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Evaluation of the labor and edu ational impact of P OGRESAR

Evaluación del impacto laboral y educativo de PROGRESAR

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ABSTRACT

Programa de Apoyo a los Estudiantes Argentinos (PROG.R.ES.AR) is a conditional cash transfer targeted to students from disadvantaged households. This program intends to tackle youth disparities encouraging youth's human capital investments to improve labor profiles. In terms of population reach and fiscal effort, PROG.R.ES.AR is the main policy aimed at young people in Argentina and one of the most relevant in Latin America. This work identifies and tracks its beneficiaries over time, in order to evaluate the impact of the program through labor indicators as well as educational achievements. Difference-in-difference and discontinuous regression methodologies were used. The results do not allow us to account for a significant impact on the activity and employment rates of the program's beneficiaries, nor for an improvement in educational achievement

KEY WORDS: human capital, labour demand, conditional cash transfers, education, impact evaluation

RESUMEN

Programa de Apoyo a los Estudiantes Argentinos (PROG.R.ES.AR) es una política direccionada a jóvenes de hogares desfavorecidos Esta transferencia monetaria condicionada, por población alcanzada y esfuerzo fiscal, constituye la principal iniciativa en Argentina hacia el segmento y una de las más relevantes en América Latina. Este trabajo identifica y sigue en el tiempo a sus beneficiarios, con el objetivo de evaluar el impacto del programa por medio de indicadores laborales, así como de logros educativos. Se utilizaron las metodologías de diferencias en diferencias y de regresión discontinua. Los resultados no permiten dar cuenta de un impacto significativo en las tasas de actividad y empleo de los beneficiarios del programa, ni de una mejora en el logro educativo.

PALABRAS CLAVE: capital humano, demanda laboral, transferencia monetaria condicionada, educación, evaluación de impacto

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INTRODUCTION

Almost five million people between 18 and 24 live in Argentina, representing 11% of the total population and 17% of those of working age. This group exhibits greater turnover rates and a large share in precarious and low-productivity jobs (Maurizio, 2011). Lower termination costs, both direct (severance pay) and indirect (replacing vacant jobs or skills), makes their working life more sensitive to macroeconomic turmoil, being the first to be fired during recessions, having to wait well into recovery until getting hired again (ILO & ECLAC, 2022). By 2018, six out of ten people under 20 years who had a salaried job, had an informal job. This number only decreased to four out of ten among those between 20 and 25, for which unemployment was also triple than population average. Furthermore, persistent skill mismatch, typical for the Argentine labor market (Marshall & Groisman, 2013), is especially pressing for youths (Favata, Leone & Lo Cascio, 2021)., could cast a long shadow on a worker's life through a lower accumulation of human capital and credentials.

Faced with this problem –replicated at both the regional and global levels (Bussolo et al., 2018; ILO, 2020) – governments have responded with distinct kinds of “youth policies”. An Argentine forerunner was Proyecto Joven (1994-2000), aimed at improving labor insertion through on-the-job training of semi-skilled youths, which latterly re-assembled as Programa Jóvenes con Más y Mejor Trabajo (PJMyMT). In 2014, the Argentine Government launched what would be the most significative youth policy in the country: the Programa de Respaldo a Estudiantes Argentinos (Program of Support for Argentine Students, or “PROG.R.ES.AR” for short), whose aim is to encourage education and training, and thus to improve the young people employability.

The program focuses on underprivileged individuals aged between 18 and 24. With coverage of up to one million beneficiaries, depending on the year, this program represents between 0.2% and 0.3% of GDP, making it Argentina's most important youth policy and one of the most significant in Latin America. PROG.R.ES.AR supports the completion of secondary schooling and higher education studies with grants and, to a lesser extent, on-the-job training. Human capital accumulation is in the Government's approach to the problem of youth employment: it aims to reduce multidimensional deprivations and break intergenerational poverty and inequality traps through a cash transfer conditioned to enrollment or training.

By design, this program targets individuals from low-income households. We show targeting is overall successful in Table A1: even under conservative baseline assumptions, the program has a positive impact on various inequality measures, both at the individual and household level, although of little magnitude compared to the projections prior to the application of PROG.R.ES.AR carried out by De Giovambattista, Gallo and Panigo (2014). Following Bustos and Villafañe (2011) and Gasparini, et al. (2017), we simulate a counterfactual distribution where the individual and household incomes are compressed by the amount of the cash transfer.

