink design (Card et al., 2015) to study how take-up rates are directly affected when changing the amount of the payment.

References

Abulkadiroglu, A., Angrist, J., and Pathak, P. (2014). The Elite Illusion: Achievement Effects at Boston and New York Exam Schools. *Econometrica*, 82(1):137–196.

Acemoglu, D., Dahleh, M. a., Lobel, I., and Ozdaglar, A. (2011). Bayesian learning in social networks. *Review of Economic Studies*, 78(4):1201–1236.

Aizer, A. and Currie, J. (2004). Networks or neighborhoods? Correlations in the use of publicly-funded maternity care in California. *Journal of Public Economics*, 88(12):2573–2585.

Anderson, P. M. and Meyer, B. D. (2012). Unemployment Insurance Takeup Rates and the After-Tax Value of Benefits. *The Quarterly Journal of Economics*, 112(3):913–937.

Angrist, J. and Imbens, G. (1994). Identification and estimation of local average treatment effects. *Econometrica*, 62(2):467–475.

Angrist, J. D. (2014). The perils of peer effects. *Labour Economics*, 30(October):98–108.

Bala, V. and Goyal, S. (1998). Learning from Neighbours. *Review of Economic Studies*, 65(3):595–621.

Ballester, C., Calvó-Armengol, A., and Zenou, Y. (2006). Who's who in networks. Wanted: The key player. *Econometrica*, 74(5):1403–1417.

Bertrand, M., Luttmer, E. F. P., and Mullainathan, S. (2000). Network effects and welfare cultures. *The Quarterly Journal of Economics*, 115(3):1019–1055.

Blasco, S. and Fontaine, F. (2009). A structural model of the unemployment insurance take-up.

Boozer, M. A. and Cacciola, S. E. (2001). Inside the 'Black Box' of Project STAR: Estimation of Peer Effects Using Experimental Data.

Borjas, G. J. and Hilton, L. (1996). Immigration and the Welfare State: Immigrant Participation in Means-Tested Entitlement Programs. *The Quarterly Journal of Economics*, 111(2):575–604.

Bravo, D. and Rau, T. (2012). Effects of Large-scale Youth Employment Subsidies: Evidence from a Regression Discontinuity Design.

Cáceres, C. S. and Bobenreith, E. H. (1993). Determinantes del Salario de los Egresados de la Enseñanza Media Técnico Profesional en Chile. *Cuadernos de Economía*, 30(89):111–129.

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2011). Robust Inference With Multiway Clustering.

Carneiro, P., Galasso, E., and Ginja, R. (2013). Tackling social exclusion: Evidence from Chile.

Carrell, S. E., Sacerdote, B. I., and West, J. E. (2013). From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation. *Econometrica*, 81(3):855–882.

Cingano, F. and Rosolia, A. (2012). People I Know: Job Search and Social Networks. *Journal of Labor Economics*, 30(2):291–332.

Currie, J. M. (2004). The Take Up of Social Benefits.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014). Peer effects in program participation. *American Economic Review*, 104(7):2049–74.

Duflo, E. and Saez, E. (2002). The role of information and social interactions in retirement plan decisions: Evidence from a randomized experiment. *The Quarterly Journal of Economics*, 118(3):815–842.

Dustmann, C., Glitz, A., and Schönberg, U. (2012). Referral-based Job Search Networks.

Ellison, G. and Fudenberg, D. (1995). Word-of-mouth communication and social learning. *Quarterly Journal of Economics*, 110(1):93–125.

Final, I. (2014). Estudio Quinta Encuesta Sobre Acceso, Usos, Usuarios y Disposición de Pago por Internet en Zonas Uranas y Rurales de Chile.

Geroski, P. (2000). Models of technology diffusion. *Research Policy*, 29(4-5):603–625.

Glitz, A. (2013). Coworker Networks in the Labour Market.

Gloria, M., García, A., Del, J., and Anabalón, R. (2005). Registro de los Estudiantes de Chile : un sistema de información masiva en extranet. In *VII Simpósio Internacional de Informática Educativa SIIE05*, pages 369–374.

Golub, B. and Jackson, M. O. (2012). How homophily affects the speed of learning and best-response dynamics. *Quarterly Journal of Economics*, 127(3):1287–1338.

Hahn, J., Todd, P., and Van der Klaauw, W. (2001). Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design. *Econometrica*, 69(1):201–209.

Hanley, J. A. and McNeil, B. J. (1982). The meaning and use of the area under a receiver operating characteristic (ROC) curve. *Radiology*, 143(1):29–36.

Heckman, J. J. and Smith, J. A. (2004). The Determinants of Participation in a Social Program: Evidence from a Prototypical Job Training Program.

Hellerstein, J. K., McInerney, M., and Neumark, D. (2011). Neighbors and Coworkers: The Importance of Residential Labor Market Networks.

Huneeus, C. (2010). Balance de los avances y desafíos de las políticas de empleo para jóvenes. Technical report.

Imbens, G. W. and Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, 142(2):615–635.

Instituto Nacional de Estadísticas (2012). *Resultado XVIII Censo de Población 2012*. Instituto Nacional de Estadísticas, Santiago.

Jadbabaie, A., Molavi, P., Sandroni, A., and Tahbaz-Salehi, A. (2012). Non-Bayesian social learning. *Games and Economic Behavior*, 76(1):210–225.

Kleven, H. J. (2008). Transfer Program Complexity and the Take Up of Social Benefits. *American Economic Journal: Economic Policy*, 3(1):54–90.

Lee, D. S. and Lemieux, T. (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, 48(2):281–355.

Manski, C. F. (1993). Identification of Social Endogenous Effects: The Reflection Problem. *The Review of Economic Studies*, 60(3):531–542.

