Peer Effects in the Adoption of a New Social Program

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Many social programs have low take-up rates, and little is known about the factors determining this regularity. This paper studies the effects of peers on the adoption of a new Youth Employment Subsidy in Chile. We focus on the effects that high school classmates' and coworkers' adoption has on one's adoption. Identification comes from discontinuities in the subsidy assignment rule inducing exogenous variation in a neighborhood around the worker's age and wage eligibility cutoffs. 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 greater among older adults with about five years of working experience and within larger companies. 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 levels.

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).[1] It has been reported that the differences in take-up rates are due to complexity or

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1. Take-up variance among programs is also large (Currie, 2004). Unemployment Insurance had a take-up rate of 83% (between 1980 and 1982), while Medicaid had a take-up rate of 96% during 2002. Something similar occurs in the United Kingdom, where the Working Families' Tax Credit has a take-up rate of 72% (Currie, 2004), and Income

high costs of learning and application (Aizer, 2007; Bitler, Currie and Scholz, 2003; Blasco and Fontain, 2012; Currie and Grogger, 2001; Dahan and Nisan, 2011; Daly and Burkhauser, 2003), and stigma (Moffit, 1983). 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, focusing on the impact of peers' participation in a social program over individual participation, possibly due to information sharing. Other works have studied this channel (Borjas and Hilton, 1996; Bertrand, Luttmer and Mullainathan, 2000; Duflo and Saez, 2001; Aizer and Currie, 2004; Dahl, Løken and Mogstad, 2014; Carneiro, Galasso, and Ginja, 2014; Figlio, Hamersma and Roth, 2015). However, estimating peer effects remains challenging due to three problems: endogenous group membership, simultaneity, and correlated unobservables (Manski, 1993; Moffitt, 2001). In this paper we propose an original approach to identify an endogenous peer effect, where we combine instrumental variables within a "partial population" approach (Moffitt, 2001)–where some peers within a group are quasi-randomly not given the opportunity to apply, while some others within that same group are left available to apply. According to Moffitt (2001), this reduces troubles caused by the second problem, simultaneity. An advantage of our excluded instrument is that it can easily be replicated at other means-tested programs around the world.

We do so by studying a social program in Chile known as Youth Employment Subsidy (YES). The YES is a twofold monetary incentive for both employed youths and their employers, and is currently an ongoing program. This is an interesting context for several reasons. On one hand, the eligibility rules of this means-tested program are very sharp and allows for a clear identification. On the other hand, data availability makes possible to study endogenous peer effects since the very beginning of the program implementation in July 2009. Also, it also offers an interesting set-up because it is a social program targeted exclusively at vulnerable workers[2].

Moreover, 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. Unlike other social programs around the world, YES has a very straightforward and private application process, which can be completed online at any time and place. This further decreases the application cost, by providing an easy and accessible way to apply[3]. However, despite the low application costs and advertising campaigns carried out at the

Support has a take-up of 64% (Cuclos, 1995). In Norway, Dahl, Løken, and Mogstad (2014) show that the take-up of paid paternity leave for admissible fathers jumped from 3% to approximately 35% in 1993 after the implementation of a reform promoting gender equality.

2. In Chile, vulnerability is related to risk of poverty at the household level. It is measured using a vulnerability score knows as *Ficha de Protección Social* (or FPS), an instrument that allows identifying people and families that are vulnerable or are living in poverty so they can access Government benefits such as the YES subsidy. For a description of the FPS please refer to Herrera, Larrañaga and Telias (2010).

3. According to SUBTEL (2014), in 2009, 31.1% of Chilean households had home internet access; during 2014, this

beginning of the program, the percentage of eligible workers that are taking up the program has remained below 20%, possibly due to peer effects.

To implement our approach, we exploit a unique database that results from merging four different administrative records using a single identification number. The dataset allows us to have access to different kinds of networks. In particular, we focus on an individual's classmates and coworkers before program implementation. 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 social program.[4] We also analyze coworker networks, since literature has provided extensive evidence on their importance over decision making. For example, in referral-based models coworkers, coworkers provide information about available jobs and unknown productivity of workers (Glitz, 2013). Dahl, Løken, and Mogstad (2014) also find important peer effects among coworkers in social program participation. However, we are not able to see other networks (for example, Dahl, Løken and Mogstad (2014) study family members, Aizer and Currie (2004) use neighbors at the zip code level, and Duflo and Saez (2001) study university departments) due to data limitations.

An additional benefit of the dataset and of studying peer effects since the early years of the program implementation is that we can fix networks before the introduction of YES, thus alleviating concerns about the first estimation problem, endogenous group membership. This is not possible in other contexts where the social program has been running for several years because groups' composition before the program started could no longer be relevant. Also, peers and groups' composition could have endogenously adapted to stigma or social program participation in general, biasing the estimates. Since in our case networks are fixed before the introduction of YES, any changes in 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.

Our excluded instrument uses a discontinuity in the eligibility rule of the means-tested program, which provides random variation around the worker's wage cutoff. In particular, in order for a worker to be characterized as eligible his monthly wage must be below CLP \$360,000. This rule induced quasi-experimental variation that comes close to an ideal experiment. Within each group we define our excluded instrument as follows.

We start by selecting a subsample such that all groups have at least one peer whose wage

amount increased to 61.6%. According to CASEN 2011, almost 80% of young Chileans between 18 and 25 years old had internet access, and in average 65% of Chileans among the lower quintiles of income had internet access.

4. 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 (2016) finds that the admission to an elite program raises the number of leadership positions at companies 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 young adults in transmitting information about social programs.

is inside a narrow interval around the cutoff (we call this interval, "the window"). We then use as an instrumental variable the fraction of eligible peers whose wage is inside this window. This variable is then used as an instrument for the endogenous peer effect. The idea is that for a 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.

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 our instrumental variable. So the instrument is likely to be orthogonal to several variables, including contextual peer effects, and quasi-randomly distributed among the population, and hence independent of unobservable shocks at the group level. This reduces the third problem, correlated unobservables.

Then, we use a "partial population" approach in which we measure how mean adoption of YES by peers whose wage is inside the window affect the individual adoption of eligible peers whose wage is outside the window. We find that coworkers played a significant role in determining program participation during the early months of YES implementation, but the classmates were not relevant. Each eligible peer outside the window was 0.16 percentage points more likely to adopt YES if they had a 10 percentage points higher fraction of eligible coworkers inside the small window. The results show that, among coworkers inside the window, a 10 percentage points higher fraction of eligible coworkers increased the fraction of coworkers with YES by 0.6 percentage points during 2009. This implies a peer effect estimate of 2.7 percentage points. First-stage estimates hold at the 1% significance level and have an F-statistic above 10. On the other hand, each eligible peer outside the window did not significantly change the probability of adopting YES in response to a 10 percentage points higher fraction of eligible classmates. Yet, the first-stage estimates hold at the 1% significance level and have an F-statistics above 10. Among classmates inside the window, a 10 percentage points higher fraction of eligible classmates increased the faction of classmates with YES by 0.4 percentage points. The results are robust to a variety of control variables.

In analyzing the mechanisms that drive the peer effects, the second set of results shows that an informational channel is a valid candidate for explaining the results. First, peer effects are more important among older adults over 20 years old, and among adults with over 5 years of experience. This is an expected result because older adults have more working experience and the interaction with other coworkers is more likely to be stronger compared to younger adults. Peer effects are also higher among admissible peers within larger companies. Finally, we look at the evolution of the peer effects over time. To do so, we fix the first-stage during 2009 and study peers that did not adopt during 2009–this includes non-adopters. We find evidence that the peer effect is sustained over time. This also provides additional evidence that simultaneity or reflection problems are not driving our results.

This paper contributes to the current state of knowledge in three ways. **First, many papers have examined the take-up of social programs and have provided explanations for their low adoption rates (Bertrand, Luttmer and Mullainathan (2000), Duflo and Saez (2001), Aizer and Currie (2004), Dahl, Løken and Mogstad (2014), Carneiro, Galasso, and Ginja (2014), among others). However, these authors study social programs many years after their date of implementation. As a result, there is little knowledge about the role of peers during the early years of the implementation phase. In this paper we study endogenous peer effects from the exact date of YES implementation in July 2009, which naturally takes data into consideration for the rougher years of early implementation. Although these years may not reflect the linear goal of social programs, the benefit of including this data is that peer effects during the implementation phase could behave differently due to information sharing, and steady state take-up could depend on the initial take-up.**

Second, other papers have used a partial population approach with instrumental variables to identify endogenous peer effects. But they do so in very different contexts to social program participation such as education (Bobonis and Finan, 2009; Figlio, 2007), labor market participation (Maurin and Mischion, 2009), teen pregnancy (Monstad, Propper and Salvanes, 2011), migration (Munshi, 2003), disability pension participation (Rege, Telle and Votruba, 2012), crime??. In this paper we are using this approach in the social program participation literature. So far only Dahl, Løken and Mogstad (2014) have used a similar approach in order to identify the causal impact of peers' participation in a social program over individual participation. In their case, they exploit a change in one of the eligibility rules of a paternity leave program in Norway. Their excluded instrument is defined within a regression discontinuity. They recognize (pg. 2053) that using an RD approach for the purpose of estimating a peer effect involves a set of challenges because multiple peers in a network can affect the same individual. In our case, we do not use a regression discontinuity and instead we fix an average peer in a network and study how it can affect each individual.

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 IX concludes.

II. The YES and Peer Effects

A. *What is the YES social program?*

The Youth Employment Subsidy (YES) program was first announced in March 2009, and was officially launched in July 2009[5]. The launching campaign consisted of field visits of the Minister and entire team of the Ministry of Labor and Social Welfare, occasionally including the Finance Minister. The campaign was also complemented with advertising placed on television, national and regional radio, national and local newspapers, subway, public transportation in Santiago and their bus stops (Huneeus, 2010).

The YES is a two-fold, monetary incentive targeted at workers in the 18-24 age range belonging to the poorest fourth decile of the population, as measured by the *Ficha de Proteccin Social* vulnerability score (or FPS score, see footnote 2 for a description) 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 on-line system. The implementation and administration of YES are administered by the Service of Training and Employment (SENCE) from the Ministry of Labor and Social Welfare, as well as the Social Security Institute (IPS) and the Internal Revenue Service.

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

1) to declare a gross annual income less than CLP$4,320,000 (US$8,640) [6] in the calendar year (or a gross monthly income less than CLP$360,000 (US$720)) in which the benefit is claimed by those who request provisional monthly payments;

2) be between 18 and 24 years old;

3) 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 score;

4) not be working in a state institution or a company with a state contribution higher than 50%[7]; and

5. A web search among national newspapers of terms related to the YES showed only one publication regarding the YES before the month the program was launched: "Subsidio al empleo beneficiar a 300 mil jvenes de bajos recursos", El Mercurio, January 25, 2009. We think that this news was not influential enough given that Google Searches on several terms related to YES does not show any increase until July 1st, 2009.

