PEER EFFECTS IN THE ADOPTION OF A YOUTH EMPLOYMENT SUBSIDY

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Abstract—This paper studies the effects of peers on the adoption of a Youth Employment Subsidy in Chile since its inception. We examine the effects that former classmates' and coworkers' adoption have on one's adoption. Identification comes from discontinuities in the assignment rule that allow us to construct valid instrumental variables for peers' adoption. Using a comprehensive set of administrative records, we find that classmates and especially coworkers play significant roles in the adoption of the subsidy. Peer effects are determined during the early stages of the program's implementation and vary by network characteristics and the strength of network ties.

I. Introduction

THE role of social interactions in individuals' decisions has been increasingly studied over the past decade. By interacting with others, we gain information and absorb different opinions, which can shape our own decisions, particularly in deciding on the adoption of social programs. However, empiricists have faced difficulties in studying social interactions, mainly because of econometric hurdles, such as reflection, correlated unobservables, and endogenous group membership (Manski, 1993). Despite these problems, a new wave of empirical research on peer effects has used instrumental variables and a method known as the "partial population" approach to estimate how another's behavior affects one's own decisions (Moffitt, 2001).[1]

In this paper, we study the role of peers on the take-up of the Youth Employment Subsidy (YES) program in Chile. To consistently estimate the peer effects of the program, we follow a partial population approach that exploits discontinuities in the program's eligibility criteria and allows us to construct credible instrumental variables for peer adoption. In March 2009, when the Chilean government announced this program, some people fell on the eligible side of the criteria (younger than 25 years of age and less than CLP $360,000 in monthly wages), while others did not. Hence, there is a small window around each threshold where eligibility is as if randomly assigned. Using this quasirandom variation in program eligibility for a subset of peers, we are able to identify peer effects in program participation among eligible workers whose networks contain individuals in such a window.

We have access to rich administrative data that allow us to examine two social networks: coworkers and former high school classmates. People who are eligible for the YES program might learn about the program from coworkers. Also, because the YES program targets young workers, friendship networks developed during high school might impact information acquisition about the program and take-up. Analyzing the effects of different networks is interesting since we do not know *a priori* which is the relevant peer group. To the best of our knowledge, this is the first paper to study the effect of classmates on the adoption of a social program.

We find that individuals learn about YES through classmates and especially coworkers, with peer effects that are greater in strong-tie networks, and the impact of peers on subsidy adoption is mostly determined in the first two years of program implementation.

II. Empirical Strategy

A. *A Partial Population Solution to the Reflection Problem*

This paper attempts to consistently estimate the effects of peers in the YES program's take-up in two different networks: coworkers and classmates. Ideally, one would like to randomly assign individuals to groups to estimate the effect of having peers with the program on one's participation. Instead, as in Dahl, Løken, and Mogstad (2014), we exploit quasirandom variation in participation for some individuals in a group and assess how other group members change their behavior.

Formally, let $g \in \{1, \ldots, G\}$ be an index for a group of peers and assume, for now, that groups have only two individuals, labeled by index 1 and 2. Assume that there is random eligibility for individual 1, denoted by a dummy variable z_{1g} that is equal to one if individual 1 is eligible and 0 if not, while there is no such variation for individual 2, who is eligible for the program. Let y_{ig} be the take-up decision of individual i in group g, x_{ig} individual's i covariates, and ε_{ig} a residual term. Thus we can write the system of simultaneous equations for peer effects as follows:

$$y_{1g} = \alpha_1 + \eta_1 x_{1g} + \beta_1 y_{2g} + \gamma_1 x_{2g} + \delta z_{1g} + \varepsilon_{1g}, \tag{1}$$

$$y_{2g} = \alpha_2 + \eta_2 x_{2g} + \beta y_{1g} + \gamma_2 x_{1g} + \varepsilon_{2g}, \tag{2}$$

where equations (1) and (2) show that the decision of one peer is influenced by the other peer's decision. Since z_{1g} is random, it is orthogonal to observed and unobserved covariates, and correlated unobservables can no longer bias the estimates.

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[1]See, for example, Joensen and Nielsen (2018); Rege, Telle, and Votruba (2012); Bobonis and Finan (2009).

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The exclusion of z_{1g} from equation (2) solves the reflection problem, and β is identified, as discussed by Moffitt (2001) and Dahl et al. (2014). In addition, if peer groups are measured before the introduction of the program, endogenous group membership does not bias the peer effect estimator.

Different versions of this *partial population approach* have recently been implemented with groups of two individuals. For example, Fletcher and Marksteiner (2017) and Joensen and Nielsen (2018) work with groups formed by spouses and sibling pairs, respectively.[2] However, when groups have more than two peers, the take-up decision of one individual can be affected by the decisions of multiple peers. A common assumption in these "many-to-one" interactions is that a coworker's decision is affected by the average decisions (and characteristics) of their peers, as in a *linear-in-means* model. In our case, we work with groups with two or more individuals that can be labeled as type 1 (with random eligibility for the program) and type 2 (without random eligibility). Groups have at least one type-1 individual and one type-2 individual, and we focus on the effect of the average take-up decisions of type-1 individuals ($\bar{y}_{1g}$) on the take-up decisions of type-2 individuals (y_{2g}). This allows us to exploit the quasirandom eligibility for the program of type-1 individuals as an instrumental variable for their take-up. Thus, in equations (1) and (2), we replace all of the variables with label 1 by their sample average and obtain the following system of equations:

$$\bar{y}_{1g} = \alpha_1 + \eta_1 \bar{x}_{1g} + \gamma_1 x_{2g} + \delta \bar{z}_{1g} + \bar{\varepsilon}_{1g}, \quad (3)$$

$$y_{2g} = \alpha_2 + \beta \bar{y}_{1g} + \eta_2 x_{2g} + \gamma_2 \bar{x}_{1g} + \varepsilon_{2g}, \quad (4)$$

where equation (3) is a first stage, equation (4) is the structural equation, and β can be consistently estimated by two-stage least squares (2SLS). Note that, as in Rege, Telle, and Votruba (2012) and Dahl et al. (2014), we use groups in which type-2 individuals make their choices after type-1 individuals, so there is no feedback from type-2 to type-1 individuals. This group structure helps to alleviate the incidence of the reflection problem.[3] Substituting equation (3) into equation (4) gives the reduced form:

$$\begin{aligned} y_{2g} = {} & (\alpha_2 + \beta\alpha_1) + (\eta_2 + \beta\gamma_1) x_{2g} \\ & + (\gamma_2 + \beta\eta_1)\bar{x}_{1g} + \delta\beta \bar{z}_{1g} + \upsilon_{2g}, \end{aligned} \quad (5)$$

which implies that δ can be consistently estimated by regressing $\bar{y}_{1g}$ on $\bar{z}_{1g}$, and covariates (equation [3]) and β can be estimated by regressing y_{2g} on $\bar{z}_{1g}$ and covariates (equation [5]) and scaling by $\hat{\delta}$.