Moffitt, R. (1983). An economic model of welfare stigma. *The American Economic Review*, 73(5):1023–1035.

Moffitt, R. A. (2001). Policy Interventions, Low-Level Equilibria, and Social Interactions. In *Social Dynamics*, pages 45–82.

Newey, W. K. (1987). Efficient estimation of limited dependent variable models with endogenous explanatory variables. *Journal of Econometrics*, 36(3):231–250.

O'Donoghue, T. and Rabin, M. (1999). Doing It Now or Later. *The American Economic Review*, 89(1):103–124.

Organización de Estados Iberoamericanos and Ministerio de Educacion de Chile (1993). Chile Sistemas Educativos Nacionales.

Riphahn, R. (2001). Rational poverty or poor rationality? The take-up of social assistance benefits. *Review of income and wealth*, 1(3):379–398.

Zimmerman, S. D. (2015). Making Top Managers : The Role of Elite Universities and Elite Peers.

Table 2—: Descriptive Statistics

	Full Sample (1)	With YES (2)	Without YES (3)	Difference (2) - (3) (4)
Age	21.4	22.0	21.4	0.64
	[2.22]	[1.71]	[2.23]	[0.004]
Women	0.50	0.51	0.50	0.01
	[0.50]	[0.50]	[0.50]	[0.001]
Migrant	0.004	0.003	0.004	-0.001
	[0.06]	[0.052]	[0.06]	[0]
Vulnerability Score	8,256	6,733	8,325	-1,592
	[3,890]	[3,034]	[3,910]	[6.4]
N	*8,826,468*	*381,963*	*8,444,505*	
Wage	223,053	213,414	223,936	-10,522
	[143,000]	[75,600]	[147,000]	[242]
Potential YES Payment	30,484	36,294	29,952	6,342
	[16,900]	[12,800]	[17,100]	[28.5]
N	*4,510,280*	*378,532*	*4,131,748*	
Years of Education	11.3	11.4	11.3	0.18
	[2.35]	[2.13]	[2.36]	[0.005]
N	*3,485,751*	*260,445*	*3,225,306*	
1(FPS≤11,734)	0.76	1.00	0.74	0.74
	[0.43]	[0.06]	[0.44]	[0]
1(18<age<25)	0.95	1.00	0.95	0.95
	[0.21]	[0.06]	[0.21]	[0]
Employed	0.51	0.99	0.49	0.50
	[0.5]	[0.09]	[0.5]	[0.001]
1(w<$360,000)	0.45	0.96	0.43	0.54
	[0.5]	[0.19]	[0.49]	[0.001]
Admissible to apply to YES	0.33	0.96	0.30	0.66
	[0.47]	[0.21]	[0.46]	[0.001]
N	*8,826,469*	*381,963*	*8,444,506*	
Fraction of classmates with YES	0.10	0.13	0.10	0.03
	[0.07]	[0.07]	[0.07]	[0]
N	*2,999,945*	*2,701,282*	*298,663*	
Fraction of coworkers with YES	0.11	0.17	0.10	0.06
	[0.16]	[0.19]	[0.16]	[0]
N	*891,344*	*791,627*	*99,717*	

Notes: Standard deviation in square brackets

Table 3—: Estimates of the effect of the instruments over covariates.

Mean peer characteristics	z_snet (1)	*R-squared*	*N*	z_lnet (2)	*R-squared*	*N*
Age	-0.0972*** [0.0343]	*0.81*	*85,868*	-0.1024 [0.1100]	*0.03*	*85,868*
Women	0.0515** [0.0212]	*0.21*	*85,868*	0.0368 [0.0270]	*0.15*	*85,868*
Years of Education	-0.1118 [0.0806]	*0.09*	*80,930*	-0.0174 [0.1917]	*0.04*	*81,533*
Peers has some college	-0.0211 [0.0196]	*0.06*	*85,868*	-0.0161 [0.0293]	*0.04*	*85,868*
Vulnerability Score	-41.0130 [132.8488]	*0.20*	*85,868*	-107.0232 [186.7564]	*0.05*	*85,868*
Fraction of last 6 months working	0.0925* [0.0536]	*0.13*	*85,868*	0.0164 [0.1428]	*0.06*	*85,868*
Fraction of last 12 months working	0.2318* [0.1259]	*0.29*	*85,868*	0.1859 [0.3102]	*0.06*	*85,868*
School size	0.5020 [3.1061]	*0.13*	*85,868*	-0.0935 [0.7786]	*0.13*	*85,868*
Firm size before SEJ	5.8361 [14.9139]	*0.05*	*85,868*	-147.622 [147.8021]	*0.05*	*85,868*
Fraction of same-school coworkers	0.0000 [0.0005]	*0.04*	*85,868*	0.0003 [0.0010]	*0.04*	*85,868*
Female workers	0.001 [0.0035]	*0.30*	*85,868*	0.013 [0.0152]	*0.30*	*85,868*

Notes: This table shows results of a regression with the dependent variable in the left column and explanatory variables given by z_lnet, z_snet and a set of individual controls that include age, age squared, sex, ln(wage), years of education, vulnerability score, and *comuna* of residence. Column (1) uses the mean characteristics of classmates inside the window and presents only the estimated coefficient for z_snet; column (2) uses the mean characteristics of coworkers inside the window and presents only the estimated coefficient for z_lnet. Standard errors clustered by school in column (1), and by firm in column (2). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4—: Effect of peer's adoption over individual YES adoption, 2009.