6. According to the law, all quantities in Chilean pesos are adjusted each year for the annual variation experienced by the Chilean Consumer Price Index.

7. The AFC database only includes private companies and not public companies. In Chile, companies 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 companies (0.3%) had a state contributing higher than 50%. For this reason, the empirical strategy will not take into consideration this point.

5) have social security contributions paid up to date.[8]

The benefit is kept 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 continue receiving the subsidy.

B. Why are peer effects a good explanation of YES's low take-up?

There are several reasons why we believe that peer effects are better at explaining the low take-up rate of YES than other alternative explanations. In general, the low YES take-up was probably due to ignorance of the existence of the social program at the beginning of program implementation.

Application. First, unlike other social programs around the world who might use complex application processes to avoid rejecting eligible candidates or awarding the subsidy to ineligible candidates (Kleven and Kopczuk, 2011), the Chilean government simplified as much as possible the YES application process. The process is simple and can be carried out at any time, from any place, via the internet. Workers and employers who want to apply must follow independent application processes that require independent information. First, they must log into the website www.subsidioempleojoven.cl. Once the worker's ID number is entered, the system immediately reports whether or not the requirements have been met. In an affirmative case, a simple and short registration form would then have to be filled out and submitted. The application status can be verified at any time by accessing the YES website. If the applicant believes the system is displaying incorrect information, he or she can contact SENCE from the same website.[9]

Paperwork. Moreover, this online application process reduces the paperwork considerably, simplifying several procedures. 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 provide documentation for medical licensing since that information is internally audited by the Social Security Institute (IPS). **Paperwork is necessary in very specific cases, but in all of these cases the required documentation may be electronically uploaded to the website.** This implies that complexity or high application costs are not the best explanations of the low take-up rate (Aizer, 2007; Bitler, Currie and Scholz, 2003; Blasco and Fontain, 2012; Currie and Grogger, 2001; Dahan and Nisan, 2011; Daly and Burkhauser, 2003).

Payments. Second, if the application is successful, the bonus will begin to be paid within a maximum period of 90 days after filing the application. Hence making very short

8. Fortunately, workers that appear in the AFC database are only those whose social security contributions are up to date.

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

the waiting period between the time when a person applies and when she gets the first payment, so time-inconsistency (ODonoghue and Rabin, 1999) should not be a problem. The benefit will be accrued from the first day of the month following the application submission date.

Payment is annual by default and is dispersed in the second half of the next calendar year, gauged by when wages and employment is earned. However, the dependent worker may opt for monthly payments while submitting the application, yet can change his or her choice only once during the year.[10]

In both cases, the subsidy is paid by the payment method indicated by the employee at the time of completing the application: either in cash or deposited in a bank account. Meaning the payments are private information which is hard to find out by other colleagues and cannot be revealed unless a YES recipient explicitly confess receiving payments, making stigma a bad explanation (Moffit, 1983; Riphahan, 2001).

The benefits levels (Anderson and Meyer, 1997; Riphahan, 2001) of YES are quite substantial. For some workers, having YES means receiving more than an extra monthly wage per year. Table 1 shows the amount of the subsidy to which the worker is entitled 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 does not exceed \$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, minus 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 amount of the subsidy will follow a similar scheme to the annual one, which is described in Panel B of Table 1.

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

Panel A Annual Gross Income (AI)	Subsidy (YES)
AI≤ \$1,920	$0.2 \times (AI)$
\$1,920<AI≤\$2,400	\$384
\$2,400<AI≤\$4,320	\$384-0.2×(AI-\$2,400)
Panel B: Monthly Gross Income (MI)	Subsidy (YES)
MI≤\$160	$0.2 \times (MI)$
\$160<MI≤\$200	\$32
\$200<MI≤\$360	\$32-0.2×(MI-\$200)

Note: figures in thousands, CLP of 2009.

10. According to Rau and Bravo (2015), in the 2009-2010 period, about two-thirds of the YES beneficiaries opted for monthly payments.

End of Payments. Finally, it is important to mention when payments end. Entitlement to the subsidy ceases for months in which the worker's employer fails to pay his/her social security contributions (or pays late)–a common occurrence in Chile–, 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 particular groups of workers have the right to request additional time to access YES. The first consists of workers who have completed regular studies in a higher education institution and are between 18 and 25 years old. The second consists of mother, who may request an extension for each child born alive within three months of her 25th birthday.

Workers whose employment relationship ended while they were receiving the subsidy do not need to submit a new application at the time their new job begins. To the extent that they meet the requirements, they will keep the benefit and will be paid as soon as the new employer's pension contribution payments 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 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 the decision simultaneously.

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 eligible to YES, and consider an experiment where his peer's 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 YES changes sharply based on the monthly wage due to the discontinuity in the assignment rule–eligible workers must have a monthly wage below CLP \$360,000. **Figure 1 shows mean take-up rates in bins of 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; the take-up also jumps sharply form 16% to less than 2% at the cutoff.** Thus, we get quasi-random variation in eligibility for YES for peers whose wage is inside a very small window of size Δ

around the cutoff x_0. This quasi-random variation is valid only if individuals are not able to perfectly manipulate their wages to sort at the left of the cutoff–Subsection V.B tests this assumption.

In the simplified version of one peer only, we first define a narrow window of size Δ around the wage cutoff x_0. Then the instrument is defined as a dummy variable that takes the value of 1 if the wage of peer 2 is at the left of the window, and 0 if the wage of peer 2 is at the right of the window[11]. This instrument is defined only for peers whose wage is inside the window $[x_0 - \Delta, x_0 + \Delta]$. Formally,

$$z_{2g} = 1\left(wage_{2g} \in [x_0 - \Delta, x_0]\right)$$

where $1(\cdot)$ is an indicator function, and $wage_{2g}$ is the wage of peer $2g$ during December 2008.

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.

The approach described above has to be implemented in a setup where there are many individuals affecting one. **This can easily be taken into account by first restricting the sample to groups who have at least one peer 2 whose wage is inside the window–in subsection VI.A we further restrict peers 2 to those whose vulnerability score is below 11,734 cutoff and whose age is within the 18-25 year range. Then, we sum equations (3) and (4) among the members of the network and divide by the size of the group–see Appendix B for a derivation under these conditions.**

Formally, assume that each individual i has an specific peer's group N_i of size n_i, and define $\overline{h}_i$ as $\sum_{j \in N_i} \frac{h_{ig}}{n_i}$, for any variable h_i. The instrumental variable is now defined as the fraction of eligible peers at the left of the window:

$$\overline{z}_{2g} = \sum_{j \in N_2} \frac{1\left(wage_j \in [x_0 - \Delta, x_0]\right)}{n_2} \tag{5}$$

where N_2 is the group of peers with wages inside the window $[x_0 - \Delta, x_0 + \Delta]$, of size n_2. The window size used is $\Delta = 70,000$, but Figure A3 studies how sensitive the results are for the chosen window width. 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.

Subsection V.A shows that, first, the instrument given by equation (7) is randomly distributed among the population given our $70,000$ chosen window width, then uncorrelated to other variables. Second, that the only way that the instrument affects individual take-

11. This is not the same as the compliant subpopulation; the compliers are peers whose wage is within the interval and who adopt when they are admissible.

up is through its effect on peer's adoption. Lastly, that individuals did not manipulate the assignment variable into treatment.

We then measure how adoption by 2 affects the individual adoption of all peers whose wage is outside the window and below $x_0 - \Delta$, labeled 1 in equation (3). The peer effect can then be identified by the following two-equation system, subtracting from control variables:

$$y_{1g} = \alpha_1 + \beta \overline{y}_{2g} + \eta_{1g} \tag{6}$$

$$\overline{y}_{2g} = \alpha_2 + \lambda \overline{z}_{2g} + \eta_{2g} \tag{7}$$

Notice that $\overline{z}_{2g}$ only affects 2 in equation (7) and not 1 because the wages of all the peers in group 1 are below $x_0 - \Delta$, so they are all eligible.

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 peer's **2** eligibility changes the individual take-up behavior of the coworker or classmate (assigned the label 1):

$$y_{1g} = \alpha_3 + \pi \overline{z}_{2g} + u_{1g} \tag{8}$$

The equations above can include controls at the group level $\overline{x}_{2g}$ and at the individual level x_{1g}. As is well known (Imbens and Angrist, 1994) the Wald estimand can be interpreted, under a set of assumptions, 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. Appendix D expands this approach a little further to allow for comparisons between networks.

This approach is similar to Dahl et al. (2014), but there is one main difference. While they have networks with only one peer in the window affecting the choice of many individuals, we have networks with one or more peers in the window affecting one person's choice.

How we define y_{1g} and y_{2g}, or adoption, is important, given that YES recipients can lose the benefit if some of the conditions explained at the end of Section II are met. We assume that a person's knowledge about YES existence–not necessarily about how it works–is kept over time after the first payment is made, even if the person stops receiving the benefit. Once infected, the person remains infected for the following years. Notice that we are underestimating knowledge about YES because we cannot observe unsuccessful applicants. We assume this because we are interested in studying if information shocks, and not other things, lead to peer effects. This is a reasonable assumption as well, because it is hard to think on other reasons that might lead to peer effects if peers are not communicating between them.

Since we do not have a database of unsuccessful applicants, we only observe people who applied to YES and did receive it. This means that we cannot

observe the characteristics with which people are applying to YES, for example the application monthly wage. We overcome this issue of unobservability by fixing the wages in one moment in time before the introduction of YES (initially in December 2008). This means that the instrument exploits the composition of the groups before the implementation of the YES.

We are not exploiting the cutoff in a regression-discontinuity design for several reasons... MANY TO ONE... because the results were not conclusive–the magnitude and significance of the peer effect depended on the assumptions we made. The next section describes our dataset.

IV. Data

A. Data

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 the socio-economic characteristics of families that are used by SIMCE to calculate the FPS score, a proxy-means score that determines social program eligibility. The FPS score is one score for all household members ranging 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, educational level, *comuna* of residence[12], the FPS vulnerability score, and an indicator if the person was born in Chile. However, data includes only individuals between 18 and 25 years old.

The AFC database contains matched employer-employee data with 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 (excluding the public sector–see footnote 6). It includes information on each employee's total taxable income, as well as 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 an individual's employment history and compare it to another's. Finally, from the AFC database we can deduce information about the employment firm, such as the number of employees, among other details.

At the time the data was delivered to us by the Ministry of Social Planning, the

12. Information on the *comuna* of residence is coded in a way that hides where each person lives. We can only know that two individuals live in different/same areas since their *comuna* of residence codes are different/the same.