This article presents an evaluation of the impact of PROGRESAR on employment and educational outcome (activity rate, employment, years of schooling and school attendance) using microdata from the Permanent Household Survey (EPH) carried out by the Argentinian national statistical office (INDEC). Our database allows us only to track individuals four quarterly observations, with two periods of intermittency. The PROG.R.ES.AR (as any public aid) was neither randomly assigned nor accompanied by a publicly available comprehensive dataset that may allow for follow-ups of the beneficiary population. The absence of these features determines both the data and the empirical strategy for assessing the program's impact on any outcome, as the EPH does not include questions that let us to identify PROG.R.ES.AR beneficiaries. Consequently, we define the 'treatment' and 'control' groups based on PROG.R.ES.AR eligibility and the total public aid amount. Two econometric approaches are used: a discontinuous regression design (DDR) and a difference-in-differences (DiD) model with treatment assignment at different periods, based on different identification strategies and takes place in a different time.

Firstly, the RDD estimate is conducted for 2014-2015 when PROG.R.ES.AR starts as a widespread public aid. Due to eligibility conditions, this allows legitimately to use age as a forcing variable, using the threshold of 25 years, beyond which any individual is automatically excluded or have been excluded. In this design, the difference around the threshold between the eligible and non-eligible groups according to age is compared for those complying with all other eligibility conditions and those not complying with them (as a placebo). The period guarantees that the comparability in the threshold is not compromised by treatment accumulation from previous periods.

In turn, the DiD model is estimated for 2018-2020 to observe potential impacts on new entrants. Based on individual and household eligibility conditions, we identify eligible group, and among them, treated and control groups. The strategy is like Bustos and Villafañe (2011), who instrument total public financial aid (and from other entities) and scholarships in the eligible group, disentangling compatible amounts with the policy's cash transfer.

Combining these approaches, (RDD and DiD), we arrive at comparable results. No significant effects are found on the impact of PROGRESAR on labor force participation, employment, years of education or school attendance. Despite identification affairs in the available database, results align with Peña (2016) concerning attendance rates, and unemployment-employment transitions from Jiménez-Martínez & Jiménez-Martínez (2019).

To the knowledge of the authors, this work represents the first contribution that seeks to analyze the impact of PROG.R.ES.AR through a panel analysis, including a strategy that allows to identify beneficiaries and track them over time, even despite a database that does not allow a direct identification. The article is structured as follows. First, we provide a brief review of the relevant literature on youth employment and youth targeted policies. Then, we present the data and methodology and discuss the results of the DiD and RDD framing.

Youth employment challenge has inspired a substantial body of literature in recent decades. With a pioneering study, based on the Current Population Survey (US), Feldstein and Ellwood (1979) show the importance of youths' qualifications to explain their labor trajectories, as Clark and Summers (1982) remarks its concentration among low-skilled youths and Holzer and LaLonde (1998) its employment intermittency, as well as the (non) voluntary nature of these transitions. Differences in turnover rates for young adults may reflect a virtuous cycle for ascending careers – "job shopping" and voluntary transitions Johnson (1978) –, or, on the contrary, job insecurity and forced transitions.

In the first case, voluntary mobility is associated with the initial stages of the working career, marked by a high turnover and search for better jobs as matching improves (Klerman and Karoly, 1994), with plausible positive income effects for high mobility in young people (Topel and Ward; 1992). On the contrary, the second case corresponds to precarious employment, inherently more unstable positions, and transitions that do not result in a higher rate of skill accumulation (Neumark, 1998). If this process becomes long-lasting, it can constitute a mechanism of social exclusion with permanent negative consequences (Royalty, 1998), especially for women (Corcoran; 1982), taking place either because of an erosion of human capital or if employers use prior job experience as a proxy for worker's productivity.

For Latin America, and especially for Argentina, research suggests that higher turnover is associated with young people in more precarious jobs, with lower qualifications, and in activities with larger job instability, supporting the hypothesis of involuntary transitions and a segmented labor market (Maurizio, 2011). Moreover, Argentine labor dynamics exhibit a persistent lack of correspondence between supply and demand for qualifications (Marshall & Groisman, 2013). Despite not being exclusive to the young segment, this phenomenon is more prevalent in the case of younger ages (Favata, Leone & Lo Cascio, 2021).