	2009				
	OLS	First Stage		Reduced	Second
		Classmates	Coworkers	Form	Stage (2SLS)
	(1)	(2)	(3)	(4)	(5)
Classmates	0.0237**				-0.0223
	[0.0095]				[0.1619]
Coworkers	0.0731***				0.4834***
	[0.0132]				[0.1446]
z_snet		0.0318***	-0.0001	-0.0008	
		[0.0116]	[0.0023]	[0.0055]	
z_lnet		-0.0060***	0.0480***	0.0233***	
		[0.0020]	[0.0096]	[0.0063]	
F-statistic (AP)		*7.5*	*55.9*		
Observations	*86,404*	*86,404*	*86,404*	*86,404*	*86,404*

Notes: This table shows the coefficients after regressing equations 9, 10 and 11. Column (1) shows OLS coefficients from equation 9, where the dependent variable is a dummy that takes the value of 1 if the individual has YES, 0 otherwise. Columns (2) and (3) shows first-stage coefficients given by equations 11 and 10, respectively. Column (4) shows reduced-form coefficients, where the dependent variable is the same as in column (1). The dependent variable in column (5) is the same as in column (1) and it shows 2sls coefficients given by equation 9 with first stages given by columns (4) and (5). Standard errors are clustered using multi-way clustering at the (school x firm) levels according to Cameron, Gelbach and Miller (2006). Individual controls include age, age squared, sex, vulnerability score, ln(Wage), years of education, and a set of dummies for *Comuna* of residence. The reported F-statistic includes the Angrist and Pischke (2009) correction. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5—: Robustness checks for the peer effects, 2009.

		First Stage			Reduced	Second	
		Classmates	Coworkers	*F-AP*	Form	Stage (2SLS)	*N*
		(1)	(2)	(3)	(4)	(5)	(6)
A	Baseline						
	Classmates	0.0318***	-0.0001	*7.5*	-0.0008	-0.0223	*86,404*
	(z_snet)	[0.0116]	[0.0023]		[0.0055]	[0.1619]	
	Coworkers	-0.0060***	0.0480***	*55.9*	0.0233***	0.4834***	
	(z_lnet)	[0.0020]	[0.0096]		[0.0063]	[0.1446]	
B	No controls						
	Classmates	0.0323***	0.0011	*7.2*	0.0065	0.1805	*86,337*
	(z_snet)	[0.0121]	[0.0026]		[0.0066]	[0.2005]	
	Coworkers	-0.0027	0.0508***	*29.9*	0.0306***	0.6128***	
	(z_lnet)	[0.0025]	[0.0100]		[0.0079]	[0.1569]	
C	One endogenous and one instrument, with controls						
	Classmates	0.0319***		*7.53*	-0.0004	-0.0111	*86,337*
	(z_snet)	[0.0116]			[0.0051]	[0.1599]	
	Coworkers		0.0480***	*24.85*	0.0232***	0.4832***	
	(z_lnet)		[0.0096]		[0.0059]	[0.1424]	
D	One endogenous and one instrument, no controls						
	Classmates	0.0322***		*7.2*	0.0068	0.2121	*86,337*
	(z_snet)	[0.0120]			[0.0067]	[0.2102]	
	Coworkers		0.0508***	*26.1*	0.0307***	0.6043***	
	(z_lnet)		[0.0100]		[0.0079]	[0.1570]	
E	Probit model, no controls						
	Classmates	0.0322***		*7.2*	0.0306	0.9317	*86,337*
	(z_snet)	[0.0121]			[0.0299]	[0.9310]	
	Coworkers		0.0508***	*26.1*	0.1407***	2.6329***	
	(z_lnet)		[0.0100]		[0.0372]	[0.6434]	
F	School and *Comuna* Fixed Effects						
	Classmates	0.0332**	-0.0012	*6.9*	-0.0016	0.0157	*86,404*
		[0.0156]	[0.0030]		[0.0071]	[0.2011]	
	Coworkers	-0.0037***	0.0477***	*30.1*	0.0242***	0.5432***	
	(z_lnet)	[0.0012]	[0.0094]		[0.0059]	[0.1527]	
G	Group-Level Controls						
	Classmates	0.0277**	0.0002	*5.8*	-0.0006	-0.0276	*82,114*
	(z_lnet)	[0.0115]	[0.0021]		[0.0052]	[0.1908]	
	Coworkers	-0.0061***	0.0464***	*61.3*	0.0284***	0.6078***	
		[0.0021]	[0.0111]		[0.0062]	[0.1745]	

Notes: To save space, columns (1) and (2) correspond to columns (2) and (3) in Table 4, and columns (4) and (5) correspond to columns (4) and (5) in that table. The F-statistic in rows C-E are not Angrist and Pischke (2009) but just an F-test that the excluded instruments equal zero. All standard errors are double clustered at the school x firm level, except for rows C-E where the standard errors are clustered at either the firm or the school level. See the notes below Table 4 for other details on regressions. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6—: Peer effects by early adopters during 2009 over individual adoption during 2010

	OLS	First Stage		Reduced Form	Second Stage (2SLS)	*N*
		Classmates	Coworkers			
	(1)	(2)	(3)	(4)	(5)	(6)
A Baseline, 2009						
Classmates	0.0237**				-0.0223	*86,404*
	[0.0095]				[0.1619]	
Coworkers	0.0731***				0.4834***	
	[0.0132]				[0.1446]	
z_snet		0.0318***	-0.0001	-0.0008		
		[0.0116]	[0.0023]	[0.0055]		
z_lnet		-0.0060***	0.0480***	0.0233***		
		[0.0020]	[0.0096]	[0.0063]		
F-statistic (AP)		*7.5*	*55.9*			
B ADOPTION DURING 2009 OVER 2010						*86,404*
Classmates	0.0274***				-0.0581	
	[0.0105]				[0.1832]	
Coworkers	0.0804***				0.3307**	
	[0.0147]				[0.1451]	
z_snet		0.0318***	-0.0001	-0.0019		
		[0.0116]	[0.0023]	[0.0058]		
z_lnet		-0.0060***	0.0480***	0.0162**		
		[0.0020]	[0.0096]	[0.0065]		
F-statistic (AP)		*7.5*	*55.9*			