AFC database was already merged to the FPS dataset described above. Notice that this "FPS+AFC" database provides 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 peer 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 higher education) officially recognized by the Ministry of Education. The variables requested in this work include the student's nameless and masked ID, a masked and nameless ID of the educational establishment where the student is registered (RBD), a study plan[13] , and grade and classroom codes. Data provided is between 2002 and 2012, for all individuals registered in those years. The RECH database allows for very precise identification of the classmates and schoolmates of a person, at different educational institutions, years and grades.

Finally, the YES database contains annual administrative lists of all monthly and annual payments of YES made by SENCE. It contains a nameless and masked ID for the YES recipient, an indicator of monthly or annual payment, and the 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, while column (2) presents the estimating sample. There are differences that are statistical significant, but the differences are very small. 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 score of 9,235 points (there is no comparison point at the national level but the full universe has an average score of 9,000). The mean wage in our estimating sample is CLP$181,600, which is lower than the national mean wage of CLP$355,771 according to CASEN (2011). The mean years of education is 12, which is quite higher than the national mean of 9.5 years of schooling during 2012 (INE, 2012). Only 67% had an FPS score lower than 11,734, but most of them had a wage lower than the $4,320,000 threshold; as a result 65% were eligible to receive YES. Appendix Table A2 shows more descriptive statistics for the estimating sample. Column (1) uses the sample of workers with YES during 2009, and column (2) uses the sample of employees without YES during 2009. Column (3) 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. The most interesting fact of all is that the mean fraction

13. The study plan refers to Educacin Parvularia, Enseñanza Básica, Educación Especial, Enseñanza Media Humanista Científica, Enseñanza Media Técnico Profesional and Enseñanza Media Artística.

of coworkers with YES is 18% higher among YES recipients than among non-recipients–77% versus 58%. The same thing happens with the fraction of classmates with YES, which is 2.3% higher among people with YES. This suggests that there could be some social multiplier in the adoption of YES. This is precisely what the next section attempts to quantify.

B. Sample Selection and Networks

The rich dataset allows us to focus on two kinds of networks or groups (g), coworkers ($g = c$) and classmates ($g = s$). We analyze coworkers because related literature has provided extensive evidence on their importance to decision making. For example, in referral-based models, coworkers provide information about available jobs and unknown productivity of workers (Glitz, 2013); and Dahl, Løken, 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, Løken and Mogstad (2014) study family members, Aizer and Currie (2004) use neighbors at the zip code level, and Duflo and Saez (2001) study university departments). On the other hand, this is the first paper, to the best of our knowledge, in the take-up literature to study the effect of classmates on the adoption of a labor social program. 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 (2016) finds that the admission to an elite program raises the number of leadership positions that students hold at companies by 50%, though 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. Finally, it is interesting to compare their importance, since the information sharing within each network can be quite different. We can expect coworkers to talk more about work-related topics while classmates may discuss day-to-day problems.

Moreover, the rich dataset allows us to track individuals before the introduction of YES so we can construct a coworkers and classmates network from before the subsidy started. Hence, we 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 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 lessens concerns of endogenous group membership because it does not create bias.

We start by fixing coworkers at a point in time by taking a cross-sectional cut of the FPS-AFC database for December 2008 (subsection VI.A presents robustness checks for other months). 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 grade twelve (known as *cuarto medio*) before 2009–the year when the subsidy started. Two persons i and j are classmates if they were enrolled in grade twelve, at the same educational institution in the same year. Thus, they are not only schoolmates, but same generation classmates.

We finally merge these two datasets together and then merge the result with the YES payments database. Once complete, we merge this with the December 2008 dataset to obtain specific information on the characteristics of these people, including wages in particular.

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[14]. Then, the coworkers of an individual are defined according to the last employer he/she had before the introduction of YES.

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

Some other considerations are in order. Firstly, the AFC dataset does not contain information on the monthly wage (w), so we must estimate this. We follow two different approaches. Firstly, we estimate it by dividing the total taxable income in the last 6 (income6) and 12 (income12) months by the number of months with taxable income in the last 6 (months6) and 12 (months12) months, respectively. Then, we take the lowest value between both (w = min{ income6 / months6, income12 / months12 }). In Table 5 we change this definition as robustness check. Secondly, in baseline estimation of 7 and 6 we use the FPS-AFC database from December 2008 to obtain the individual characteristics from this dataset, including wages. 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 imple-

14. This is an important point since, ccording to Sojourner (2013), this can render biased and inconsistent estimates.

mentation. If individuals cannot perfectly manipulate their wage, the aggregate distribution of the assignment variable should be continuous around the cutoff value.

In Figure 2, we both 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 cutoff. In Appendix A2, 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 assignment throughout the population. In other words, companies 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 those 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 classmate and coworker levels on both instrumental variables and report results in Table 3. These regressions are similar to equation (7); instead of using $\overline{y}_{2g}$ at the left side, we take the mean of characteristics specified in each row at the level of the coworkers in column (1) and classmates in column (2). As we can see, most of the coefficients are small and not significant, especially for the coworker network. This suggests that the instruments were quasi-randomly assigned among groups. There are some differences in age and sex, so we include the mean age and sex as additional controls as robustness check in Table 5.

VI. Results

Table 4 is meant to solve the estimation problems described above and to establish causality. In this table, we initially use the December 2008 database to define networks and obtain covariates. We measure adoption during any month throughout the year of 2009, regardless of the order (subsection VII changes this). All regressions in this table include, as control variables, peer's 1 age, age^2, sex, FPS score, Ln(wage), and years of education. Results from an OLS regression of equation 6 (not in this table) show that each peer 1 is 0.8 pp more likely to have YES when the fraction of his classmates or coworkers with YES increases by 10 pp (significant at the 1% level).

Column (1) shows first-stage estimates from equation (7). There is a very strong first-stage for both coworkers and classmates with F-statistics above 10. The estimates show that having a 10 pp higher fraction of eligible coworkers increased the fraction of coworkers with YES during 2009 in 0.59 pp, and in 0.44 pp for classmates.

Column (2) shows reduced form intention to treat (ITT) estimates. This column shows that each peer is 0.16 pp more likely to adopt YES if they have a 10 pp higher fraction of eligible coworkers. On the other hand, there is no significant change in the probability of adopting YES if the individual have a 10 pp higher fraction of eligible classmates.

Finally, column (3) shows the Wald 2SLS estimates of equation (6). These coefficients measure the average causal peer effect of the coworkers' and classmates' adoption on individual adoption. Peers with a 10 pp higher fraction of coworkers with YES are 2.7 pp more likely to adopt YES. However, a 10pp higher fraction of classmates with YES do not change the individual likelihood of adopting YES.

Given that the mean take-up rate among this sample was 9%, the peer effect translates into a 30% increase in take-up. This coworkers' peer effect is very important in size. For example, it is almost two times Dahl, Løken and Mogstad (2004)'s effect whose point estimate is 0.11, this implies a 16% take-up increase. The different magnitudes may be expected since the features of the YES program make the application process simple and cheap. Hence, admissible workers have a lot to gain from finding out information concerning the existence of YES. The next subsection tests how robust these results are.

A. Robustness Checks

Table 5 summarizes several robustness checks. The first robustness check excludes all individual controls. 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 cutoff and, hence, assures that it is independent of other covariates.

The next robustness test includes the mean age, sex, vulnerability score, and years of education $\overline{x}_{2g}$ of peers inside the window as covariates. The first-stage coefficient decreases a little. We then include *comuna* fixed effects according to peer's 1 *comuna* of residence. A *comuna* is the lower and primary administrative division in Chile and corresponds to what is known in other countries as a municipality. The *comuna* is a division for local government purposes only, because the state government only has jurisdiction at the regional and provincial level in Chile. This is interesting because, despite the YES program was managed by the state government institution SENCE, some municipalities could have had characteristics which 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* level. Results remain unchanged.

Next, we change the standard errors clustering to the *comuna* level instead. Then, since we cannot observe application wages and monthly wages, we change the wage definition. Finally, we run the first-stage regression on a sample conditional on peers 2 being admissible in both vulnerability score and age. This increases the first-stage coefficient, as expected.

Figure A3 represents evidence on the robustness of the results to the chosen window width. Each dot in the window presents a peer effect from 2SLS estimates for the given window width, including both coworkers and classmates. As shown, the peer effect is quite stable around our estimate. Figure A4 presents results on placebo estimates where each estimate assigns a false wage cutoff, and then estimates a reduced-form peer effect at the coworkers level. As can be seen, it is hard to obtain a point-estimate similar to ours just by chance.

B. Mechanisms

The results in section VI show significant peer effects from coworkers. In this context, these peer effects are likely due to informational benefits which translate into adoption. One information channel is the program's initial information campaign. However, knowledge about the existence of the YES program would not automatically translate into adoption if the application procedure, admissibility rules, and other details are not understood. For example, during November 2014 the Minister of Labor did a media campaign encouraging the 122,000 YES beneficiaries of YES to claim USD$37,029,382 unclaimed payments. The accumulation of unclaimed payments is sometimes due to unfamiliarity with how YES works[15]. Unclaimed payments for some individual cases summed up to USD$2,067[16]. In total, during November 2014, 1,800 young adult workers could have claim amounts higher than USD$1,378[17].

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 participation benefits and costs is quite limited. However, the admissibility requirements allowed some networks to quasi-randomly have more/fewer peers around a small window of the cutoff while leaving everyone else intact. The results show that this exogenous increase in the fraction of admissible peers that are quasi-randomly allowed to apply translated into a higher adoption of YES inside each network, hence potentially reducing uncertainty about the costs and benefits.

With the current data, it is not possible to explain what these young adults were discussing among peers and the type of information that was being transmitted inside each network. Classmates might be talking more about YES existence, while coworkers may provide more details about its costs and benefits, for example. Despite this, there are several subsamples that we can study. The first subsample is older versus younger adults. Older adults have more working experience and have spent more time in contact with their cowork-

15. As explained before, a person does not lose the benefit when he/she changes jobs, but the payments are held until she becomes eligible once again. At this time, payments resume automatically.

16. "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

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

ers than younger adults, allowing them to building strong ties with their coworkers. Hence older adults are more likely to keep a closer interaction or appeal to their coworkers when they seek information about social programs. Table 9 tests if peer effects among older workers are stronger. The results show that coworkers are important only among old workers with over five years of experience.

Table 9 also shows results using subsamples of small and large companies. Small companies are those with less than 50 workers. Table 9 shows that the coworkers' peer effects are concentrated in companies 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 companies.

C. Networks with weaker and stronger ties

Table 6 tests the existence of peer effects in networks with weaker and stronger ties. First, we look at peer effects among coworkers during December 2007 (one year before baseline) and find no peer effects among these coworkers. However, peer effects are stronger among recent coworkers during March 2009. On the other hand, we expand the definition of classmates to schoolmates–in this case two persons are schoolmates if they graduated from the same school, regardless of the year. We also restrict the classmates to those graduating recently during 2008. In general, there is no peer effect among classmates or schoolmates.