Governments seek to remediate this problem with public policies. These are not only focused on youth unemployment directly, but they also aim at "smoothing" the school-job transition

(Osterman and Iannozzi, 1993). Different theories justify this type of intervention. One of them posits that liquidity constraints, information failures, myopia, or misalignment between the interests of parents and children, can result in a sub-optimal level of education demand (Le Grand, Propper & Robinson, 1992). A second argument concentrates on the role of education as an equalizing force (Goldin and Katz, 2010, Alejo, Gabrielli & Sosa-Escudero, 2014). Meanwhile, a third argues for the importance of different externalities education entails beyond its private, market benefits. (Münich and Psacharopoulos 2018; Schäferhoff, et al., 2016).

On the other hand, there are arguments against these interventions. Among the most prominent of them, one goes that, due to dynamic complementarities of human capital, greater benefits could be obtained by focusing public policies on early childhood (Heckman, 2008; Bernal & Camacho, 2010; Cunha & Heckman, 2007; Bertranou & Casanova, 2015; Jenkins et al. 2003). Others qualify the notion that youth policies “arrive too late.” For example, Kilpi-Jakonen (2012) and Stenberg de Luna & Westerlund (2011) find significant effects of interventions at advanced ages. Finally, works such as those by Crépon et al. (2013) warn about displacement effects in interventions that do not increase the labor demand but rather facilitate matching for a group of beneficiaries at the expense of other workers. For Argentina, Favata, Leone & Lo Cascio (2021) show that the gap observed for positions with higher qualifications is not entirely explained by educational differences, which points to the presence of other factors beyond education in the problem of youth employment.

Among youth policies, there are active labor market policies (ALMPs) and educational incentives. ALMPs, in turn, are classified based on training (in the classroom or on the job), job search, subsidizing private employment, and those in which the Government directly becomes the employer (Card, Kluve & Weber, 2017). These authors, as well as Escudero et al. (2019), point to the first group, the one that operates via human capital formation, as the one that results in higher returns in the medium term in terms of formal employment and income, with benefits being fully counted around the second or third year since the intervention. Through education and training, this type of policy seeks to improve future performance in the labor market, reducing future income inequality (Mayer, 2002) and breaking intergenerational poverty traps (Cetrángolo, 2020). Interestingly, both meta-analyses by Card, Kluve, and Weber (2017) and Escudero et al. (2019) find no significant differences in the results when comparing experimental with quasi-experimental studies.

Regarding educational incentives, the primary means are scholarships, credits, and tax deductions. Among them, scholarships, with significant effects in reducing educational gaps in the youth segment (Deming & Dynarski, 2010), combine the advantages of lower psychological costs that credit mechanisms entail, but are associated with the risk of repayment, especially relevant for an intrinsically vulnerable population. (Marx & Turner, 2018) with larger salience, and thus larger impact, compared to tax deductions (Hoxby & Bulman, 2016; LaLumia, 2012).

Among youth policies evaluations in Latin America, the evaluation of Argentina’s Proyecto Joven shows that close to 30% of the graduates in the training courses were employed immediately after the program (Castro, 1999), decreasing more than half months after internships ended (Gluz & Moyano, 2016). In turn, Attanasio, Kugler, and Costas (2011) experimentally evaluated Colombia's Jóvenes en Acción, which between 2001 and 2005, combine three months classroom training and three months internships to young people between 18 and 25 years in lower quantiles of the income distribution, finding significant effects on their salaries (8% higher for men and 18% for women). For the Peruvian PROJoven program, Ñopo, Robles & Saavedra (2002) use a propensity score matching technique, showing that it results in positive effects regarding insertion, income, hours worked, and gender gap.

Mata & Hernández (2015) found through Propensity Score Matching method that Costa Rica's Avancemos, for ages 12 to 25, reduced dropout rates by 14% while helping children in extreme poverty get back to school. Closer to PROG.R.ES.AR, Mexico's Jóvenes Construyendo el Futuro stands out as a generous transfer aimed at generating work experience for young people and facilitating their access to the labor market. Rubio Ugalde, Razo Zamora, & Loredo Castillo (2022) use a difference-in-difference model and find no evidence of its effects. In the same country, there are other programs, although of lesser scope, such as Ingenio Joven, which aims to improve the school-work transition through scholarships in strategic areas but with lesser coverage.