Notes: Specifications mirror those in Table 4. Adopters during 2009 are those who received YES during 2009, and they are used to construct the numerator of the variables s_net and l_net, number of peer with YES during 2009. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7—: Subsamples by group size

		Reduced Form (1)	Second Stage (2)
A	SMALL SCHOOLS		
	Classmates	-0.0902	-0.045
		[0.2027]	[0.1515]
	Coworkers	0.4121***	0.3763**
		[0.1590]	[0.1475]
	LARGE SCHOOLS		
	Classmates	0.0735	-0.0284
		[0.2520]	[0.5252]
	Coworkers	0.5588***	0.6028***
		[0.1583]	[0.1997]
B	SMALL FIRMS		
	Classmates	-0.2944	-0.2753
		[0.3092]	[0.3344]
	Coworkers	0.0589	0.0654
		[0.1433]	[0.1498]
	LARGE FIRMS		
	Classmates	0.073	0.0818
		[0.1948]	[0.2015]
	Coworkers	0.8694***	0.8154***
		[0.2029]	[0.3032]

Notes: Specifications mirror the baseline specification described in Table 4 and have the same first stage estimates. *N=86,404* in both panels. There are only two regressions in this table and it is constructed as follows. The first step is to estimate a first stage using the sample of 68,589 similar to the one reported in Table 4. Then fitted values for s_net and l_net are predicted using this first stage. Those fitted values and each individual and group controls are then interacted with two dummy variables, D_i and its complement $D_i^c = D_i - 1$. For example, $D_i = 1$ if the individual i belongs to a small schools. Then the small firms panel show coefficients that accompany the D_i dummy, while the large firms show coefficients that accompany the D_i^c dummy. Standard errors clustered at the school x company level, following Cameron, Gelbach and Miller (2006). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8—: Other subsamples

		First Stage			Reduced	Second	
		Classmates	Coworkers	*F-AP*	Form	Stage	*N*
		(1)	(2)	(3)	(4)	(5)	(6)
A	OVER 21 YEARS OLD						86,404
	Classmates	0.0271**	0.0035	*5.8*	-0.0067	-0.3188	
		[0.0116]	[0.0028]		[0.0063]	[0.2534]	
	Coworkers	-0.0049**	0.0476***	*37*	0.0218***	0.4380***	
		[0.0024]	[0.0096]		[0.0073]	[0.1561]	
	UNDER 21 YEARS OLD						
	Classmates	0.0396***	-0.0063*	12.7	0.0086	0.2942	
		[0.0135]	[0.0038]		[0.0098]	[0.2565]	
	Coworkers	-0.0079***	0.0485***	75.9	0.0261***	0.5559***	
		[0.0029]	[0.0109]		[0.0084]	[0.1982]	
B	OVER 5 YEARS OF WORKING EXPERIENCE						*86,404*
	Classmates	0.0305***	-0.0001	*6.8*	-0.0017	-0.0527	
		[0.0117]	[0.0024]		[0.0056]	[0.1736]	
	Coworkers	-0.0056***	0.0480***	*55.0*	0.0241***	0.4955***	
		[0.0020]	[0.0096]		[0.0062]	[0.1457]	
	UNDER 5 YEARS OF WORKING EXPERIENCE						
	Classmates	0.0611**	0.0002	*6.8*	0.0243	0.379	
		[0.0271]	[0.0138]		[0.0262]	[0.4625]	
	Coworkers	-0.0381	0.0523	*20.1*	-0.0274	-0.3243	
		[0.0244]	[0.0319]		[0.0387]	[0.8828]	
C	TECNICO PROFESIONAL						*86,404*
	Classmates	0.0384**	0.0018	*6.3*	-0.0081	-0.2294	
		[0.0158]	[0.0030]		[0.0077]	[0.2224]	
	Coworkers	-0.0051**	0.0479***	*50.2*	0.0198***	0.3991***	
		[0.0023]	[0.0092]		[0.0067]	[0.1435]	
	HUMANISTA CIENTIFICO						
	Classmates	0.0209	-0.0022	*1.7*	0.0077	0.6546	
		[0.0152]	[0.0035]		[0.0080]	[0.8190]	
	Coworkers	-0.007	0.0478***	*31.9*	0.0341***	0.7737***	
		[0.0047]	[0.0122]		[0.0103]	[0.2942]	
D	ADMISSIBLES						*86,404*
	Classmates	0.0333***	-0.0005	*8.9*	-0.0007	-0.0144	
		[0.0120]	[0.0029]		[0.0076]	[0.2167]	
	Coworkers	-0.0078***	0.0469***	*66.5*	0.0327***	0.6905***	
		[0.0027]	[0.0096]		[0.0084]	[0.2009]	
	NON-ADMISSIBLES						
	Classmates	0.0289**	0.0005	*4.8*	-0.003	-0.0917	
		[0.0133]	[0.0043]		[0.0067]	[0.2212]	
	Coworkers	-0.0019	0.0504***	*43.7*	-0.0004	0.0036	
		[0.0030]	[0.0106]		[0.0055]	[0.1017]	

Notes: Specifications mirror the baseline specification described in Table 4. This table was modified in order to fit this page and to save space, see the notes below Table 5. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9—: Other dates