In section V, we estimate different variants of this model, including one that considers the effect of all peers (not only type-1 individuals) on type-2 individuals. See the text for details.

B. *Quasirandom Eligibility*

Eligibility for the YES program changes sharply due to discontinuities in the assignment rule–eligible workers must have a monthly wage below CLP \$360,000 as of 2009 (CLP \$360K hereafter) and be in the 18–25 age range. Figure 1 shows the mean take-up rates by wage and age in 2009 and reveals that take-up jumps sharply at each threshold: \$360K and 25 years of age, respectively.

We argue that discontinuities in the YES assignment rule provide random variation within a narrow window around the eligibility thresholds. While individuals making slightly less than CLP \$360K per month are eligible for the program, individuals making slightly more are not. The same applies to age: individuals slightly younger than 25 years old are eligible, and those slightly older are not.

Note that the program was announced in March 2009, and implementation began in July of the same year, meaning that agents' income and work decisions made before 2009 cannot be a function of this program. That is, before 2009, agents could not finely manipulate their earnings as a function of the YES program because the program did not exist. As such, if we consider a period before 2009, there exists an income interval around threshold t where an individual's placement within that interval is quasirandomly assigned. Based on this fact, we build a window of size 2Δ around the threshold eligibility wage, and we then calculate the percentage of agent i's peers who are eligible inside the window. If more peers are on the eligible side, then more peers will potentially benefit from the program, and agent i will be more likely to receive information about the program.

Since we have two discontinuities inducing random variation in eligibility, we construct two instruments for peers' adoption: a "wage" instrument and an "age" instrument. To build the wage instrument, let us define $\mathcal{N}_{-i,g}$ as the set of $i's$ peers in a given group g that have a gross monthly income within the 2Δ window around the threshold (type-1 individuals). Then, the share of agent i's peers on the eligible side of the threshold can be written as $\bar{z}_{1g} = \frac{1}{n_g} \sum_{j \in \mathcal{N}_{-i,g}} \mathbf{1}\left[\text{wage}_j < 360\right]$, where n_g is the cardinality of set $\mathcal{N}_{-i,g}$, and $\mathbf{1}[\cdot]$ is an indicator function. Thus, $\bar{z}_{1g}$ is simply the share of peers inside the 2Δ window who make CLP \$360K or less.[4] We proceed in the same way to construct the age instrument. In this case, $\bar{z}_{1g} = \frac{1}{n_g} \sum_{j \in \mathcal{N}_{-i,g}} \mathbf{1}\left[age_j < 25\right]$.

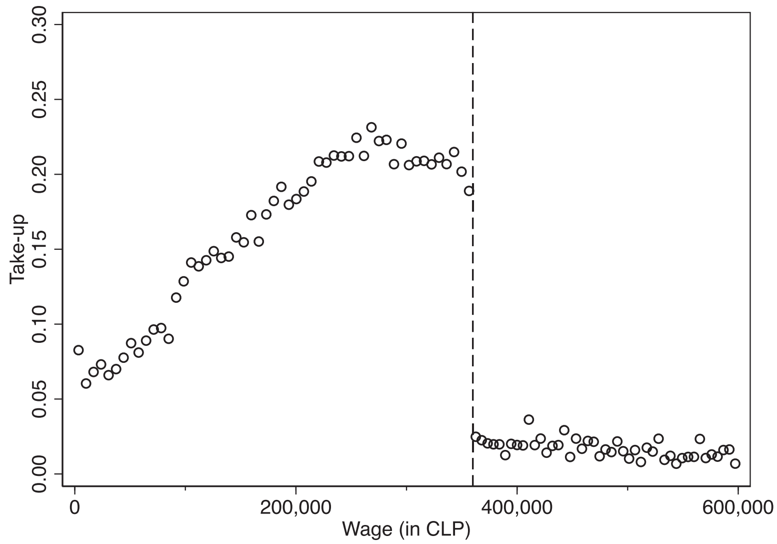
Figure 2 depicts the construction of the instrumental variable using wages (in CLP thousands). We plot a real line for wages with three cutoff points. Individuals to the left of

[2]Fletcher and Marksteiner (2017) estimate the spillover effects of alcohol and smoking suspension between spouses and find a 30-percentage point decrease effect of one spouse's cessation on the other. Joensen and Nielsen (2018) evaluate the effect of an older sibling on the educational choice of the younger one and find significant effects.

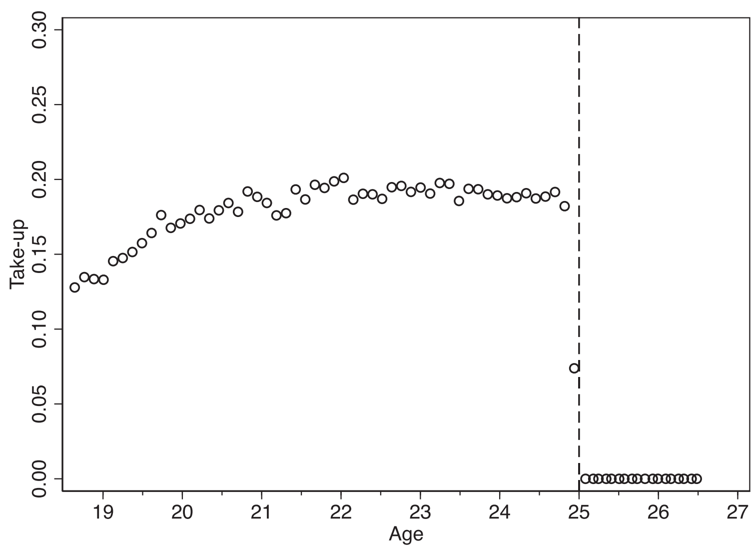
[3]A full derivation of the estimating equations in the general case and a discussion of this particular case are provided in the online appendix A.

[4]If agent i has no peers within the window, the instrument has a missing value, and we follow a casewise deletion procedure. In that case, agent i is not in the working sample.