Forerunners of this study are: Bustos and Villafañe (2012) and UNICEF (2017), who evaluate the impact of Argentina's Asignación Universal por Hijo with data from the Permanent Household Survey using a difference-in-differences strategy; Panigo et al. (2014), who carry out a prospective study of the distributive impact of PROG.R.ES.AR based on microsimulations; and Peña (2016) and Jiménez-Martínez & Jiménez-Martínez (2019), who, respectively, evaluate through difference-in-differences the impact of PROG.R.ES.AR on university enrollment and labor transitions. However, these authors' identification strategy relies on comparing eligible and non-eligible but not treated and controlled populations.

MATERIALS AND METHODS

Our analysis relies on microdata from the Permanent Household Survey (EPH), a nationally representative sample of Argentine communities, families, and individuals. This large-scale survey covers thirty-one urban agglomerations, with a statistical coverage rate of approximately 62% of the country's urban population.

Through a unique ID, it allows individuals to identify over four quarterly observations in a non-continuous manner. Thus, a person is surveyed for two quarters and then again for two quarters after two "resting" ones. For example, a person "i" is surveyed in the first quarter of 2018, the second quarter of 2018, the first quarter of 2019, and finally, in the second quarter of 2019. In other words, cohorts are observed four times over an 18-month window: they are interviewed in two consecutive quarters, leaving the panel in the next two, and are interviewed again in the two subsequent quarters. This allows us to create a balanced panel in both four observations, establishing transitions between the initial and final wave for both labor and educational variables.

Unfortunately, EPH respondents do not declare in a precise and detailed way social security aids, scholarships or income transfers or private aid. In general, these amounts are usually differentiated in questions related to non-labor income that refer to subsidies or social assistance (in money) from governments, churches, or other institutions (question code V5_M) and scholarships (question code V11_M). Therefore, there is an inherent uncertainty to identify PROG.R.ES.AR beneficiaries as any other public aid within the sample. Individuals declare the amount of income received from government transfers or social assistance, on the one hand, and scholarships, on the other, but without any specific reference to which program or subsidy it belongs. Therefore, identifying the treaty and control groups for the regression of differences in differences we exploit the aids information (aid amount perceived) and socioeconomic conditions of eligibility to access PROG.R.ES.AR. Peña (2016) or Bustos, Giglio & Villafañe (2012) relies on a similar approach for the Asignación Universal por Hijo; they are used to build the eligible population. Other papers of UNICEF (2017) and Garganta and Gasparini (2015) also use a similar identification methodology for social policies in the EPH.

Thus, first classification differentiates two major population groups: “eligible group” includes individuals who gather formal requirements to perceive the conditional cash transfer associated with PROGRESAR. “Non eligible group” does not have at least one of the formal requirements to receive the cash transfer. Eligible group must fulfill all below conditions to achieve the perception of public aid and included in “eligible group”:

- Being between the ages of 18 and 24
- Being Argentine or having permanent residence in the national territory (more than five years).
- Attending a middle, tertiary, or university educational establishment.
- Being either inactive, unemployed, or employed not registered in social security.
- For those employed registered in the social security, their labor income must be less than one minimum wage (2014-2018) or three (since 2018)
- For non-household head individuals, they must also belong to a household in which the head has a labor income in his main job, less than one minimum wage (2014-2018) or three (since 2018).

Considering those formal requirements, it is possible to identify both potential beneficiaries or eligible group of PROGRESAR and those who are not eligible. While the "ineligible" group includes those between the ages of 18 and 24 who do not meet one the other requirements, the "eligible" are people of the same age who meet all socioeconomic requirements, both at the individual and family level.

Regression discontinuity desig

The first strategy for the estimation of the policy impact is a Regression Discontinuity Design (RDD). It works by attributing to the treatment effect (PROG.R.ES.AR), the difference in the outcome variables on one side and the other of a threshold or cut-off value defined on another variable, known as the "forcing variable." This threshold usually results from an administrative decision and discontinuously affects the probability of receiving treatment. In other words, in the case of a first-order polynomial, is equivalent to the following equation:

$$Y_{it} = a + d\,D + b_1\,(X_{it} - c) + b_2\,D\,(X_{it} - c) + e_{it}$$

Where Y is the outcome variable (labor force participation, employment, advanced years of education), D is a dummy variable that takes a value of 1 for the values of the forcing variable (X) compatible with a higher probability of being treated given its position relative to the cutoff point (c), and a value of 0 otherwise.