VII. Reflection and Other Dates

In Table 7 we study another potential concern: reflection. If everyone inside a network is adopting YES at the same time, then a statistically significant peer effect could be found even though peers are not necessarily influencing each other to adopt YES by talking about the program or the application process. It just happens that all of them are applying at the same time. To tackle this issue we study how take-up by 2009 early adopters affect the probability of adopting YES during 2010. To do so, we keep the same first-stage estimates and study take-up during 2010 by changing how y_{1g} is measured: we drop from our sample those individuals that received any YES payment during 2009. This implies that we work only with peers who either received at least one YES payment after their classmate or coworker during 2010, or either did not received any YES payment during the 2009-2010 period. Remember that we measure adoption as a stock: a person's knowledge about YES is kept even if that person stops receiving the benefit payments. Table 7 shows that a 10 pp increase in the fraction of coworkers with YES during 2009 increased the individual probability of adopting YES during 2010 by 2 pp; classmates remains insignificant.

But, does this effect hold over time? There is no reason to believe that peer effects will remain constant over time, as they may increase or decrease and shift in importance between one network or another. 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, including details of its application procedure and costs and benefits. However, afterward, as people learn about the program, peers become less relevant. Second, peer effects can follow a herd behavior where peer effects are important at the beginning, when advertising was undergoing, leading to other peers to adopt. But as advertising campaigns decreases, the amount of time devoted to YES in the conversations also decreases. Moreover, peers can shift in importance between networks and may depreciate over time. For example, classmates can become less important as people get older, since the classmates definition is fixed here. But this does not necessarily mean that people stop learning about YES existence or about other details of its application procedure, costs, or benefits. Bravo and Rau (2013) find that admissibility to YES had an impact on formal employment, though this effect decayed over time. As a result, adopting YES several months after its implementation becomes less relevant.

In Panel A of Table 8 we study how take-up by 2009 early adopters affect the probability of adopting YES during the following years. Similar as before, we keep the same first-stage and we work only with peers who either received at least one YES payment after their classmate or coworker during 2010 (or 2011, 2012), or either did not received any YES payment during the 2010-2012 period. This variation over time provides interesting evidence on the behavior of the peer effects and how they adapt over time. There is a small change in the first-stage coefficient because we are dropping from our estimating sample people who was already receiving the YES.

The results 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 2 pp, and coworkers peer effects are statistically insignificant by 2012. The classmates peer effect remain insignificant, even when the first-stage remained strong. The accumulated effect is a 2.5 pp increase in the probability of adapting YES at anytime during the 2010-12 period. Given that the mean take-up rate among this sample was 17.6%, then increasing the fraction of coworkers with YES by 10 pp increases take-up by 14% over the long run.

VIII. Using the Age Cutoff

In this section we consider using age as another instrumental variable. Age is harder to manipulate than wages or the vulnerability score. We exploit the fact that people had to be between 18 and 24 years old at the time of application in order to get YES. The subsidy is kept as long as the beneficiary's age is within this range. This means that people who are 24.9 years old are eligible to receive

the subsidy, but people who are 25.1 years old are not. These are not our baseline results for one clear reason: there is a big correlation in ages among classmates, making the age instrument not properly defined for this network. This means that we cannot compare the coworkers peer effect to the classmates peer effect. However, we consider that age is more difficult to manipulate than wages or the vulnerability score, which make these results interesting.

Formally, the window is now defined around the age cutoff of 25, and the instrumental variable is defined as the fraction of peers whose age is at the left area inside the window:

$$\overline{z}_{2g} = \sum_{j \in N_2} \frac{1\left(age_j \in [x_0 - \Delta, x_0]\right)}{n_2} \tag{9}$$

where N_2 is the group of peers whose age is inside the window $[x_0 - \Delta, x_0 + \Delta]$, of size n_2. 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. Next, we describe how x_0 and Δ were chosen.

Initially, the chosen window size was $\Delta = 300$ days–Figure D3 studies how sensitive the results are for the chosen window width. Figure D1 shows no manipulation at the cutoff, despite some seasonality.

To calculate the age of each person we first have to take into account that we only observe the age at application of successful YES applicants that are receiving payments. We do not have data on the age at application of unsuccessful applicants. In order to overcome this issue, we use the fact that people 25 years old or older by August 2009 would never by eligible to get YES. We start by taking the same cross-sectional cut as before, December 2008. Figure D2 shows the fraction of people taking YES in one week bins, by date of birth and for the December 2008 cross-sectional cut. As can be seen from this figure, mean take-up jumps close to July 1, 1984. Between July 1 and August 31, 1984 the fraction of people taking the subsidy is lower than 1%. This is understandable because people turning 25 during July 2009 will be entitled to less than one monthly YES payment. However, people turning 25 during August 2009 would be entitled to at least one payment.

For this reason we calculate how old each person would be in August 2009, one month after the program was launched in July 2009–results using July 2009 are available upon request from the authors. Since people very close to the cutoff had no big financial incentive to apply to YES, we chose a small 7 day donut, but results are robust to different donuts. We also added the same individual controls as in Table 4.

The results in Table 8 show that the individual probability of adopting YES increases in 3.5 pp in response a 10 pp increase in the fraction of coworkers with YES, over the 2010-12 period. Figure D4 tests how robust our results are to randomly assigning an age cutoff. Since the mean take-up during this period was 20%, this implies a 17% increase in take-up over the long-run. This result is similar to our finding in Section VII when the wage cutoff was used as an instrumental variable, where increasing the fraction of coworkers with YES by 10 pp increases take-up by 14% over the long run. Altogether, peers do help solve a lack of information, though once information is available they lose importance.

IX. Conclusions

This paper estimates the effect of peers on the decision to adopt 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 2.7 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 the informational channel that communicates the existence of the program as well as 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, 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 more important among older workers with more working experience, especially if they were working in a large firm. These results are interpreted as evidence that an informational channel is the most important driver of peer effects, since people with more work experience have had the opportunity to build stronger relations within the workplace.

Future studies could exploit the partial population approach used in this work to study peer effects in other means-tested social programs. For example, program eligibility in the *Chile Solidario* (CS) program is a discontinuous function of the old version of the current FPS score. Carneiro, Galasso, and Ginja (2014) find that CS participants increased take-up of a family allowance for poor children (the *Subsidio Unico Familiar*, SUF). Since CS provided information of other social programs, they could also study peer effects in take-up rates of other social programs. On the other hand, Bravo and Rau (2013) find 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 as an instrumental variable to study peer effects over employment and participation rates of peers outside the window.

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

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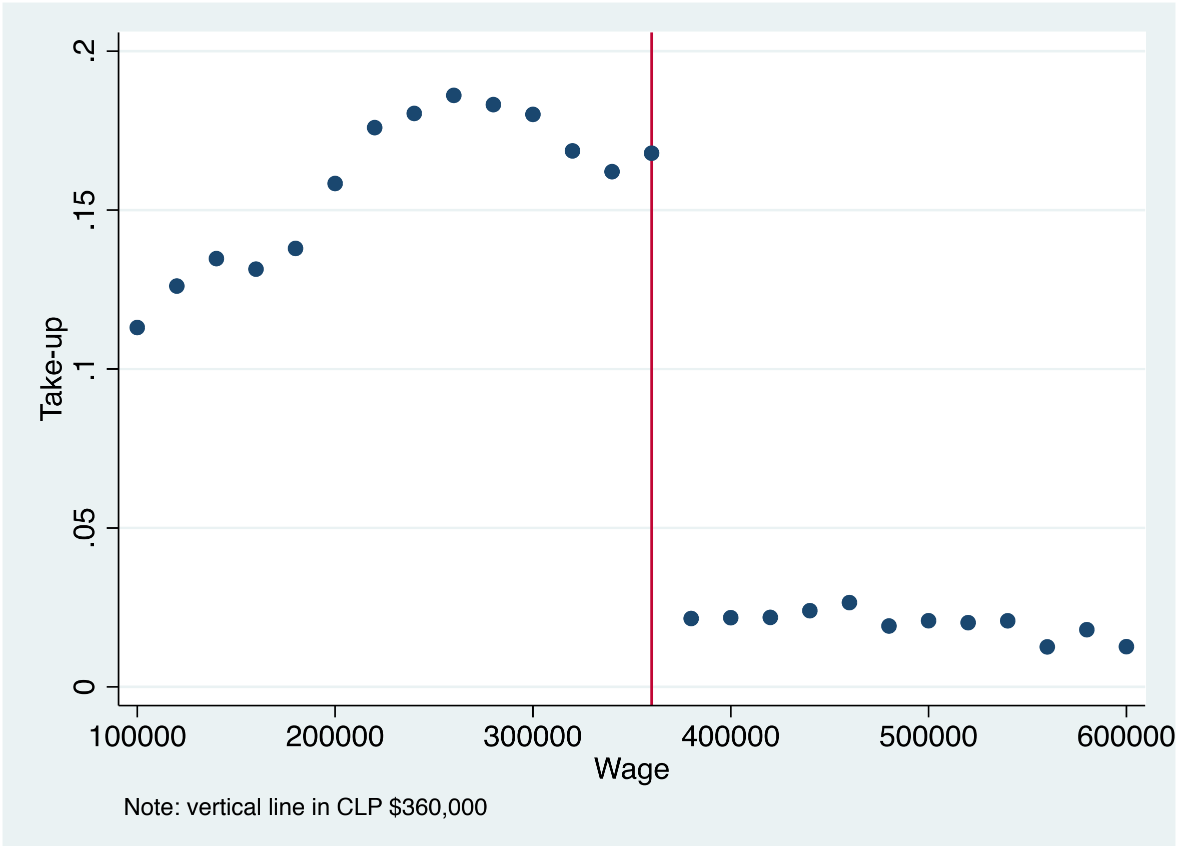
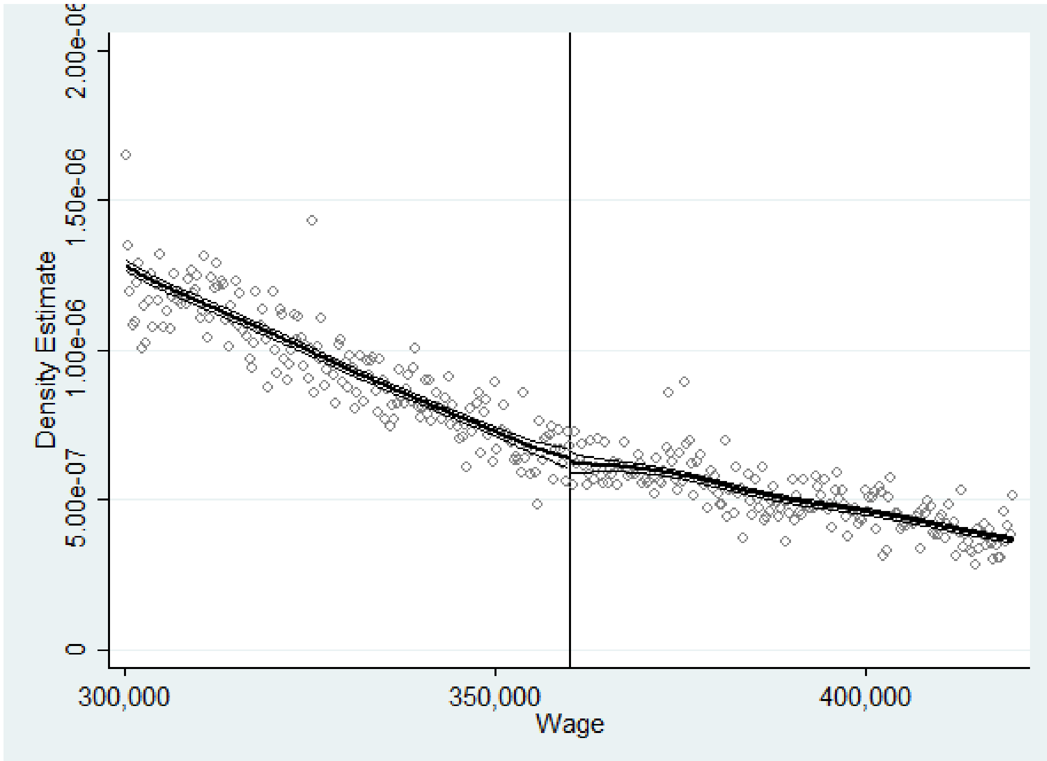
Figure 1. : Mean take-up rate by wage, March 2010.