FIGURE 1.—YES TAKE-UP BY WAGE AND AGE IN 2009



Panel A. Take-up by wage



Panel B. Take-up by age

Panel A shows the take-up rate of the program by wage in CLP. Each dot shows the share of individuals taking up the subsidy in a given wage bin. The sample is restricted to youths between 18 and 25 years old with an FPS score less than 11,734. Panel B shows the take-up rate by age. Each dot shows the share of individuals taking the subsidy in a given age bin. The sample is restricted to workers earning less than CLP $360,000 years and an FPS score less than 11,734. The dashed vertical line in each panel denotes the eligibility cutoff according to the corresponding variable (wage and age).

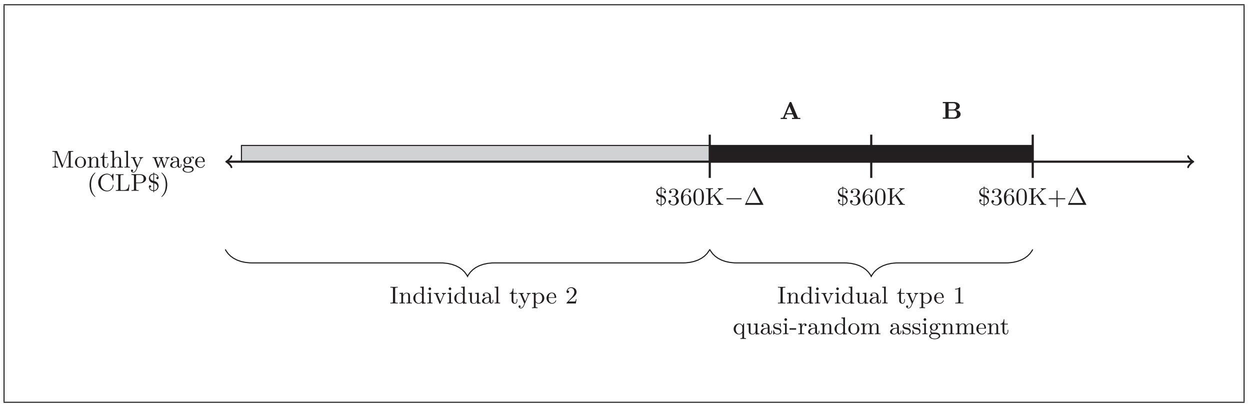
\$360K $-\ \Delta$ are eligible (type-2 individuals). The quasirandom eligibility ($z_{1g} \in \{0, 1\}$) occurs in a narrow window defined by the closed interval [360K $-\ \Delta$, 360K $+\ \Delta$], where 2Δ is the window size. Type-1 individuals (inside the window) are eligible only if their wages are less than \$360K (A), and those whose wages are greater than \$360K are not (B). The instrumental variable is the proportion of eligible peers inside the window (A/(A+B)). Individuals to the right of \$360K+Δ are not used in the analysis. For the age instrument, we follow the same procedure described before but in a narrow window around the age cutoff.

III. Institutional Background

The YES program was launched in July 2009. It is a twofold monetary incentive targeted at workers in the 18–25 age range belonging to the poorest 40% of the population, according to a proxy means index called the *Ficha de Protección Social* (FPS, hereafter).[5] It complements the wages of low-income individuals under the age of 25 by up to 20%

[5]The FPS is a proxy means index used for targeting social programs that ranges from 2,072 to 16,316 points, with a higher number implying a lower degree of vulnerability. It is updated every month, and an FPS score less

FIGURE 2.—QUASIRANDOM ELIGIBILITY USING THE WAGE CRITERION



Individuals to the left of \$360K − Δ are eligible individuals (type 2). The quasirandom eligibility occurs in a narrow window defined by the interval [360K − Δ, 360K + Δ], where 2Δ is the window size. Type 1 peers inside the window are eligible only if their wages are less than \$360K (A), and those whose wages are greater than \$360K are not (B). The instrumental variable is the fraction A/(A+B).

and awards the employer a bonus of up to 10% of an employee's wage. This program is a significant cash incentive for vulnerable youths to enter the labor market and for firms to employ them. However, the take-up of the YES program is low.[6]

Less than 21% of those who are eligible apply, and millions of dollars go unclaimed, raising the question of why so much cash is left on the table. There is a host of reasons why nearly 80% of those eligible might not be applying to receive YES. Perhaps they (mistakenly) believe that they are ineligible; they might not know enough about the program's benefits or believe that the cost of applying is prohibitive. Another potential factor preventing people from applying is social stigma associated with this program since it explicitly targets low-income families. However, information about the program (or the lack thereof) is likely an important factor in determining whether Chileans ultimately benefit from this subsidy.[7]

A. *Eligibility Criteria*

A worker is admissible if he/she: (i) has a gross annual income less than CLP\$4,320,000 (US\$8,640) or a gross monthly wage less than CLP\$360K (US\$720);[8] (ii) is between 18 and 25 years old; (iii) has an FPS score equal to or less than 11,734 points, corresponding to the poorest 40%; (iv) is not working in a state institution or a firm with a state contribution greater than 50%; and (v) has social security contributions paid up to date.[9]

B. *Application*

The YES application process differs from those of many social programs that use complex procedures to avoid rejecting deserving candidates or awarding the subsidy to ineligible ones (Kleven & Kopczuk, 2011). Instead, it is simple and can be completed at any time, from any place, over the internet.[10] This online application process considerably reduces the necessary paperwork and simplifies several procedures,[11] indicating that complexity or high application costs are not a plausible explanation for the program's low take-up rate

than or equal to 11,734 indicates that a household is in the lowest 40th percentile of the distribution.

[6]Despite the launch of a promotional campaign for the program in 2009, the take-up rate was only 3.6% for employers and approximately 21% for employees according to Bravo and Rau (2012). The campaign consisted of field visits by the minister and a team from the Ministry of Labor and Social Welfare. It was complemented by advertising placed on national television, regional radio, national and local newspapers, subways and public transportation in Santiago, as well as bus stops (Huneeus, 2010).

[7]Wage subsidies suffer from particularly low take-up rates, even when compared with other social programs. Impact evaluations of such subsidies in Turkey, Austria, Belgium, Jordan, and the United States support this claim (see Almeida, Orr, & Robalino, 2014, for an overview). The main reasons proposed for the low adoption of this type of program are the complexity or high costs of learning and application (Aizer, 2007; Bitler, Currie, & Scholz, 2003; Currie & Grogger, 2001; Dahan & Nisan, 2011; Daly & Burkhauser, 2003) and stigma (Moffitt, 1983). Furthermore, limited benefits from applying (Anderson & Meyer, 1997; Riphahn, 2001) and behavioral issues related to the profile of the payments, such as time inconsistency and present bias, have also been suggested as explanations (O'Donoghue & Rabin, 1999).