	OLS	First Stage		Reduced	Second	
		Classmates	Coworkers	Form	Stage (2sls)	*N*
	(1)	(2)	(3)	(4)	(5)	(6)
			2009			
Classmates	0.0237**				-0.0223	*86,404*
	[0.0095]				[0.1619]	
Coworkers	0.0731***				0.4834***	
	[0.0132]				[0.1446]	
z_snet		0.0318***	-0.0001	-0.0008		
		[0.0116]	[0.0023]	[0.0055]		
z_lnet		-0.0060***	0.0480***	0.0233***		
		[0.0020]	[0.0096]	[0.0063]		
F-statistic (AP)		*7.5*	*55.9*			
			2010			
Classmates	0.0246***				-0.0306	*86,404*
	[0.0085]				[0.1373]	
Coworkers	0.0756***				0.3033**	
	[0.0124]				[0.1280]	
z_snet		0.0427***	-0.0019	-0.0019		
		[0.0147]	[0.0027]	[0.0058]		
z_lnet		-0.0041*	0.0531***	0.0162**		
		[0.0023]	[0.0105]	[0.0065]		
F-Statistic (AP)		*8.6*	*32.8*			

Notes: The first panel shows the baseline results, while the second panel shows the peer effects during 2010. Specifications mirror the baseline specification described in Table 4. * significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix A

To relax the one peer only assumption, assume that each individual 1 has a specific peer's group N_1 of size n_1 (for example peers 2, 3, 4,...). This reference group or network contains individuals whose adoption may affect 1's adoption decision, and vice versa. Unless otherwise stated, we assume that 1 is excluded from his network, $1 \notin N_1$. This corresponds to the usual empirical formulation when there is more information than in a survey data (*e.g.* Sacerdote (2001), Soetevent and Kooreman (2007), and Bramoullé *et al.* (2009)). In the most simple case, an individual's take up decision may be affected by the mean take up decision of his friends' group.[20] The next step is to sum equations (3) and (4) among the members of the network and divide by n_1. Define $\overline{x}_2$ as $\sum_{j \in N_1} \frac{x_{jg}}{n_1}$, for any variable x, then

$$y_{1g} = \alpha_1 + \beta_1 \overline{y}_{2g} + \gamma_1 x_{1g} + \tau_1 \overline{x}_{2g} + \theta_1 w_g + \eta_{1g} \tag{A1}$$

$$\overline{y}_{2g} = \alpha_2 + \beta_2 y_{1g} + \gamma_2 \overline{x}_{2g} + \tau_2 x_{1g} + \theta_2 w_g + \lambda \overline{z}_{2g} + \overline{\eta}_{2g} \tag{A2}$$

Appendix B

Assume that the endogenous peer effect model $y_{ij} = \bar{y}_{-i,j}\beta + \epsilon_{ij}$ is instrumented with a variable z_j which is constructed using the full sample as $z_j = \frac{1}{N_j} S_j$, where $S_j \equiv \sum_j d_{ij}$ where $d_{ij} = \mathbf{1}\{FPS_i \leq 11734\}$ and assume that $N_j = N$, hence $z_j = \frac{1}{N} S_j$. The instrumental variables estimator for β in a sample of NJ youths in J groups is:

$$\hat{\beta} = \frac{\sum_j \sum_i S_j y_{ij}}{\sum_j \sum_i S_j \bar{y}_{-i,j}} \tag{B1}$$

Boozer and Cacciola (2001) shows that this expression is equal to 1 in the absence of other covariates. The reason is that $\bar{y}_{-i,j}$ in equation B1 can be rewritten as $N\bar{y}_j - y_{ij}$ and then

$$\hat{\beta} = \frac{\sum_j \sum_i S_j y_{ij}}{\sum_j \sum_i S_j [\frac{1}{N-1}(N\bar{y}_j - y_{ij})]} \tag{B2}$$

Notice that the S_j is not affected by the sum over the i subscripts, and the only terms affected are the y_{ij}, then

$$\hat{\beta} = \frac{\sum_j S_j \bar{y}_j}{\sum_j S_j [\frac{1}{N-1}(N\bar{y}_j - \bar{y}_j)]} \tag{B3}$$

[20] This literature assumes that the relevant peer effect measure is the average behavior of the reference group, "but it could be the 90th percentile, or the 10th percentile, or possibly not just the mean, but perhaps also lower variance aids in enhancing individual achievement" (Boozer and Cacciola, 2001). We assume that the correct measure is the average behavior, but it is certainly interesting for further research to study if "one bad apple can spoil the bunch" or other measures.

"This expression is easily seen to equal 1..." (pg. 46). However, in the case of this paper, the instrumental variable z_j is actually $z_{-i,j}$ because it is constructed in a "leave-out" way using only a small subgroup whose FPS lies within a small window around the $x_0 = 11,734$ cut off. Define $left_{i,j} = \mathbf{1}\{FPS_{ij} \in [x_0 - \Delta, x_0]\}$ and $inside_{i,j} = \mathbf{1}\{FPS_{ij} \in [x_0 - \Delta, x_0 + \Delta]\}$, hence in this paper:

$$z_{-i,j} = \frac{\sum_k left_{kj} - left_{ij}}{\sum_k inside_{kj} - inside_{ij}} \tag{B4}$$

Notice that this instrument takes zero or some positive value only within networks with at least one person with $FPS_{ij} \in [x_0 - \Delta, x_0 + \Delta]$, otherwise it takes missing values. Also notice that now the $z_{-i,j}$ is affected by the sum over the i subscripts in equation B2, so the sums cannot be carried out through, and this implies that the coefficient $\hat{\beta} \neq 1$. As a result,

$$\hat{\beta} = \frac{\sum_j \sum_i z_{-i,j} y_{ij}}{\sum_j \sum_i S_j \bar{y}_{-i,j}} \neq 1 \tag{B5}$$

Appendix C

Table C1—: Descriptive statistics of the number of peers inside the window, by network.