Thus, the first alternative is to consider as a forcing variable the amount of income derived from transfers, subsidies, or scholarships in categories compatible with PROGRESAR. The amount of the transfer in 2014 would be taken as a limit, so it is assumed that eligible people receiving treatment remain to the right of the threshold and eligible people who do not participate in the program remain on the left side, It is worth clarifying that the amounts of complementary transfers with other pre-existing social policies that could swell the total amount of income considered as social assistance or scholarships are considered.

It is not possible to identify enough individuals who reported income from cash transfers in the amounts of PROGRESAR. This may be due to the novelty of the policy during the study period. Therefore, this approach is left aside, and we employ an alternative RDD with age as a forcing variable. This method allows for a larger sample size, enabling us to better detect the causal effect; among individuals aged twenty-five or older when the policy was launched there will be none who benefited from it, while the amount will be positive for those below the threshold. Outcomes are then compared in 2015 for those eligible according to criteria other than age, as for those not eligible (as a placebo). Thus, if PROG.R.ES.AR had an effect in its first application, discontinuities should be observed in the results at the 25-year-old threshold among eligible youth based on socioeconomic characteristics but not among those who were non-eligible.

This design, a priori, satisfies the fundamental assumptions necessary for the validity of RDD. First, only the treatment group is discontinuous at the threshold since no other factor affects the 24-year-old population that does not affect the 25-year-old population in the place and period studied. Although age and cohort effects come into play when comparing employment and education outcomes of individuals of different ages, these operate continuously throughout the forcing variable.

However, there could be heterogeneous cohort effects that correlate with program eligibility, representing a potential challenge for the RDD strategy if it explains the results. The continuity and balance of the distribution of the forcing variable around the threshold are verified with a density test (see Figure A6); this shows that the location of the individuals with respect to the threshold is not endogenous and, thus, the no selective sorting across the boundary condition, as it was expected since people do not choose their age. This methodology has the advantage of avoiding the problems related to income variables in the EPH. However, it also has disadvantages. Firstly, it only estimates the effects at the margin, that is, on the population of twenty-four. Secondly, as a discrete variable, the age also reduces variability and increases the risk of overfitting. Regarding the choice of the 2014–2015-time frame for the RDD study, it is essential to note that the duration of the Program is not limited. Thus, as the years pass since it was launched, the maximum amount of treatment an individual may have accumulated tends to vary linearly with age. As times are taken further from the year of launch, the discontinuity in the probability of being assigned to treatment disappears. For this reason, 2014 and 2015 are selected as the time target for this study, in which the relationship between the probability of assignment to treatment and age is closer to that of a truncated function.

RESULTS AND DISCUSSION

RDD results considering age in the first quarter of 2014 as a forcing variable led to positive results both for the entrance to the labor market as to get a job (Tables 1 and 2). Individuals with potential treatment are placed below the threshold of the score variable, so results tables show coefficient with opposite signs. In this sense, a higher activity and employment rate effect is observed for the population eligible to the program.

Table 1: RDD (2014-2015). Labor Force Participation (Y), Age (X)

Order Polynomial Controls	Eligible				Not eligible			
	First		Second		First		Second	
	No	Yes	No	Yes	No	Yes	No	Yes
Conventiona	**0,236 *****	**0,248 *****	**0,482 *****	**0,492 *****	**0,087 ***	**0,0810**	**0,180 ***	**0,178 ***
	(0,0310)	(0,0310)	(0,0500)	(0,0500)	(0,0420)	(0,0420)	(0,0710)	(0,0720)
Bias-corrected	**0,482 *****	**0,492 *****	**0,570 *****	**0,598 *****	**0,180 *****	**0,178 *****	**0,1330**	**0,1350**
	(0,0310)	(0,0310)	(0,0500)	(0,0500)	(0,0420)	(0,0420)	(0,0710)	(0,0720)
Robust	**0,482 *****	**0,492 *****	**0,570 *****	**0,598 *****	**0,1800**	**0,178 ***	**0,1330**	**0,1350**
	(0,0500)	(0,0500)	(0,0500)	(0,0950)	(0,0710)	(0,0720)	(0,1340)	(0,1350)
Left N effect	2144	2144	2144	2144	1052	1045	1052	1045
Right N effect	1918	1918	1918	1918	1348	1342	1348	1342