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

[9]The AFC database only includes private firms and not public firms. In Chile, firms for which the state has a stake exceeding 50% are defined as state-owned enterprises (SOEs). SOEs also include firms created by law, those in which the state is the owner and those for which the state appoints most members of their boards. According to the Chilean Securities and Insurance Supervisor (SVS), there are only 34 SOEs, the names of which can be found 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 conducted in 2013, only 22 of the 7,267 surveyed firms (0.3%) had a state contribution in excess of 50%. For this reason, the empirical strategy will not consider this point.

[10]Workers and employers' applications must be submitted at the website http://www.subsidioempleojoven.cl. Once the worker's ID number is entered, the system immediately reports whether the requirements have been met. In a favorable case, a simple and brief registration form must then be completed and submitted. The application status can be verified at any time by accessing the YES website. If the website refuses the application option on the grounds of an FPS score higher than the cutoff score, the worker can approach his/her municipality and ask for the FPS score to be recalculated. However, this request does not guarantee that the FPS score will be updated.

[11]Paperwork is necessary in very specific cases, but in these cases, the required documentation may be electronically uploaded to the website.

(Aizer, 2007; Bitler et al., 2003; Currie & Grogger, 2001; Dahan & Nisan, 2011; Daly & Burkhauser, 2003).

C. *Payments*

Once approved, the bonus (up to 20% of the wage) is paid in one installment in the second half of the next calendar year (annually paid by default). However, the worker may opt for monthly payments that begin to be paid within a maximum period of 90 days after application.[12] Hence, the waiting period between application and the first payment is relatively brief, and a behavioral explanation (time-inconsistency bias) for low take-up rates should not apply in this case (O'Donoghue & Rabin, 1999).

In both cases (annual or monthly payments), the subsidy is distributed through the payment method chosen by the employee: cash or a bank deposit, which means that the payments are private information that is difficult for colleagues to obtain unless a YES recipient explicitly discloses receiving the bonus. The privacy of payments renders stigma effects less likely to be a factor explaining the low take-up rate of the program (Moffitt, 1983; Riphahn, 2001; Bursztyn, Egorov, & Jensen, 2019).

For employers, the subsidy (up to 10% of the wage) is paid in monthly installments that begin within a maximum period of 90 days from the date of submission, in the form of a direct deposit into a checking account (it is not a tax credit).[13]

D. *End of Payments*

The benefit is retained as long as the recipient continues to meet the above criteria. Beginning in April 2011, a worker who is 21 years old or older must have obtained a high school diploma to access or continue receiving the subsidy. Subsidy payments cease during months in which the worker's employer fails to pay his/her social security contributions (or pays late) and resume when contributions are again up to date.[14]

IV. Data

A. *Various Sources of Administrative Records*

We have access to four different administrative datasets that can be merged using a unique identification number (ID): FPS, Unemployment Insurance (AFC), Chilean Student Registration (RECH), and the YES.

The FPS database includes the individual's date of birth, sex, educational level, municipality of residence, an indicator for whether the person was born in Chile, and the FPS (proxy means test) score.

The AFC database contains matched employer-employee data with information on all of the workers who have ever contributed to the Chilean unemployment insurance (UI) system since it was introduced in October 2002. These are formal, dependent, employed workers from the private sector (not the public sector—see footnote 10). It includes information about each employee's total taxable income, social security contributions, and the number of months with taxable income in the last six and 12 months.

Together, the FPS and AFC databases provide information on unemployed individuals and on workers and their firms of employment before and after the YES program began.[15] It allows us to determine whether a worker meets the YES eligibility criteria, the labor network of formal dependent workers, individual and mean peer characteristics, and characteristics at the employer level. We have access to a panel from December 2007 to September 2013 with periodicity of March, September, and December.

The RECH database contains information about the academic history of students enrolled at primary and secondary educational institutions (other than higher education) between 2002 and 2012. The variables include the school ID under which the student is registered, grades, and classroom codes. The RECH database allows for exact identification of classmates at different educational institutions, years, and grades.

Regarding the program, the YES database contains administrative lists of all payments from 2009 to 2012. It contains an indicator of monthly or annual payment and the month and year of the subsidy. This database allows us to determine when a worker began to receive YES and whether, at any time t, the worker is receiving YES. Once the unique individual identifiers are used to merge the data, we obtain a sample of 394,668 individuals. The estimating samples vary from 50,746 to 190,541 observations due to eligibility requirements, network definitions, and missing values. In online appendix tables B2 and B3, we report descriptive statistics for the entire sample and three different estimating samples.

B. *Network Definitions and Working Sample*

We study two types of reference groups: coworkers and classmates. The literature has provided extensive evidence on the importance of peer coworkers in one's decision making. For example, Glitz (2017) reportes that coworker-based networks increase the probability of re-employment in Germany after a layoff. Related to the adoption of social programs, Dahl et al. (2014) find that coworkers (and family

[12] According to Bravo and Rau (2012), in the 2009–2010 period, approximately two-thirds of YES beneficiaries opted for monthly payments.

[13] Online appendix table B1 displays the amount of the subsidy to which eligible workers are entitled in a phase-in, plateau, and phase-out manner in 2009. For employers, the amount of the subsidy is simply half of that in table B1.

[14] Two particular groups of workers have the right to request additional time to access YES: (i) workers who have completed regular studies in a higher education institution and (ii) mothers, who may request an extension for each child born alive within three months of her 25th birthday.

[15] At the time when the data were delivered to us by the Ministry of Social Planning, the AFC database was already merged with the FPS dataset.

members) positively affect paid paternity leave participation in Norway.

In contrast, this paper is the first to study the role of (former) classmates in the adoption of a social program. This may be particularly important since there is some evidence that classmates can affect labor market outcomes. In Chile, Zimmerman (2019) find that admission to a selective college program increases the number of leadership positions that students hold at firms by 44%, although gains are more significant for those who attended elite private high schools and are negligible for students who did not. There is also evidence for Norway that peer composition in ninth grade, as measured by the proportion of girls in the class, affects long-term outcomes, such as education and labor market outcomes (Black, Devereux, & Salvanes, 2013).

Studying a social program since the beginning of its implementation has one particular advantage. We can use networks formed before the announcement of the program. In this case, endogenous group membership should not create bias since changes in group membership that occur after the introduction of the program are orthogonal to changes in z_{1g}.