	Variable	N	Mean	Std. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)	(6)
A	Coworkers	86,404	46.9	115.9	1	736
B	Classmates	86,404	17.9	18.7	1	134

Notes: This table shows descriptive statistics for the number of coworkers and classmates inside the window of $60,000 CLP, using the estimating sample of 86,404 individuals. Row A shows the number of coworkers with a wages inside the 60,000 window and row B shows the number of classmates with a wages inside the 60,000 window. Column (4) shows the standard deviation, and columns (5) and (6) shows the minimum and maximum values. Figure B2 shows the corresponding Kernel Density Estimates.

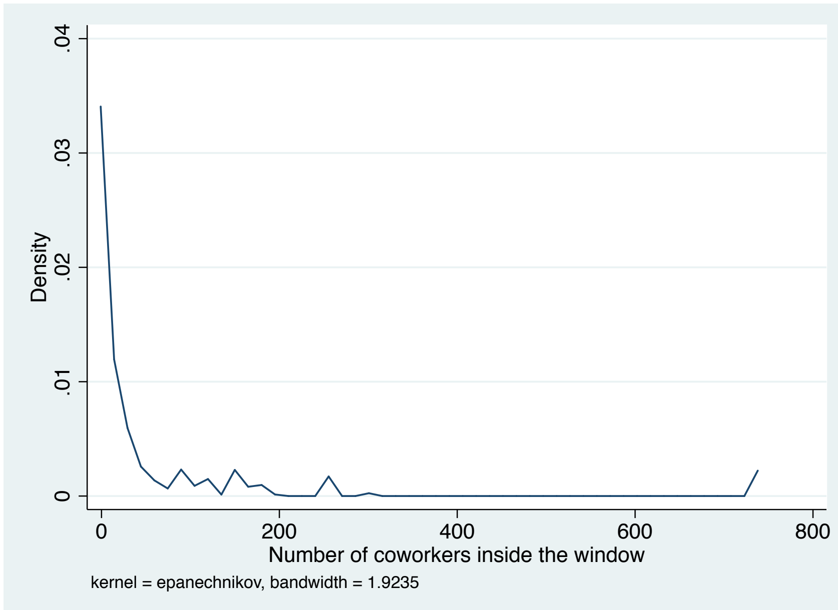
Table C2—: Descriptive Statistics

	Full Sample (1)	With YES (2)	Without YES (3)	Difference (2) - (3) (4)
Age	21.4	22.0	21.4	0.64
	[2.22]	[1.71]	[2.23]	[0.004]
Women	0.50	0.51	0.50	0.01
	[0.50]	[0.50]	[0.50]	[0.001]
Migrant	0.004	0.003	0.004	-0.001
	[0.06]	[0.052]	[0.06]	[0]
Vulnerability Score	8,256	6,733	8,325	-1,592
	[3,890]	[3,034]	[3,910]	[6.4]
N	*8,826,468*	*381,963*	*8,444,505*	
Wage	223,053	213,414	223,936	-10,522
	[143,000]	[75,600]	[147,000]	[242]
Potential YES Payment	30,484	36,294	29,952	6,342
	[16,900]	[12,800]	[17,100]	[28.5]
N	*4,510,280*	*378,532*	*4,131,748*	
Years of Education	11.3	11.4	11.3	0.18
	[2.35]	[2.13]	[2.36]	[0.005]
N	*3,485,751*	*260,445*	*3,225,306*	
1(FPS$\leq$11,734)	0.76	1.00	0.74	0.74
	[0.43]	[0.06]	[0.44]	[0]
1(18<age<25)	0.95	1.00	0.95	0.95
	[0.21]	[0.06]	[0.21]	[0]
Employed	0.51	0.99	0.49	0.50
	[0.5]	[0.09]	[0.5]	[0.001]
1(w<$360,000)	0.45	0.96	0.43	0.54
	[0.5]	[0.19]	[0.49]	[0.001]
Admissible to apply to YES	0.33	0.96	0.30	0.66
	[0.47]	[0.21]	[0.46]	[0.001]
N	*8,826,469*	*381,963*	*8,444,506*	
Fraction of classmates with YES	0.10	0.13	0.10	0.03
	[0.07]	[0.07]	[0.07]	[0]
N	*2,999,945*	*2,701,282*	*298,663*	
Fraction of coworkers with YES	0.11	0.17	0.10	0.06
	[0.16]	[0.19]	[0.16]	[0]
N	*891,344*	*791,627*	*99,717*	

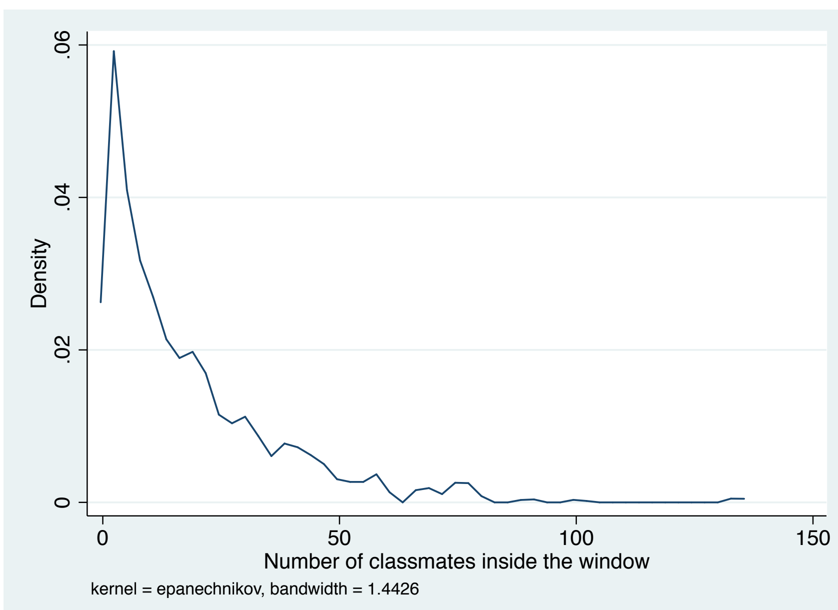
Notes: Standard deviation in square brackets