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

Figure 2. : Density Estimates of Wages, during December 2008



Notes: This figure was constructed using the McCrary (2008) test and shows density estimates of the probability density function for monthly wages and different dates, around the selected window of width $70,000 CLP. To construct this figure, we kept only wages inside the selected window and saved 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, we created the graph by limiting the domain to wages within the window.

Table 2—: Descriptive Statistics

	Full Sample	Estimating Sample	Diff: (1)-(2)
	(1)	(2)	(3)
Age	22.08	21.55	0.53***
	[2.2]	[1.8]	
Women	1.41	1.46	-0.05***
	[0.5]	[0.5]	
Migrant	1.00	1.00	-0.002***
	[0.1]	[.0]	
Vulnerability Score	9,007.33	9,235.08	-227.80***
	[3,765.6]	[3,752.1]	
Wage (CLP $)	194,321.10	181,595.90	12,725.20***
	[116,386.9]	[67,053.2]	
Year of education	11.44	12.00	-0.56***
	[2.3]	[1.2]	
1(FPS¡=11,734)	0.69	0.67	0.02***
	[0.5]	[0.5]	
1(18¡=age¡25)	0.88	0.97	-0.09***
	[0.3]	[.2]	
1(annual earnings¡4,320,000)	0.93	1.00	-0.07***
	[0.3]	[0]	
Admissible	0.60	0.65	-0.06***
	[0.5]	[0.5]	
Company size before YES	299.49	527.49	-228.00***
	[750.3]	[931.8]	
Coworkers with YES	31.89	61.66	-29.80***
	[89.2]	[119.6]	
Observations	*542,453*		
School size	63.57	69.71	-6.14***
	[49.0]	[50.1]	
Classmates with YES	8.02	9.06	-1.04***
	[8.9]	[9.5]	
Observations	*298,214*		
Has some college	0.35	0.08	0.27***
	[0.5]	[0.3]	
Has YES	0.09	0.14	-0.06***
	[0.3]	[0.3]	
Observations	*542,454*	*86,404*	

Notes: Standard deviation in square brackets

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

	z_lnet (1)	R-squared	Observations	z_snet (2)	R-squared	Observations
Age	-0.2346**	0.04	*175,561*	-0.1454***	0.81	*139,599*
	[0.1191]			[0.0331]		
Women	0.0581**	0.16	*175,561*	0.0464**	0.21	*139,599*
	[0.0242]			[0.0206]		
Years of Education	0.15	0.06	*166,871*	-0.12	0.09	*132,582*
	[0.2934]			[0.0796]		
Peers has some college	-0.04	0.05	*175,561*	-0.02	0.07	*139,599*
	[0.0257]			[0.0202]		
Vulnerability Score	-287.79	0.06	*175,561*	49.13	0.21	*139,599*
	[190.5134]			[149.6196]		
Fraction of last	-0.13	0.07	*175,561*	-0.02	0.30	*139,599*
12 months working	[0.1744]			[0.1298]		
School size	-1.07	0.13	*95,610*	1.11	0.14	*139,599*
	[0.7470]			[2.4735]		
Company size	-35.11	0.06	*175,561*	5.62	0.03	*139,599*
before SEJ	[26.0962]			[3.6604]		
Fraction of same-school	0.00	0.04	*95,610*	0.0015**	0.02	*139,599*
coworkers	[0.0008]			[0.0007]		
Female workers	0.01	0.30	*175,561*	0.00	0.09	*139,599*
	[0.0154]			[0.0058]		

Notes: Each row and column in this table correspond to one regression where the dependent variable is shown in the left column of the Table. This dependent variable is measured as its mean among the peers inside the window. The explanatory variable in column (1) is z_lnet and in column (2) is z_snet. We also include age, age squared, sex, ln(wage), years of education, vulnerability score, and *comuna* of residence as individual controls. Each row presents only the estimated coefficient for z_lnet and z_snet for column (1) and (2), respectively. Standard errors are 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.

	First-Stage (1)	Reduced Form (2)	Second Stage (3)	N (4)
A. Coworkers				
YES Take-up	0.0592***	0.0157***	0.2654***	*175,561*
	[0.0093]	[0.0051]	[0.0874]	
F-statistic	*40.2*			
B. Classmates				
YES Take-up	0.0438***	0.002	0.0448	*139,631*
	[0.0111]	[0.0057]	[0.1281]	
F-statistic	*15.5*			

Notes: This table includes individual's 1 age, age squared, sex, vulnerability score, ln(Wage) and years of education as controls. Column (1) shows first-stage coefficients given by equation (7). Column (2) shows reduced-form coefficients from equation (8), where the dependent variable is a dummy that takes the value of 1 if the individual has YES, 0 otherwise. Column (3) shows 2sls coefficients given by equation (6) with the same dependent variable as in column (1). Standard errors are clustered at the company level for Panel A and at the school level for Panel B. * significant at 10%, ** significant at 5%, *** significant at 1%.

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

	First-Stage (1)	Reduced Form (2)	Second Stage (3)	N (4)
A. Coworkers				
Baseline	0.0592***	0.0157***	0.2654***	*175,561*
	[0.0093]	[0.0051]	[0.0874]	
No controls	0.0594***	0.0170***	0.2858***	*175,561*
	[0.0093]	[0.0052]	[0.0908]	
Group controls	0.0487***	0.0174***	0.3566***	*166,871*
	[0.0104]	[0.0051]	[0.1147]	
Comuna fixed effects	0.0592***	0.0150***	0.2541***	*175,561*
	[0.0092]	[0.0044]	[0.0790]	
Clustering s.e. at comuna level	0.0592***	0.0157***	0.2654***	*175,561*
	[0.0038]	[0.0033]	[0.0559]	
Change w definition income12/months12	0.0597***	0.0143***	0.2391***	*180,512*
	[0.0093]	[0.0048]	[0.0816]	
Change w definition income6/months6	0.0621***	0.0087*	0.1393*	*176,330*
	[0.0094]	[0.0050]	[0.0813]	
Only elegibles	0.0902***	0.0144*	0.1591*	*95,401*
	[0.0190]	[0.0085]	[0.0911]	

Notes: Regressions in this table mirror those in Table 4, see that table for details. Group controls include the mean age, sex, vulnerability score and years of education of peers inside the window as controls, $\overline{x}_{2g}$. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6—: Peers effects in networks with stronger and weaker ties

	First Stage	Reduced Form	Second Stage	N
	(1)	(2)	(3)	(4)
A. Coworkers				
Baseline	0.0592***	0.0157***	0.2654***	*175,561*
	[0.0093]	[0.0051]	[0.0874]	
Recent coworkers	0.0758***	0.0228***	0.3006***	*146,684*
	[0.0124]	[0.0061]	[0.0849]	
Coworkers for at least two consecutive periods	0.0943***	0.0329***	0.3486***	*116,433*
	[0.0130]	[0.0073]	[0.0856]	
Old coworkers	0.0406***	0.0067	0.1650	*90,147*
	[0.0081]	[0.0046]	[0.1148]	
B. Classmates				
Baseline	0.0438***	0.002	0.0448	*139,631*
	[0.0111]	[0.0057]	[0.1281]	
Cuarto medio (schoolmates)	0.0817***	0.0099	0.1207	*160,402*
	[0.0186]	[0.0105]	[0.1247]	
Primero medio (classmates)	0.0548***	0.0103*	0.1888*	*101,751*
	[0.0153]	[0.0055]	[0.1044]	
Primero medio (schoolmates)	0.0877***	0.0133	0.1515	*125,091*
	[0.0180]	[0.0085]	[0.0966]	

Notes: Specifications mirror the baseline specification described in Table 4. Old coworkers are defined according to the coworkers during December 2007. Recent coworkers are people that shared the same company during March 2009. Schoolmates are people that attended the same school, regardless of the year. Recent classmates are people who graduated from *cuato medio* during the year of 2008. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7—: Effect of 2009 early adoptions over adoption during 2010.

	First-Stage	Reduced Form	Second Stage
	(1)	(2)	(3)
A. Coworkers			
2009 (baseline)	0.0592***	0.0157***	0.2654***
	[0.0093]	[0.0051]	[0.0874]
2010	0.0592***	0.0118**	0.1992**
	[0.0093]	[0.0059]	[0.0992]
B. Classmates			
2009 (baseline)	0.0438***	0.002	0.0448
	[0.0111]	[0.0057]	[0.1281]
2010	0.0438***	0.0034	0.0780
	[0.0111]	[0.0066]	[0.1485]

Notes: Specifications mirror those in Table 4. Adopters during 2009 are those who received any YES payment during any month of 2009, and they are used to construct the numerator of the endogenous variable $\overline{y}_{2g}$, number of peer with YES during 2009. **Adopting peers during 2010 are those who received any YES payment during any month of 2010 only and not during any other previous year.** * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8—: Effects of 2009 early adoptions over 2010-12 adoption.