When constructing the coworker network, we proceed as follows. For any type-2 individual, we define his/her reference group as all coworkers during December 2008 whose wages or ages were within the small window previously defined (type-1 individuals). We choose this date because it is the closest date available in our sample to the YES announcement (March 2009).[16]

Conversely, when constructing the classmate network, we define its reference group as all former classmates in eleventh and twelfth grades whose wages were within the aforementioned small window.[17] Finally, the window widths in our baseline estimation are CLP \$70K and 260 days (similar to Dahl et al. [2014]) for the coworkers' network and \$65K for the classmate network.[18] With these window widths, we obtain estimating samples of 50,746 to 190,541 individuals with at least one peer inside the "window." The average network sizes vary from 2.5 to 3.9 peers (see online appendixes B2 and B3 for details). Due to the lack of overlap between the two networks, the construction of the instruments and the estimation of distinct forms of peer effects are based on separate models that shut down one possible form of the channel.[19]

V. Results

A. *Assessing the Validity of the Identification Assumptions*

Our identification strategy assumes that, in a small neighborhood around the assignment variable cutoffs (wage and age), eligibility is as good as random. Thus, as in a regression discontinuity design, identification fails if individuals perfectly manipulate the assignment variable (McCrary, 2008; Lee & Lemieux, 2010).

In online appendix figure B1, we present the densities of wages and ages in December 2008 to assess whether individuals manipulate this variable to access the program. The results show a smooth density function for wage, panel A, and age, panel B, at their respective cutoff points. In online appendix figure B2, we present the densities for other dates and other measures of wages.

Moreover, as in any instrumental variable approach, the validity of the instrument depends on the absence of correlation between the error term and the instruments themselves (clean instruments). Hence, the instruments operate through a single known causal channel and induce random assignment throughout the population. In online appendix table B4, we regress a variety of mean type-1 characteristics ($\bar{x}_{1g}$) at the classmate and coworker levels on each instrumental variable ($\bar{z}_{1g}$) separately as a validity test for the instruments. Online appendix table B5 shows regressions of type-2 characteristics (x_{2g}) on each instrumental variable ($\bar{z}_{1g}$) to assess whether these characteristics are correlated with the instruments. The results indicate that most of the estimates are small and nonsignificant.[20]

B. *Main Results*

Table 1 shows the estimation results of equations (3)–(5) using the two instruments. We focus on the effect of peers who adopt the subsidy in 2009 on individuals' adoption in the subsequent years (2010–2012) to reduce the incidence of the reflection problem, as discussed in section II. The set of covariates used and the network's construction are based on pre-treatment information (December 2008). To avoid correlated unobservables, we use a large set of covariates. Unless otherwise stated, all of the specifications include own age, age squared, sex, vulnerability score, ln(wage), and years of education and peers' sex, vulnerability score, and years of education. Standard errors are clustered at the group level.[21]

We implement the estimation separately for the wage (panel A) and the age (panel B) instruments. Column (1) shows first-stage estimates; there is a strong relationship between y_{1g} and $\bar{z}_{1g}$ using both instruments, with F-statistics greater than 20 for the wage instrument and greater than 159 for the age instrument. Note that, when using the age instrument, we focus only on the coworker network since we do not have sufficient variation in the instrument for the classmate network (classmates have nearly the same age). The

[16] Age was measured in July 2009—the month YES started—using the date of birth.

[17] Note that they are not only schoolmates but same-grade classmates.

[18] In section VC we provide a sensitivity analysis of the results using different window sizes for the instruments.

[19] Only 5% of coworkers are classmates, and only 2.5% of former classmates are coworkers.

[20] The coefficients for age are small but significant in columns (3) and (5) of tables B4 and B5. The negative relationship between age and the age instrument is expected since the younger the peers, the larger the proportion of eligible ones. Similarly, for the wage instrument, the younger the peers, the lower their wages, and the higher the share of eligible ones. We control for own and peers' ages in different specifications in sections VB and VC.

[21] Some specifications add additional covariates; see the text.

TABLE 1.—EFFECT OF PEERS' ADOPTION IN 2009 ON YES ADOPTION DURING 2010–2012

		First stage (1)	Reduced form (2)	Second stage (3)	N (4)
Panel A. Wage instrument					
1.	Coworkers	0.0490***	0.0143**	0.2916**	173,901
		[0.0104]	[0.0069]	[0.1472]	
	F-statistic	22.4			
2.	Classmates	0.0889***	0.0128*	0.1441*	50,746
		[0.0199]	[0.0071]	[0.0834]	
	F-statistic	20.1			
Panel B. Age instrument					
1.	Coworkers	0.0418***	0.0090*	0.2162*	190,541
		[0.0033]	[0.0047]	[0.1120]	
	F-statistic	159.4			

Regressions include as covariates type-2 individuals' age, age squared, sex, vulnerability score, $ln(wage)$ and years of education as controls. They also include mean type 1 age, sex, vulnerability score, $ln(wage)$ and years of education. Column (1) shows first-stage coefficients, and column (2) shows reduced-form coefficients. Column (3) shows 2SLS coefficients. Adopters during 2010–2012 are those who received any YES payment between 2010 and 2012 only and not during 2009. Panel A uses the wage instrument, while panel B uses the age instrument. Clustered standard errors at the group level appear in brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

TABLE 2.—ROBUSTNESS CHECKS

		First stage (1)	Reduced form (2)	Second stage (3)	N (4)
Panel A. Wage instrument					
A.1 Coworkers' network					
1.	No controls	0.0570***	0.0180**	0.3156**	173,901
		[0.0104]	[0.0076]	[0.1363]	
2.	Individual controls	0.0567***	0.0155**	0.2740**	173,901
		[0.0104]	[0.0073]	[0.1292]	
3.	Individual + group controls	0.0490***	0.0143**	0.2916**	173,901
		[0.0104]	[0.0069]	[0.1472]	
A.2 Classmates' network					
1.	No controls	0.0926***	0.0231***	0.2493***	50,746
		[0.0204]	[0.0076]	[0.0905]	
2.	Individual controls	0.0905***	0.0147**	0.1623*	50,746
		[0.0203]	[0.0072]	[0.0834]	
3.	Individual + group controls	0.0889***	0.0128*	0.1441*	50,746
		[0.0199]	[0.0071]	[0.0834]	
Panel B. Age instrument					
B.1 Coworkers' network					
1.	No controls	0.0421***	0.0130**	0.3094**	190,541
		[0.0033]	[0.0053]	[0.1263]	
2.	Individual controls	0.0421***	0.0105**	0.2501**	190,541
		[0.0033]	[0.0048]	[0.1156]	
3.	Individual + group ontrols	0.0418***	0.0090*	0.2162*	190,541
		[0.0033]	[0.0047]	[0.1120]	

This table sequentially adds the covariates mentioned in each row. First, we estimate the model without covariates. Then, we add a vector of individual covariates, and finally, we estimate a specification with individual and group covariates. See table 1 for details of the specification. Clustered standard errors at the group level appear in brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

estimates show that having a 10 pp higher fraction of eligible coworkers increased adoption by between 0.42 and 0.49 pp, and a 10 pp higher fraction of classmates increased adoption by 0.89 pp. These effects are statistically significant at the 1% level.