Figure C1. : Kernel Density Estimates and Descriptive Statistics

(a) Number of coworkers with a wage inside the $60,000 window

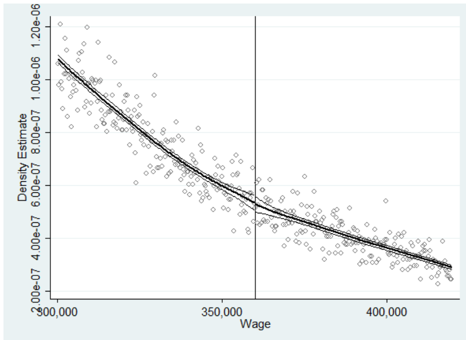


(b) Number of classmates with a wage inside the $60,000 window

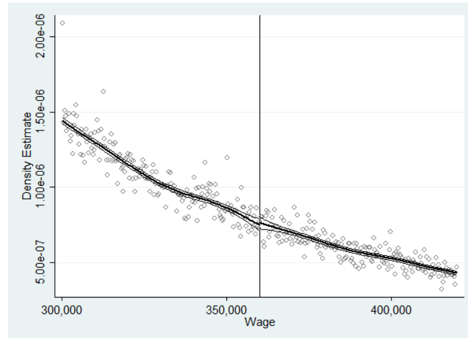


Notes: These figures show Kernel Density Estimates for the corresponding number of coworkers and classmates inside the window of $60,000 CLP, using the estimating sample of 86,404 individuals. Table B1 shows some descriptive statistics corresponding to these graphs.

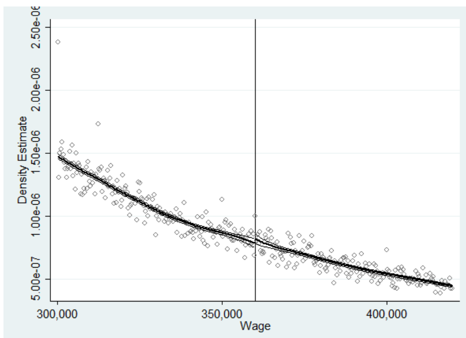
Figure C2. : Density functions of wages around cutoff x_0 for different dates.