	First-Stage (1)	Reduced Form (2)	Second Stage (3)
A. Wage cutoff as IV			
2010	0.0592***	0.0118**	0.1992**
	[0.0093]	[0.0059]	[0.0992]
2011	0.0563***	0.0029*	0.0510*
	[0.0089]	[0.0016]	[0.0296]
2012	0.0563***	0.0015	0.0275
	[0.0089]	[0.0012]	[0.0213]
2010-2012	0.0592***	0.0149**	0.2515**
	[0.0093]	[0.0063]	[0.1073]
B. Age cutoff as IV			
2010	0.043***	0.010***	0.2344***
	[0.003]	[0.004]	[0.0858]
2011	0.042***	0.004*	0.1034*
	[0.003]	[0.002]	[0.0550]
2012	0.042***	0.002*	0.0583*
	[0.003]	[0.001]	[0.0311]
2010-2012	0.043***	0.015***	0.3474***
	[0.003]	[0.005]	[0.1076]

Notes: All estimations mirror those on Table 4. Adopting peers during 2010 (or 2011, or 2012) are those who received any YES payment during any month of 2010 only (or 2011, or 2012) and not during any other previous year. Adopters during 2010-2012 are those who received any YES payment during any month between 2010 and 2012 only and not during 2009. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9—: Mechanisms in the workplace network

	First-Stage (1)	Reduced Form (2)	Second Stage (3)	N (4)
A. Coworkers: combined regressions				
1. Over 5 years of experience	0.0615***	0.0159***	0.2583***	*190,365*
	[0.0092]	[0.0050]	[0.0823]	
Under 5 years of experience	0.0752***	-0.0047	-0.0625	
	[0.0222]	[0.0236]	[0.3174]	
2. Older than 20	0.0607***	0.0177***	0.2925***	*190,365*
	[0.0092]	[0.0049]	[0.0839]	
Younger than 20	0.0649***	0.0097	0.1493	
	[0.0112]	[0.0081]	[0.1231]	
3. Big Companies	0.0765***	0.0372***	0.4864***	*190,365*
	[0.0167]	[0.0087]	[0.1381]	
Small Companies	0.0474***	-0.0044	-0.0931	
	[0.0076]	[0.0044]	[0.0961]	

Notes: Specifications mirror the baseline specification described in Table 4. Small companies are companies with less than 50 workers, as defined by Chilean legislation. * significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix A: Other Figures and Tables

Table A1—: 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 $70,000 CLP, using the estimating sample of 86,404 individuals. Row A shows the number of coworkers with a wages inside the 70,000 window and row B shows the number of classmates with a wages inside the 70,000 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.

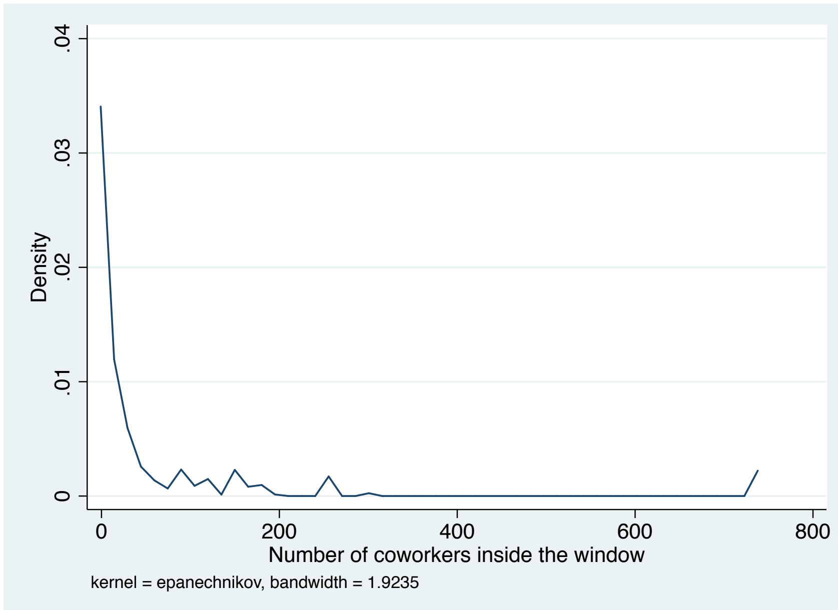
Table A2—: Descriptive Statistics

	With YES (1)	Without YES (2)	Diff: (1)-(2) (3)
Age	21.20	21.61	-0.41***
	[1.6]	[1.9]	
Women	1.52	1.45	0.070***
	[0.5]	[0.5]	
Migrant	1.00	1.00	0.00004
	[.0]	[.0]	
Vulnerability Score	7,718.69	9,486.49	-1,767.8***
	[3,275.2]	[3,766.9]	
Wage (CLP $)	183,219.90	181,326.70	1,893.3**
	[62,711.7]	[67,742.7]	
Year of education	12.02	12.00	0.02
	[1.2]	[1.2]	
1(FPS¡=11,734)	0.90	0.63	0.27***
	[0.3]	[0.5]	
1(18¡=age¡25)	1.00	0.97	0.030***
	[.0]	[.2]	
1(annual earnings¡4,320,000)	1.00	1.00	0
	[0]	[0]	
Admissible	0.90	0.61	0.29***
	[0.3]	[0.5]	
Company size before YES	582.55	518.37	64.2***
	[952.2]	[928.1]	
Coworkers with YES	76.96	59.12	17.8***
	[133.3]	[117]	
School size	73.24	69.13	4.12***
	[53.0]	[49.5]	
Classmates with YES	11.06	8.73	2.34***
	[10.9]	[9.2]	
Has some college	0.08	0.08	0.003
	[0.3]	[0.3]	
Observations	*12,288*	*74,116*	

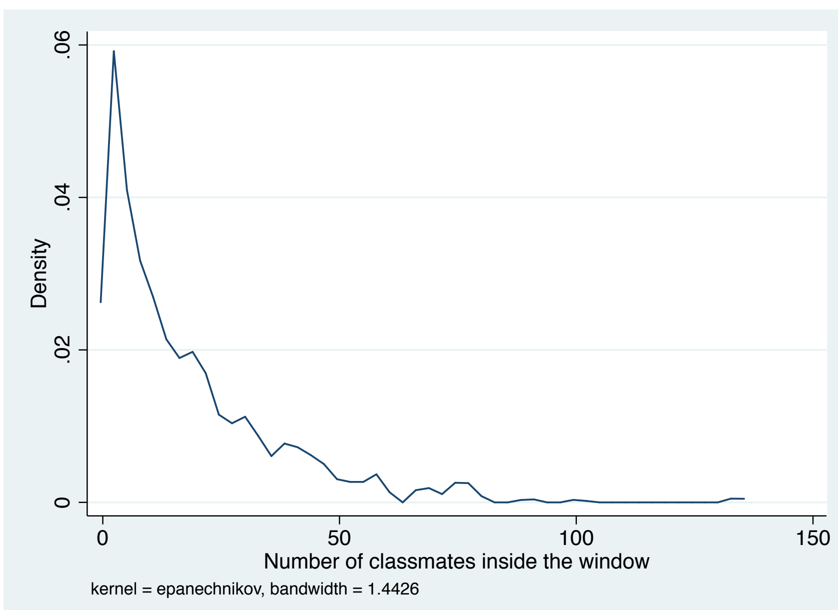
Notes: Standard deviation in square brackets

Figure A1. : Kernel Density Estimates and Descriptive Statistics

(a) Number of coworkers with a wage inside the $70,000 window

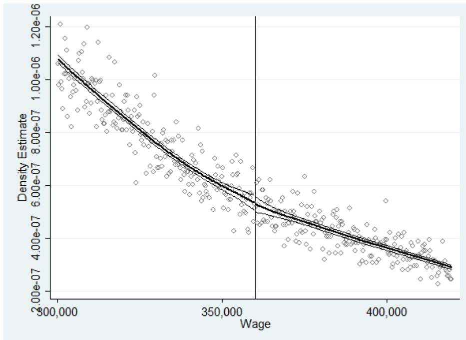


(b) Number of classmates with a wage inside the $70,000 window

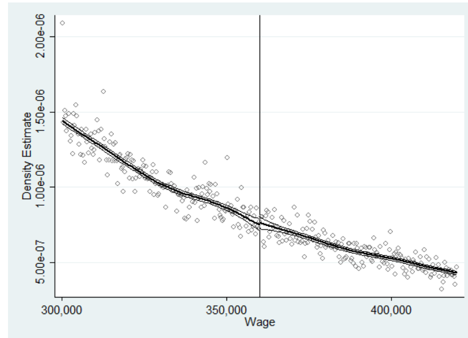


Notes: These figures show Kernel Density Estimates for the corresponding number of coworkers and classmates inside the window of $70,000 CLP, using the estimating sample of 86,404 individuals. Table B1 shows some descriptive statistics corresponding to these graphs.

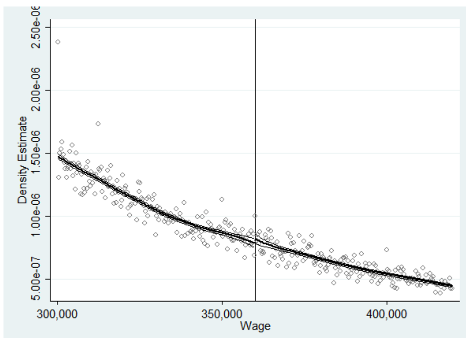
Figure A2. : Density estimates of wages around cutoff x_0 for different dates.