Column (2) shows the reduced-form estimates. This column shows that a worker is between 0.09 and 0.14 pp more likely to adopt YES if he or she has a 10 pp higher fraction of eligible coworkers and 0.13 pp more likely if he or she has a 10 pp higher fraction of eligible classmates. These effects are statistically significant at the 5% and 10% levels, respectively.

Finally, column (3) shows the 2SLS estimates. Workers with a 10 pp higher fraction of coworkers with YES are 2.16–2.92 pp more likely to adopt YES, and those with a 10 pp higher fraction of classmates with YES are 1.44 pp more likely to adopt the subsidy. While the context differs, our peer effects estimates for the coworker networks (0.29 pp) are between those found by Dahl et al. (2014) and Bobonis and Finan (2009).

Online appendix table B6 provides heterogeneity analyses, in which networks with younger and less-experienced workers show larger peer effects estimates.

C. *Robustness Checks*

Table 2 summarizes the robustness of our results to different specifications. First, we estimate the model without covariates. Then, we add a vector of individual controls, and finally, we estimate a specification with individual and group covariates. Panel A shows the results for the coworker and classmate networks using the wage instrument. The results are overall robust to the three specifications and consistent with those reported in table 1. Peer effects remain stable and significant, ranging from 0.29 to 0.32 in the coworker network. When using the classmate network, there is more variance, but the peer effects are significant, ranging from 0.14 to 0.25. Panel B shows the same specifications as those in panel A but using the age instrument. The results are consistent with those using the wage instrument, with estimates ranging from 0.22 to 0.31.

The online appendix shows that the results are robust to the inclusion of the average value of the running variable as a control (table B7). Moreover, the results are not sensitive to controlling for the proportion of individuals who have peers close to each of the eligibility cutoffs (table B8) or to the inclusion of polynomials of the share of eligible individuals in the overall reference group (table B9).[22] Lastly, when the networks are not restricted to peers in the window only, the peer effects increase and become noisier. This result is expected since using all peers changes the endogenous peer effect variable and, in turn, the first stage, but it does not affect the reduced form. Now, since the share of total peers adopting changes less with the instrument than the share of "local" peers (those adopting in the window), a smaller first-stage coefficient generates a larger 2SLS coefficient. See online appendix table B10 for more details.

Figures 3 and 4 provide a sensitivity analysis of the results using different window sizes for the instruments. Figure 3 shows that the results are robust to different window sizes of the wage instrument for the coworker and classmate networks, and figure 4 shows the sensitivity of the results to different window sizes of the age instrument. The results are robust to different age windows.

Finally, in online appendix figures B3 and B4, we provide placebo estimates of the peer effects using different cutoff points for the wage and age eligibility rules. The results indicate that moving the cutoff points from their actual values decreases the estimated peer effects to close to zero. In what follows, we analyze other aspects of the peer effects using the coworker network only. The main reason is because, for the coworker network, we have the two instruments, while for the classmate network, we have only the age instrument since we do not have sufficient variation in age for classmates. Moreover, as displayed in tables 1 and 2, the results for the coworker network are more stable across different specifications than those from the classmate network.

D. Networks With Weaker and Stronger Ties

Table 3 presents evidence on the sensitivity of the peer effects to networks with weaker and stronger ties using both instruments: wage and age. First, we consider peer effects among "former" coworkers, defined as the person's coworkers in March 2008 (one year before the announcement of the YES program), which is expected to be a weak-tie network since it is likely that they are no longer coworkers (due to the high turnover rate in the Chilean labor market) and might have lost contact and thus the ability to share information.[23] Panel A shows that the point estimate using the wage instrument is approximately 50% that from the baseline specification (the network constructed with coworkers in December 2008). Panel B shows that the peer effect is nonsignificant for the age instrument and smaller in magnitude than the baseline specification. Hence, the results are overall weaker in this weak-tie network for both instruments.

Then, we construct an alternative network that includes an individual's coworkers between September 2008 and December 2008, which we call "longer-term coworkers." These networks are expected to be strong-tie networks compared to the baseline network since working with peers for a more extended period increases the likelihood of interaction and the diffusion of information about the program. As panels A and B of table 3 reveal, peer effects in the strong-tie network are statistically significant at the 5% and 10% levels for the wage and age instruments, respectively, and the point estimates are larger than those in the weak-tie network and similar to those of the baseline specification.

E. Evolution

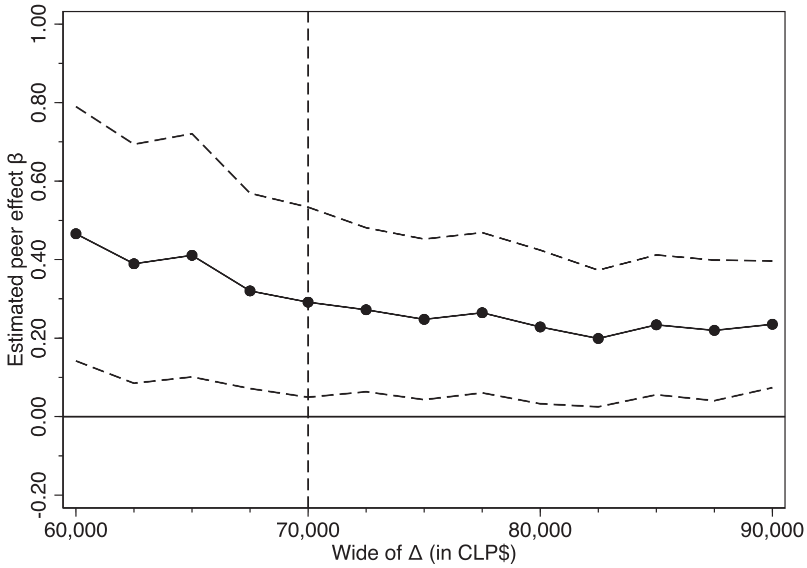
As discussed in Section VB, thus far, we have reported the effects of peer adoption in 2009 on individual adoption in the subsequent years (2010–2012). To analyze the evolution of the adoption process, we present the effects of peer adoption in 2009 on individual adoption within different time frames. In particular, we study the effect of peer adoption in 2009 on individual adoption in 2010, 2010–2011, and 2010–2012.