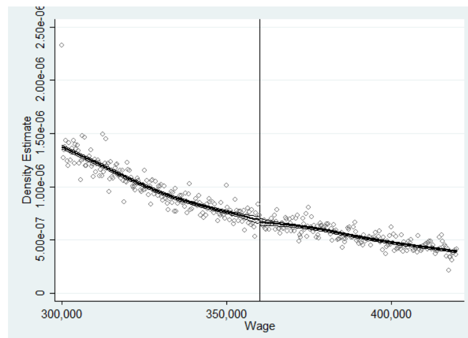
(a) March 2009



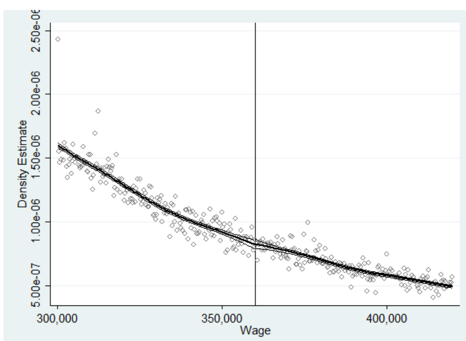
(b) September 2009



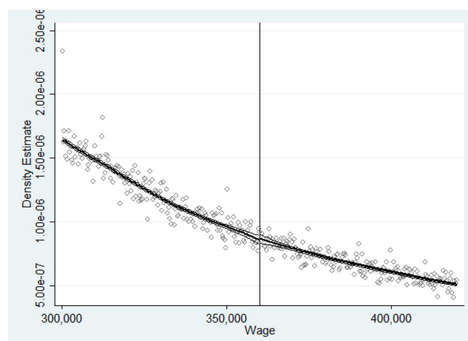
(c) December 2009



(d) March 2010



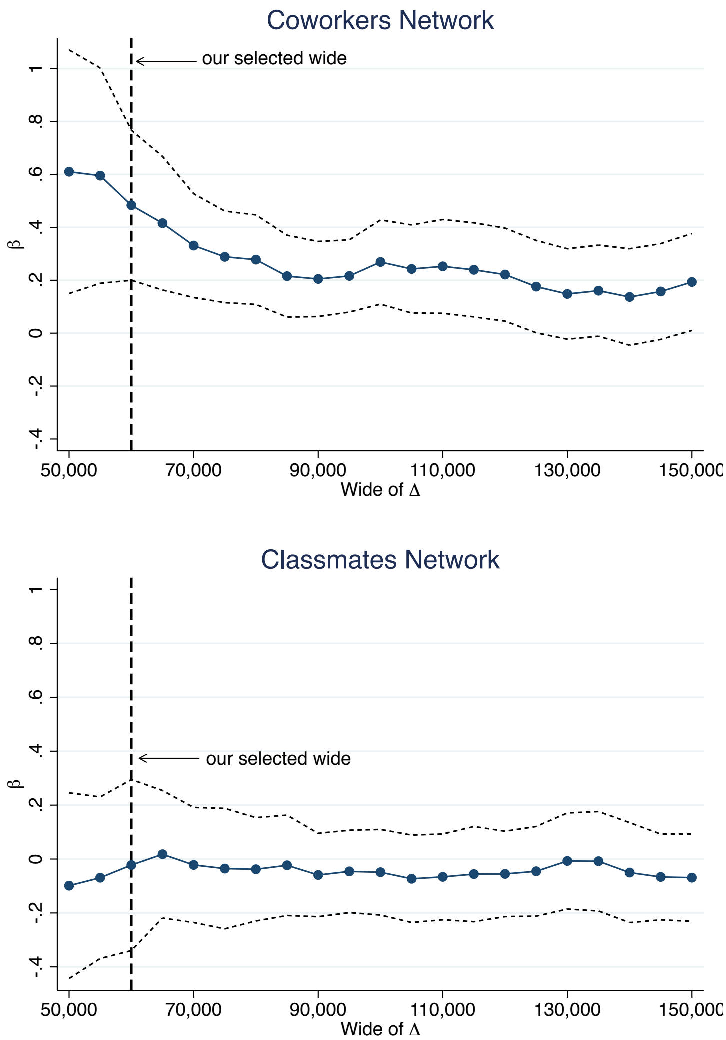
(e) September 2010



(f) December 2010

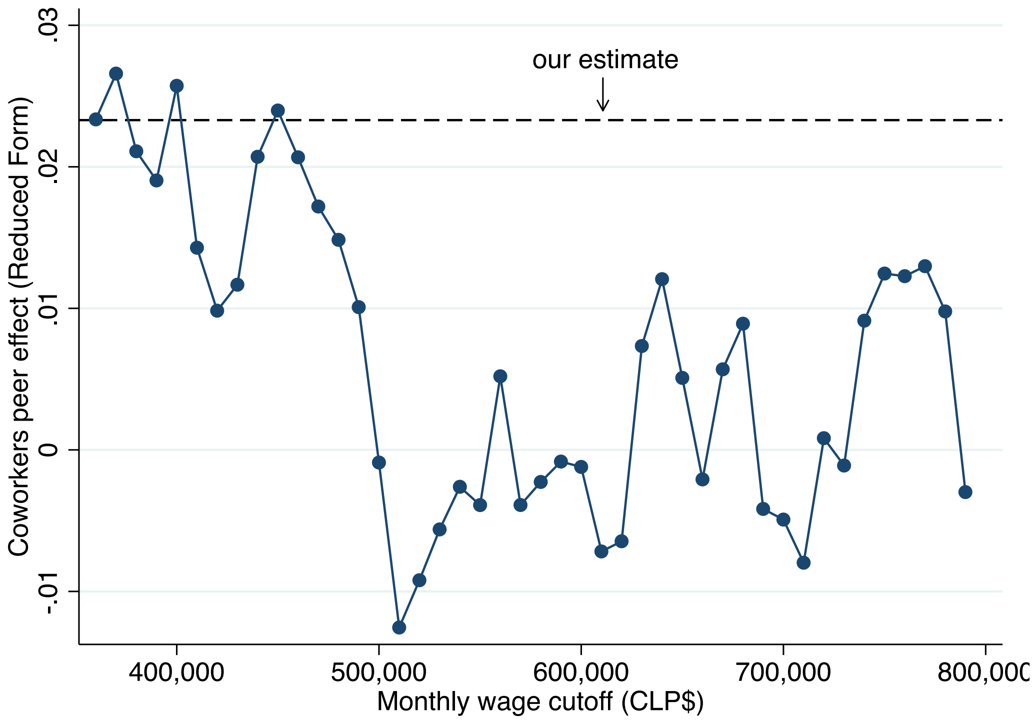
Notes: These figures were constructed using the McCrary (2008) test and they show density estimates of the probability density function for monthly wages and for different dates, around the selected window of width $60,000 CLP. To construct these figures, first we kept only wages inside the selected window and save the bandwidth used by the McCrary (2008) code to construct the graphs, for each date. Then, using the full sample of wages and the saved bandwidth, the graphs were created by limiting the domain to wages within the window.

Figure C3. : Sensitivity of results to different window sizes.



Notes: Each dot on the two solid lines represent the estimated peer effect either β_1 or β_2 from the single 2SLS equation 9 for a given window width. The dots in the upper panel represent the 2SLS coefficient β_2 between y_{1ls} the individual decision to adopt YES and l_net_{2l} the fraction of coworkers inside the window that adopted YES. Similarly, the dots in the bottom panel represent the 2SLS coefficient β_1 between y_{2ls} the individual decision to adopt YES and s_net_{2l} the fraction of classmates inside the window that adopted YES. All regressions include individual controls and a series of dummy variable for *comuna* of residence. The dotted lines on each graph are the 95% confidence interval, with double-clustering standard errors at both school and firm level. The horizontal axis measures the width of the window and the vertical axis measures the coefficients. The vertical line shows our selected width of \$60,000 Chilean pesos.

Figure C4. : Placebo estimates of the peer effects.



Notes: Each placebo estimate first assigns a window around the false wage cutoff, and then estimates a reduced form peer effect at the coworkers level. There are 44 estimates in this graph (from \$360,000 to \$800,000), where each estimate increases the false cutoff by \$10,000. The horizontal axis measures the cutoff wage and the vertical axis measures the coefficients. The horizontal line shows our baseline estimate of 0.0233.