Source: own computations based on EPH-INDEC

Table 2: RDD (2014-2015). Employment (Y), age (X)

Order Polynomial Controls	Eligible				Not eligible			
	First		Second		First		Second	
	No	Yes	No	Yes	No	Yes	No	Yes
Conventiona	**0,245 *****	**0,254 *****	**0,497 *****	**0,504 *****	**0,0670**	**0,0600**	**0,159 ***	**0,155 ***
	(0,0310)	(0,0310)	(0,0510)	(0,0510)	(0,0430)	(0,0430)	(0,0740)	(0,0740)
Bias-corrected	**0,497 *****	**0,504 *****	**0,5930**	**0,615 *****	**0,159 *****	**0,155 *****	**0,1340**	**0,1370**
	(0,0310)	(0,0310)	(0,0510)	(0,0510)	(0,0430)	(0,0430)	(0,0740)	(0,0740)
Robust	**0,497 *****	**0,504 *****	**0,5930**	**0,615 *****	**0,159 ***	**0,155 ***	**0,1340**	**0,1370**
	(0,0510)	(0,0510)	(0,0970)	(0,0970)	(0,0740)	(0,0740)	(0,1380)	(0,1400)
Effect. N left	2144	2144	2144	2144	1052	1045	1052	1045
Effect. N right	1918	1918	1918	1918	1348	1342	1348	1342

Source: own computations based on EPH-INDEC

Results are robust when we include controls (gender, family income and employment and formal employment of the individual in the first quarter of 2014 and that of the head of household) and maintain its sign through different polynomial specifications. This discontinuity is notable even though activity and employment rates tend to increase when young people arrive at the age of twenty-five, leaving the youth stage, in some cases associated with full time education. Conversely, those not eligible do not present a significant discontinuity. Similarly, the assumption that there is no selective classification across the limit is verified, so these results are not explained by density differences on both sides of the threshold (see APPENDIX). However, irrelevant thresholds of placebo tests lead to statistically significant results, requiring caution when considering the importance of coefficients at the 25-year cut-off point.

On the other hand, no significant effects were found for the education year´s advance (Table 3). A plausible explanation is that the 24-year threshold is characterized by formal education period ending, or when progress in the educational career slows down or even stops. Therefore, RDD results could not be representative at the threshold, especially when individuals above the cut point (over 25 years) only a small population share continues its study.

RDD results require a limited approach around an age dimension only. Thus, PROGRESAR seems to relate to interns who do not have a job, regardless of whether they are looking for a job or not. This moderate effect of PROGRESAR around the exit of the labor force, attributed to objectives of educational advancement, still forces people to be conservative in the result given the statistical analysis of placebos. Based on this, the DDR exercise needs to be complemented as an additional econometric approach that uses a different identification strategy. The joint analysis of both results will provide greater robustness and external validity to the estimates, as well as a more complete picture of the phenomenon.

Table 3: RDD (2014-2015). Advance educational years (Y) Age (X)

Order Polynomial Controls	Eligible				Not eligible			
	First		Second		First		Second	
	No	Yes	No	Yes	No	Yes	No	Yes
Conventiona	**0.593 ****	**0.687 ****	**0.292**	**0.386**	**0.066**	**0.085**	**-0.296**	**-0.245**
	(0.235)	(0.233)	(0.379)	(0.375)	(0.310)	(0.299)	(0.514)	(0.493)
Bias-corrected	**0.292**	**0.386**	**0.645**	**0.896 ****	**-0.296**	**-0.245**	**-0.232**	**-0.101**
	(0.235)	(0.233)	(0.379)	(0.375)	(0.310)	(0.299)	(0.514)	(0.493)
Robust	**0.292**	**0.386**	**0.645**	**0.896**	**-0.296**	**-0.245**	**-0.232**	**-0.101**
	(0.379)	(0.375)	(0.688)	(0.685)	(0.514)	(0.493)	(0.940)	(0.917)
Effect. N left	1933	1933	1933	1933	946	940	946	940
Effect. N right	1645	1645	1645	1645	1015	1012	1015	1012

Source: own computations based on EPH-INDEC

In this case, the difference-in-differences approach combines treatment at different time periods, considering a pseudo panel framing. Labor and educational performance from those identified as beneficiaries (treatment group), is compared during the 2018-2020 period with a control group, composed of those individuals who make up the eligible group, but who do not receive the transfer. It is worth noting, that due to the nature of the program —non-random assignment–, treatment and control groups differ by construction.