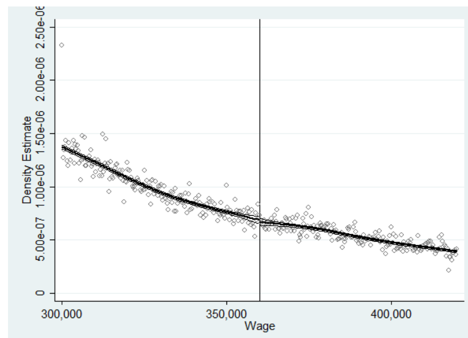
(a) March 2009



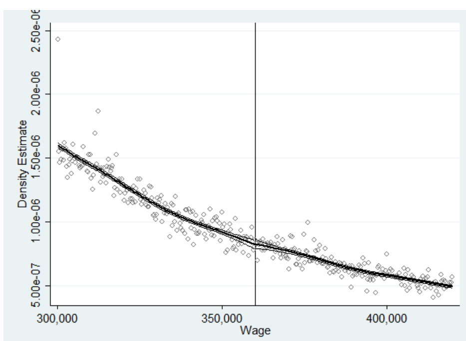
(b) September 2009



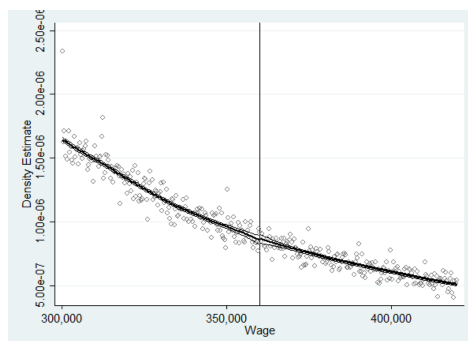
(c) December 2009



(d) March 2010



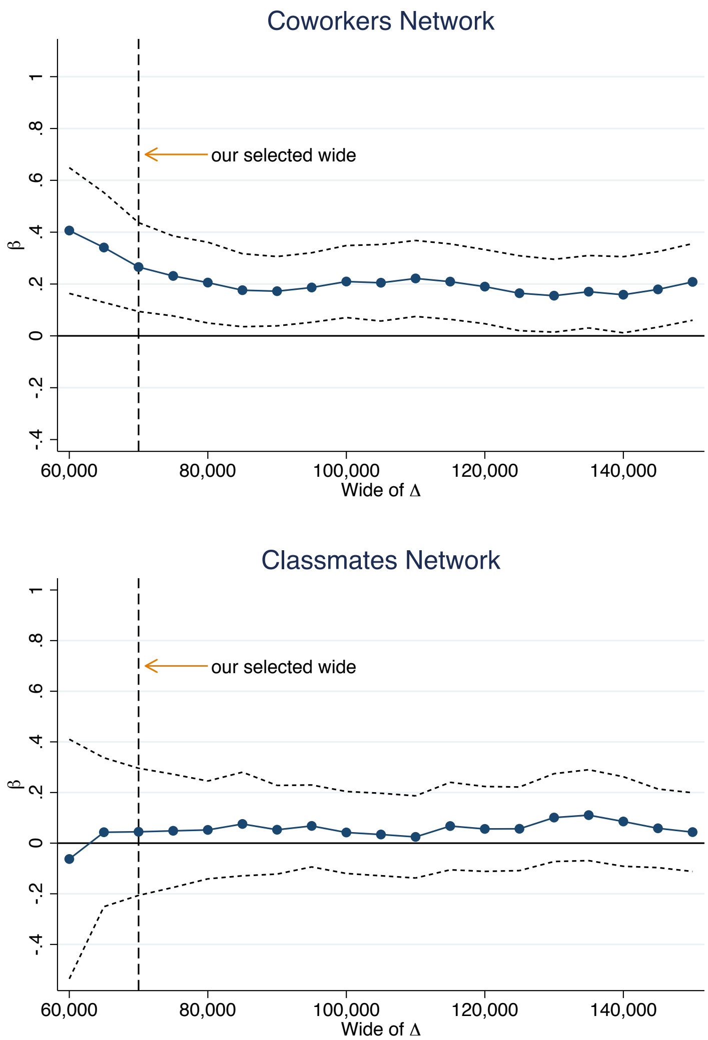
(e) September 2010



(f) December 2010

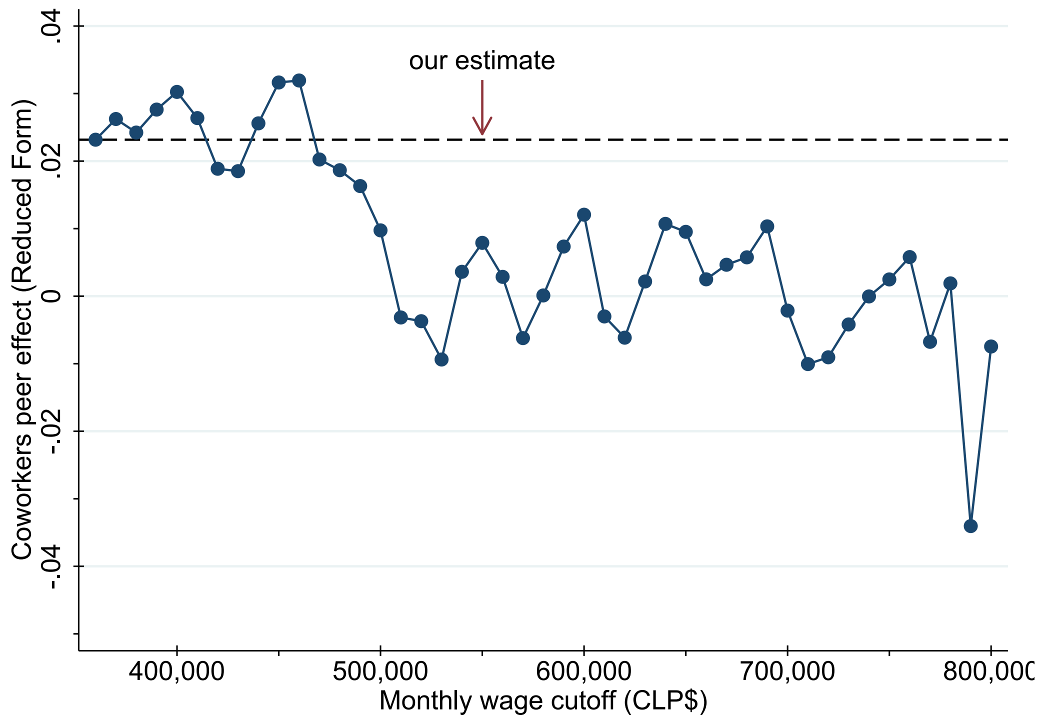
Notes: These figures were constructed using the McCrary (2008) test and they show density estimates of the probability density function for monthly wages and for different dates, around the selected window of width $70,000 CLP. To construct these figures, first we kept only wages inside the selected 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 A3. : **Sensitivity of coworkers peer effects to different window wides, constructing the instrumental variable using the wage cutoff.**



Notes: Each dot on the two solid lines represent the estimated peer effect β from the single 2SLS equation (6) for the given window width shown in the horizontal axis. The dots in the upper panel represent the 2SLS coefficient at the coworkers network; while the dots in the bottom panel represent the 2SLS coefficient at the classmates network. All regressions follow the specifications in Table 4. The dotted lines on each graph are the 95% confidence interval, with clustering standard errors at the company or school level. The vertical line shows our selected wide of $70,000 Chilean pesos.

Figure A4. : **Placebo estimates of the peer effects using the wage cutoff to construct the instrumental variable.**



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.

Appendix B: More than one peer

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.[18] 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

$$y_{1g} = \alpha_1 + \beta_1 \overline{y}_{2g} + \gamma_1 x_{1g} + \tau_1 \overline{x}_{2g} + \theta_1 w_g + \eta_{1g} \tag{B1}$$

$$\overline{y}_{2g} = \alpha_2 + \beta_2 y_{1g} + \gamma_2 \overline{x}_{2g} + \tau_2 x_{1g} + \theta_2 w_g + \lambda \overline{z}_{2g} + \overline{\eta}_{2g} \tag{B2}$$

Equations (6) and (7) can be obtained from the equations above by not taking into account x_{ig} and $\overline{x}_{ig}$.

18. 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.

Appendix C: Our peer effect is not mathematically equal to 1

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:

$$\hat{\beta} = \frac{\sum_j \sum_i S_j y_{ij}}{\sum_j \sum_i S_j \bar{y}_{-i,j}} \tag{C1}$$

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 C1 can be rewritten as $N\bar{y}_j - y_{ij}$ and then

$$\hat{\beta} = \frac{\sum_j \sum_i S_j y_{ij}}{\sum_j \sum_i S_j[\frac{1}{N-1}(N\bar{y}_j - y_{ij})]} \tag{C2}$$

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

$$\hat{\beta} = \frac{\sum_j S_j \bar{y}_j}{\sum_j S_j[\frac{1}{N-1}(N\bar{y}_j - \bar{y}_j)]} \tag{C3}$$

"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 $x_0 = 11,734$ cutoff. 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:

$$z_{-i,j} = \frac{\sum_k left_{kj} - left_{ij}}{\sum_k inside_{kj} - inside_{ij}} \tag{C4}$$

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 C2, so the sums cannot be carried out through, and this implies that the coefficient $\hat{\beta} \neq 1$. As a result,

$$\hat{\beta} = \frac{\sum_j \sum_i z_{-i,j} y_{ij}}{\sum_j \sum_i S_j \bar{y}_{-i,j}} \neq 1 \tag{C5}$$

Appendix D: 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) [19]. 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:

$$y_{1ls} = \alpha + \beta_1 \text{l\_net}_{2l} + \beta_2 \text{s\_net}_{2s} + \nu_{1ls} \tag{D1}$$

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:

$$\text{l\_net}_{2l} = \gamma_1 + \lambda_{11} \text{z\_lnet}_{2l} + \lambda_{12} \text{z\_snet}_{2s} + \xi_{2ls} \tag{D2}$$

$$\text{s\_net}_{2s} = \gamma_2 + \lambda_{21} \text{z\_lnet}_{2l} + \lambda_{22} \text{z\_snet}_{2s} + \xi_{2ls} \tag{D3}$$

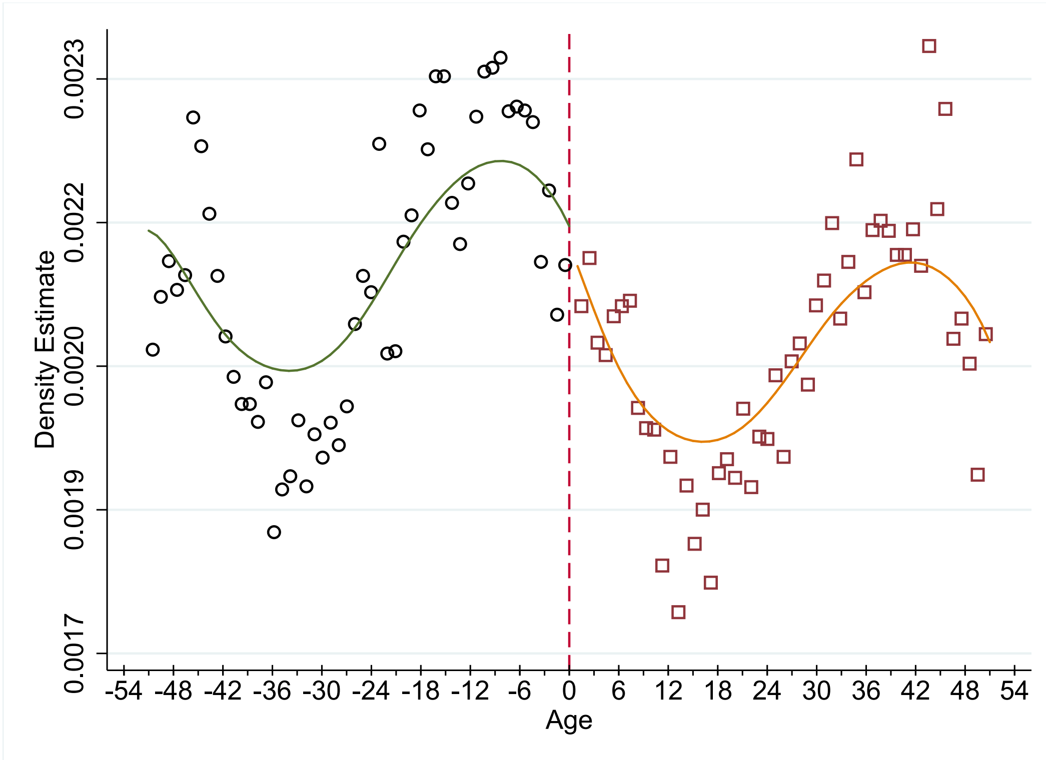
where $\text{z\_lnet}_2 = \frac{\sum_{j \in C_1^*} z_j}{c_1^*}$ and $\text{z\_snet}_2 = \frac{\sum_{j \in S_1^*} z_j}{s_1^*}$ are the fraction of coworkers and classmates whose wage is at the left of the cutoff and inside the small window, respectively. And C_1^* and S_1^* are the groups of 1's coworkers and classmates whose wage are inside the small window of size Δ around the x_0 cut-off, respectively.[20] Equations (D1), (D2), and (D3) can be augmented to include covariates X_i at both the individual and group level.