Table 4 presents the evolution of the estimated peer effect for the coworker network. As panels A and B show, the peer effect exhibits an increasing pattern but at a decreasing rate. Regardless of the instrument used (wage or age), the effects of early adopters (in 2009) on new adopters are sizable and significant, ranging from 0.13 to 0.23 pp in the year after the implementation of the program (2010). These figures are close to the estimates for the entire period (2010–2012),

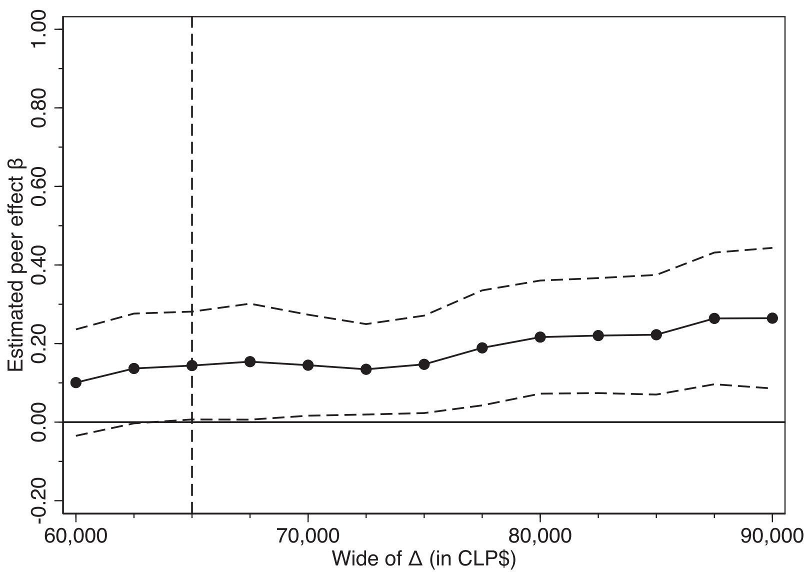
[22] Note that, even if a particular peer being bellow or above the eligibility cutoff can be considered as good as random, the number or proportion of individuals in the reference group close to each of the eligibility thresholds is not necessarily random. This threat to identification is analogous to those in models aiming to understand the outcomes of close elections in multiple members' decision-making bodies (e.g., Clots-Figueras [2012]). As seen in online appendix tables B7 and B8, our results are robust to the inclusion of the proportion of individuals who have peers close to each of the eligibility cutoffs or to the inclusion of polynomials of the share of eligible individuals in the overall reference group. We thank an anonymous referee for pointing this out.

[23] According to Albagli et al. (2017) Chile ranks highest among the OECD countries regarding employee turnover, with sizable job creation and destruction rates at the firm.

FIGURE 3.—SENSITIVITY OF PEER EFFECTS TO DIFFERENT WINDOW WIDTHS USING THE WAGE CUTOFF



Panel A. Coworkers' networks



Panel B. Classmates' networks

Each dot in panels A and B represents the estimated peer effect β from equation (4) for the given window width shown on the horizontal axis. Each dot in panel A represents the 2SLS coefficient in the coworkers' network, while each dot in panel B represents the 2SLS coefficient in the classmates' network. All of the regressions use the specifications in table 1. The dashed lines are the 95% confidence intervals, with clustered standard errors at the group level. The vertical lines show our selected window widths of $70,000 and $65,000, respectively.

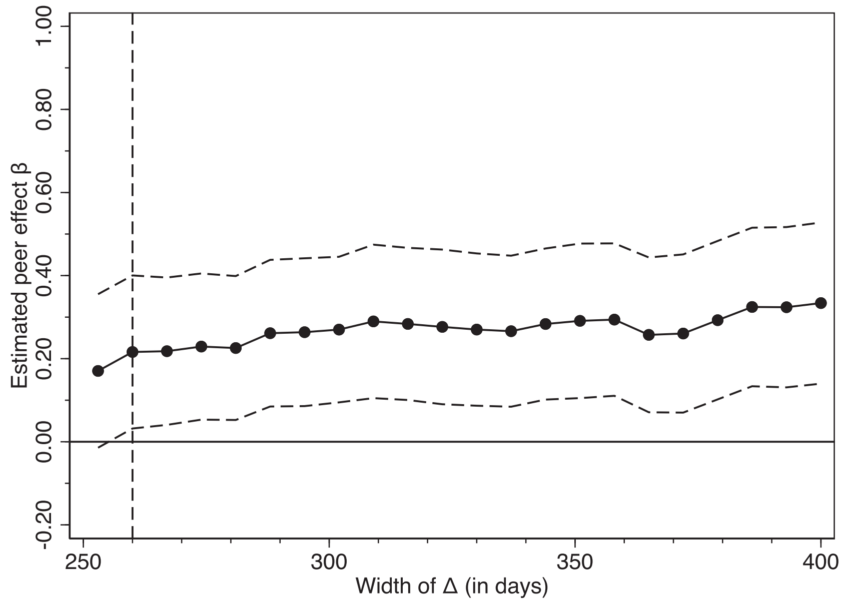
suggesting that the long-term effect of peers is mostly determined during the early stages of program implementation.

VI. Discussion

The results in section V reveal significant peer effects on the adoption of the program. In this context, peer effects are likely due to information transmission, which translates into adoption. Indeed, in a survey conducted in 2014, 53% of YES recipients said that they learned about the program through friends, family members, and acquaintances (ACNEXO, 2015).

Hence, a natural mechanism explaining our results is "social learning" (Bursztyn et al., 2014). Peers learn about the benefits and costs of program participation from one another. In this setting, the initial information set regarding the benefits and costs of participation was limited since it was an incipient program. Thus, information transmission among peers might play a crucial role in program participation, especially during the first months of program implementation. As shown

FIGURE 4.—SENSITIVITY OF PEER EFFECTS TO DIFFERENT WINDOW WIDTHS USING THE AGE CUTOFF



See figure 3 for a description. Standard errors are clustered at the group level. The vertical line shows our selected window width of 260 days.