The identification assumptions are that outcome variables of treatment and control groups would have evolved similarly in the absence of the program and that there was no other contemporaneous event to the benefit of PROG.R.ES.AR that could have caused differences in their evolution between both groups.

As for the difference-in-difference model, we use the standard linear specification:

$$Y_{it} = \alpha + \beta_1 \text{Treated}_i + \beta_2 \text{Post}_t + \gamma (\text{Treated}_i \text{Post}_t) + \theta X_{it} + \Omega_i + \phi_t + u_{ti}$$

Where Y is the relevant outcome variable as a binary indicator that takes value 1 for individuals who look for a job, are employed or move forward in their education career (depending on the outcome variable analyzed) and zero otherwise. Treated_i is a dummy for treated groups, and Post is a temporal dummy. It includes an interaction term between these last two variables, as well as a set of controls at the individual and family level (Xit), such as gender, type of employment of the head of household and region. Treatment is imposed at different time periods, so it is essential to include fixed (Ω_i) and temporal effects (ϕ_t).

Panel data available presents observed individuals four times during a temporary period of 18 months with an observation window of two consecutive quarters, leaving the panel in the next two and being interviewed again in the following two quarters. Consequently, we define four observation moments for everyone, even while treatment is not at the exact moment during the period compound by first quarter 2018 and first quarter 2020; individuals may receive treatment at different time points, considering all possibilities between period-gaps and time-specific confounder. However, the definition of the control group and treaties is made considering that in the first two time-specific moments the individuals belong to the eligible group, but do not receive a public aid compatible with the monetary transfer of PROGRESAR. Treatment is assigned between time two and time three, control groups are eligible individuals without public aid, while the treated group is composed of individuals who receive the benefit. Finally, it is at moment four where the characteristics corresponding to the returned variables are analyzed.

In this way, the DiD approach with treatment at separate times (TWFE) is complementary to the RDD approach. It uses a different identification strategy and focuses on a different time frame, increasing external validity. Therefore, the joint analysis of both results provides greater robustness and reflects a bigger picture. DiD approach is complementary to the RDD approach for two main reasons: first, RDD is only useful for initial policy periods, due to potential accumulations in individuals' benefits. This is not a limitation in DiD approach which allows a more precise discrimination between treated and control groups. In contrast, RDD faced problems that made it impossible to differentiate the two groups accurately, necessitating the use of a forcing variable, such as age.

A preliminary analysis of descriptive statistics on job transitions (see APPENDIX) shows that eligible groups are more likely to be inactivity in all states compared to the median for the age group (18-25) over the 2018-2020 period. However, among the eligible population, transitions in control and treatment groups are quite different. The control group shows much higher job retention values (66%) than the treated group (44%), and overall, the treated group has a larger transition chance to inactivity across states. In addition, employment-unemployment transitions are more than twice as frequent in the treated group.

The evidence presented in table 4 suggests that an assumption of comparability between groups based on parallel trends in the LFP and employment variables do not reinforce the confidence in our identification assumption and do not allow us to present rigorous causal conclusions. This lack of comparability may be due to the volatile nature of job transitions in young people, as well as biases inherent in the youth labor market. Determinants of youth employment and their labor income are related to the socioeconomic origin of the household, as evidenced using both MCO models and a Heckman correction model by Favata, Leone and Lo Cascio (2022) in a previous study. The authors found that higher household per capita income is associated with larger employment opportunities for young people, explaining non-parallel trends between the control and treated groups in our study.