Equation (D1) is estimated by 2SLS, with first-stages given by (D2) and (D3). 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, Gelbach and Miller (2006). See Appendix Figure A1 for Kernel density estimates and Table A1 for other descriptive statistics.

19. 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.

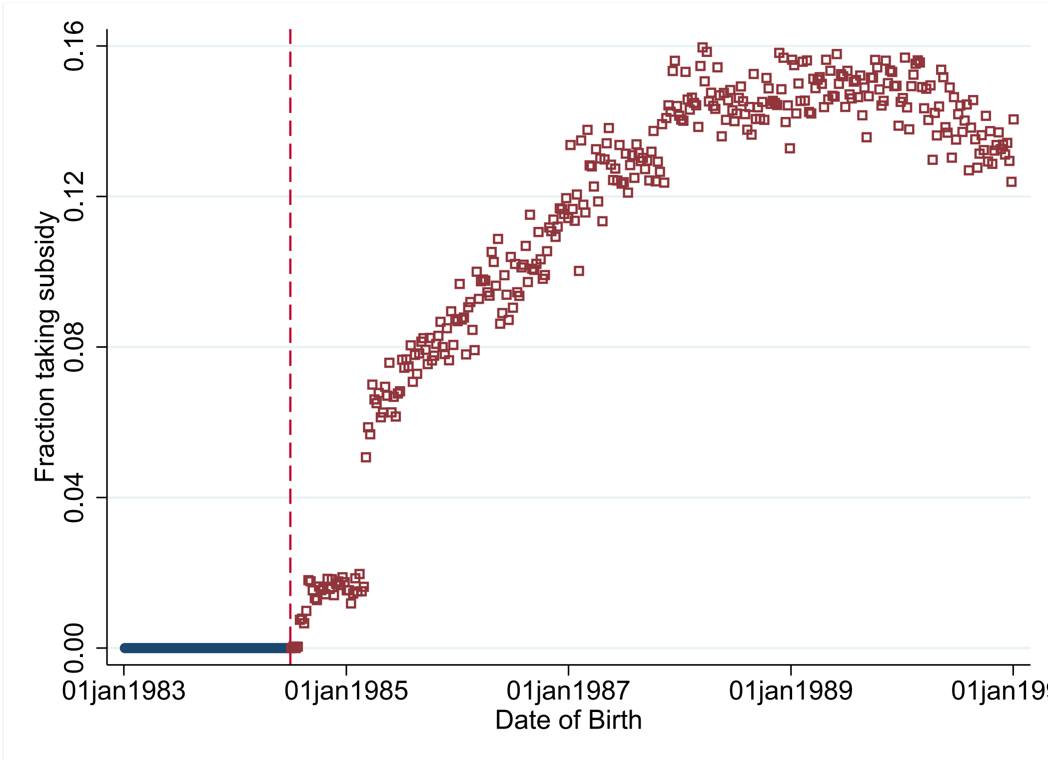
20. Then $C_i^* \equiv \{j \in C_i : FPS_j \in [x_0 - \Delta, x_0 + \Delta]\}$ and similarly $S_i^* \equiv \{j \in S_i : FPS_j \in [x_0 - \Delta, x_0 + \Delta]\}$.

Figure D1. : **Density Estimate around the 25 age cutoff by week, during December 2008.**



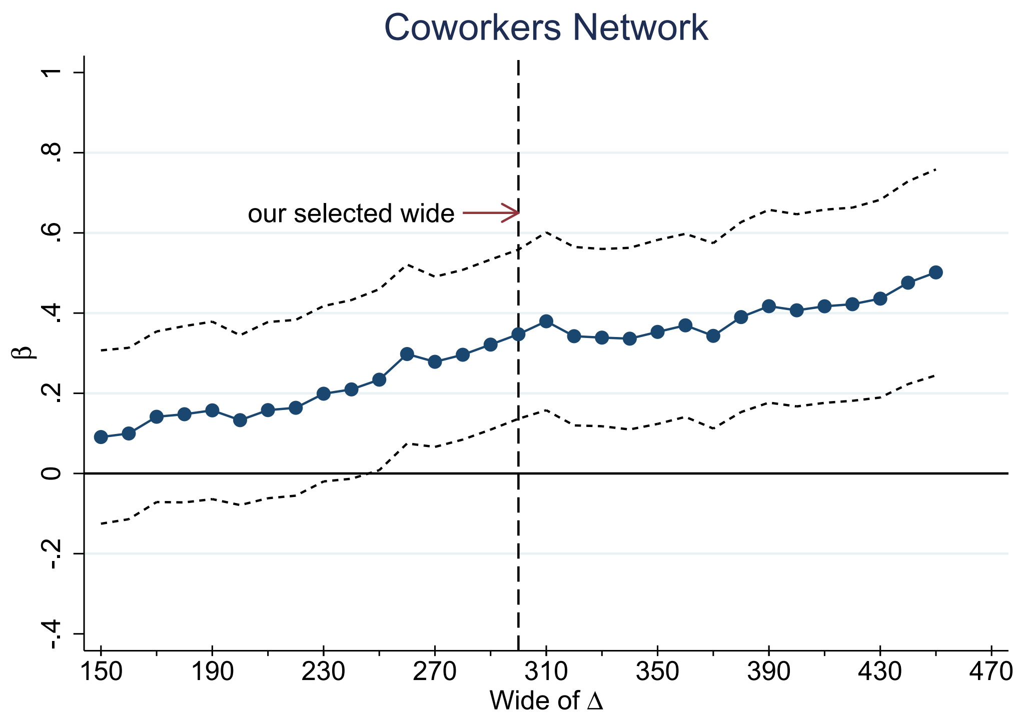
Notes: **This figure shows density estimates of the probability density function for age measured in weeks, 52 weeks before and after the cutoff age of 25. Each dot shows the frequency of people whose birthday is within one-week bins. The dashed vertical line denote the age cutoff of 25, which has been normalized to zero.**

Figure D2. : **Fraction taking YES by date of birth over 2010-12.**



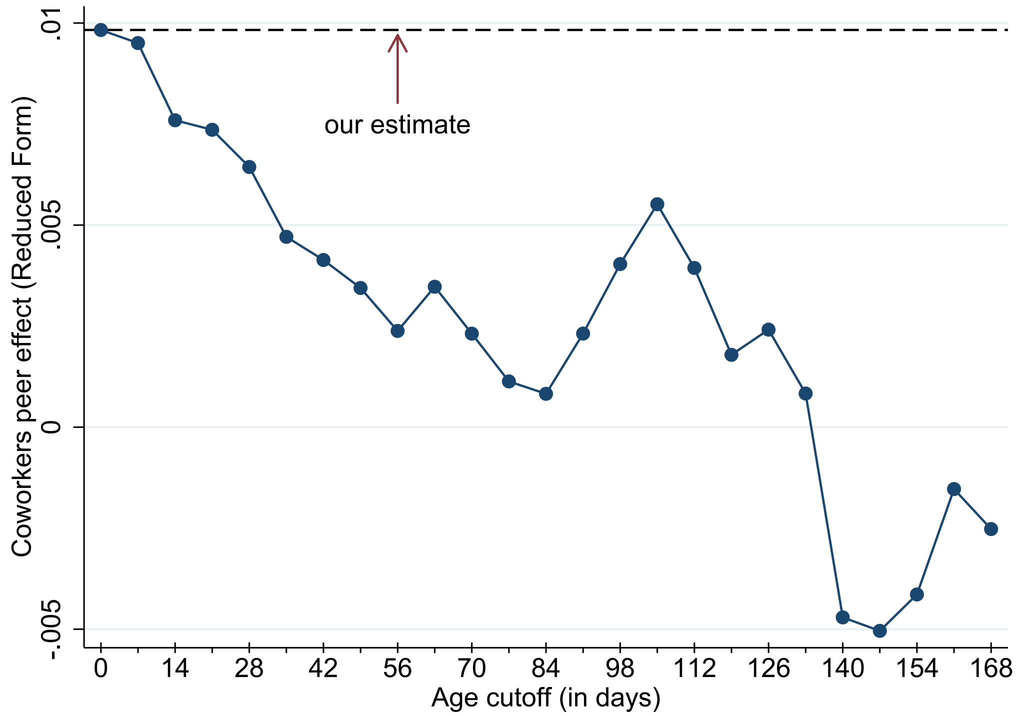
Notes: **Each dot in this graph shows the fraction taking the subsidy in one one-week bins. The vertical line shows July 1st, 1984. In this graph, the fraction of people taking the subsidy is higher than zero but below 1% between July 1st, 1984 and September 1st, 1984. YES was implemented on July 1st, 2009, but successful applicants could claim retroactive payments if they applied before October 2009. Take-up jumps again on March 1, 1985**

Figure D3. : **Sensitivity of coworkers peer effects to different window wides, constructing the instrumental variable using the age cutoff.**



Notes: **See Figure A3 for a description. Standard errors clustered at the company level. The vertical line shows our selected wide of 300 days.**

Figure D4. : **Placebo estimates of the peer effects using the age cutoff to construct the instrumental variable.**



Notes: **Each placebo estimate first assigns a window around the false age cutoff, and then estimates a reduced form peer effect at the coworkers level. There are 25 estimates in this graph (from 0 to 168), where each estimate increases the false cutoff by 7 days. The horizontal axis measures the cutoff age and the vertical axis measures the coefficients. The horizontal line shows our estimate of 0.015. Standard errors clustered at the company level.**