TABLE 3.—PEER EFFECTS IN THE COWORKER'S NETWORK WITH STRONGER AND WEAKER TIES, 2010–2012

		First stage (1)	Reduced form (2)	Second stage (3)	*N* (4)
Panel A. Wage instrument					
1.	Former coworkers	0.0504***	0.0062*	0.1238*	113,867
	(network in Mar 2008)	[0.0119]	[0.0033]	[0.0707]	
2.	Baseline	0.0490***	0.0143**	0.2916**	173,901
	(network in Dec 2008)	[0.0104]	[0.0069]	[0.1472]	
3.	Longer term coworkers	0.0642***	0.0224***	0.3493**	97,906
	(network in Sep 2008–Dec 2008)	[0.0114]	[0.0082]	[0.1361]	
Panel B. Age instrument					
1.	Former coworkers	0.0415***	0.0059	0.1424	114,096
	(network in Mar 2008)	[0.0037]	[0.0056]	[0.1347]	
2.	Baseline	0.0418***	0.0090*	0.2162*	190,541
	(network in Dec 2008)	[0.0033]	[0.0047]	[0.1120]	
3.	Longer term coworkers	0.0415***	0.0090*	0.2181*	107,755
	(network in Sep 2008–Dec 2008)	[0.0042]	[0.0054]	[0.1314]	

Specifications mirror the baseline specification described in table 1. "Former coworkers" are those who were coworkers in December 2007 (one and a half years before the beginning of the program). "Baseline" corresponds to our preferred network, constructed with coworkers in December 2008, and "Longer-term coworkers" are those who worked at the same firm from March 2008 to December 2008. Clustered standard errors at the group level appear in brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

in table 3, peer effects are larger in strong-tie networks, in which interactions and the diffusion of information about the program are more likely to occur.[24]

Our results for peer effects in the adoption of YES suggest that studying social interactions in program participation is relevant for policy. As discussed earlier, peers could serve as a source of information transmission about the costs and benefits of social programs in settings where information is scarce, and perceptions are in their formative stage (Dahl et al., 2014). This finding is particularly relevant since many programs suffer from low take-up rates, and non-take-up behavior could be a serious threat to the efficiency of public policy in reaching the intended population. For example, during November 2014, the Ministry of Labor conducted a media campaign encouraging the 122,000 YES beneficiaries to claim US$37,029,382 in unclaimed payments. The accumulation of unclaimed payments is sometimes due to unfamiliarity with how YES works.[25]

[24]While we cannot exclude the "social utility" channel discussed by Bursztyn et al. (2014), we believe that it is unlikely that one's utility of possessing a subsidy (for vulnerable youth) depends on the possession of the subsidy by another individual. As discussed by Moffitt (1983) "being on welfare carries some disutility, possibly arising from the stigma of being on welfare." Hence, we argue that the stigma effect reduces the plausibility of the social utility mechanism.

[25]24 Horas (October 24, 2014) "Bonos y subsidios Sence: llaman a cobrar $21 mil millones no reclamados." 24 Horas, retrieved from https://www.24horas.cl/economia/bonos-y-subsidios-sence-llaman-a-cobrar-21-mil-millones-no-reclamados-1468667.

TABLE 4.—EVOLUTION OF PEER EFFECTS OVER TIME, COWORKERS' NETWORK

		First stage (1)	Reduced form (2)	Second stage (3)	N (4)
Panel A. Wage instrument					
1.	2010	0.0490***	0.0115*	0.2354*	173,901
		[0.0104]	[0.0065]	[0.1361]	
2.	2010–2011	0.0490***	0.0136**	0.2775*	173,901
		[0.0104]	[0.0067]	[0.1435]	
3.	2010–2012	0.0490***	0.0143**	0.2916**	173,901
		[0.0104]	[0.0069]	[0.1472]	
Panel B. Age instrument					
1.	2010	0.0418***	0.0055	0.1324	190,541
		[0.0033]	[0.0038]	[0.0911]	
2.	2010–2011	0.0418***	0.0081*	0.1927*	190,541
		[0.0033]	[0.0046]	[0.1099]	
3.	2010–2012	0.0418***	0.0090*	0.2162*	190,541
		[0.0033]	[0.0047]	[0.1120]	

This table shows the effect of peers' adoption in 2009 on adoption in different time frames using the same specifications as in table 1. Clustered standard errors at the group level appear in brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

In addition, (endogenous) peer effects can amplify the impact of changes in the distribution of private incentives and resources (Maurin & Moschion, 2009). This amplification effect, known as a "social multiplier," affects the take-up of social programs. Using the framework of Glaeser, Sacerdote, and Scheinkman (2003), the social multiplier roughly equals $1/(1-\beta)$ for sufficiently large groups, giving us social multiplier estimates between 1.3 to 1.4 for the coworker network and of 1.2 for the classmate network. Our estimates for the coworker network are in line with those found by Glaeser et al. (2003) for Dartmouth College roommates. However, since we analyze the effect of peers who adopt in 2009 on individuals' adoption in 2010–2012, our estimates of the endogenous peer effect and the social multiplier might be downwardly biased. As in Rege et al. (2012), such bias can occur if the time required for peer participation rates to converge extends beyond this period.

According to our results, these social multipliers are higher among younger workers with less working experience and in networks with closer peers (measured by being at the same firm for at least four months). The policymaker can exploit the structure of the network to target interventions efficiently. For example, an information campaign to increase the take-up of the program could be targeted to firms with younger workers and lower turnover rates.[26]

VII. Conclusions

This paper estimates the effect of peers on the adoption of the Youth Employment Subsidy (YES) program in Chile. We follow an instrumental variable approach by exploiting the exogenous variation in eligibility induced by discontinuities in the assignment rule. To address endogenous group membership, we use networks constructed with preprogram information, and as a result, all of the changes in group composition are likely to be orthogonal to the introduction of YES.

[26]Bursztyn et al. (2014) come to a similar conclusion about providing information to affect behavior in settings with social interactions.

The results indicate that coworkers and classmates play a crucial role in determining participation in the program. A 10 pp higher fraction of coworkers with YES implies a 2.9 pp greater individual probability of adopting YES; a 10 pp higher fraction of classmates with YES increases the individual probability of adopting the program by 1.4 pp. The results are robust to different specifications and mostly determined during the early stages of program implementation.

We present suggestive evidence favoring an informational channel driving our results. Peer effects vary by network characteristics and the strength of network ties. In particular, peer effects are larger in strong-tie networks and in networks with younger and less-experienced workers.

Finally, our results have significant implications for public policy. Not only coworkers but also classmates could play a significant role in information transmission regarding labor market policies for youth, especially for new programs for which information is scarce.

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