Table 4: DiD (2018-2020). Labor Force Participation, Employment, and advanced educational years

	LFP	Employment	Advance educational years	Change in a endance rate
PROGRESAR	**-0,0107353**	**-0,0449762**	**-0,0277**	**0,0439**
Robust std. err.	**(0,0305437)**	**(0,0263751)**	**(0,1083)**	**(0,0275)**
Controls	Yes	Yes	Yes	Yes
Number of observations	6412	6412	2516	2516
Clusters (individuals)	1603	1603	629	629
Parallel trends	No	No	Yes	No

Source: own computations based on EPH-INDEC

Although no parallel trends are observed in both LFP and employment data, table 4 summarizes the results of the DiD model. No statistically significant effects were found for the results considered, with coefficients that are also around zero. In Table No. 5 we assess the inclusion of temporal effects in a TWFE framework, robust to that kind of framing.

While we verify parallel trends for the advance in years of education (Panel A10), the effect is not statistically significant. These parameters are supported by the stability in the time frame studied, while educational trajectory of both groups are similar before and after treatment, and data source could not allow to clearly capture the advance of the educational years.

We run an additional regression to analyze the impact of PROGRESAR on new entries into the educational system for eligible groups. For this, a slightly different DiD model had to be used, since attendance is now considered an outcome variable, whereas, in the previous exercise, it was used as a proxy for enrollment, that is, as a constraint defining the treated group. Thus, the control and treated groups comply with the same conditions as previous identification routine that strictly mimics the program's conditionalities, except for the attendance an educational establishment (main condition of the conditional cash transfer PROG.R.ES.AR to remain in the program), which is considered the model-dependent variable. As pre-treatment trend is not parallel, control and treated groups are not strictly comparable so therefore, other strategies should be considered in the future to evaluate the impact of PROGRESAR on enrollment or re-enrollment in an educational institution, a crucial purpose for a public initiative that seeks to increase educational credentials to improve youth employability.

In summary, although the control and treated groups are similar in terms of their socioeconomic characteristics since both belong to the eligible population group, tests of parallel trends in specific variables show that they are not strictly comparable considering activity rate and the employment rate. With size of the effect informed close to zero, it induces not to be conclusive in causal relationships. These results thus do not support the hypothesis that PROG.R.ES.AR contributes to the improvement of labor conditions, or at least do not alter labor status in a short temporal period. For the variable advance in educational years, the trends are parallel, but the associated coefficient is not statistically significant. This is a certainly non-trivial finding for a program which initially seeks to alter future labor status based on human capital accumulation, but finally limits to target schooling ending and by sure still shows room for improvement.

CONCLUSIONS

Thus, in general, the results do not show solid evidence of a significant impact of PROGRESAR on the labor or educational realm. In addition, we assess different econometric approaches for impact evaluation divergent and inconclusive results.

PROG.R.ES.AR impact evaluation conducted with data from the Permanent Household Survey, one of the few relevant public data sources in Argentina, and the unique socio-economic massive database. This presents some limitations that do not allow for follow-ups in the beneficiary population while manifest inconsistencies for short follow-up education periods.

However, given that PROGRESAR is Argentina's main youth policy, both in terms of fiscal and population scope, and one of the most important in Latin America, it aims to mitigate the problems faced by young people in the labor market through educational incentives. Due to its magnitude and progressiveness, it entails redistributive effects for targeted people aged 18-24 years are too old or young to benefit from other social policies and/or government transfers. However, no effects were found for educational and employment dimensions that may affect long-term income and multidimensional well-being.

Thus, the results do not determine a clear feature for PROGRESAR program impact, impeding clear causal interpretations. The RDD approach states a faint positive effect for the eligible population. However, due to the low variability in the independent variable and overfitting issues, we perform a series of counterfactual experiments or placebo exercises to gain more confidence in the validity of the identification assumption, with relatively doubtful outcomes. Finally, for both DiD conventional approach, and TWFE with temporal control for treatment, coefficients are not statistically significant compared as parallel trends are not guaranteed. For years of education advancement, no specification of RDD leads to a significant shift for the eligible group. The results of the DiD model show a small and non-significant effect.

In summary, no robust effects were found with the methodologies and databases used. Some evidence from other CCT programs find that the size of the impact grows with the generosity of the transfers, as PROG.R.ES.AR does not represent a sizable amount. Notwithstanding that, further studies will mandatorily require a data source with larger accuracy to recognize public aids and which also allow to more deeply follow-up individuals to fully manifest the benefits effects.

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