Growing Up Over the Social Safety Net: The Effects of a Cash Transfer Program on the Transition to Adulthood

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Abstract

Countries spend a large share of their budgets on aid to families with children, with cash transfers being one of the most used policy instruments for this purpose. In this paper, I present novel evidence about the effects of a large-scale and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*. I focus on three dimensions: education, fertility, and labor market decisions. Among other markers, these dimensions characterize individuals' transition to adulthood. I use a unique combination of individual-level administrative records that exhaustively describes the year-by-year trajectory in each one of these dimensions. The empirical strategy consists of a Regression Discontinuity Design that exploits the use of a poverty score and an arbitrary threshold to define participation in the program. The main results show that the cash transfer has mostly null effects on men's outcomes but reduces women's teenage pregnancies by 9.4p.p. (or 41.2%), with a subsequent increase in their labor market participation, months worked, and earnings. These effects peak at 25 years old, where the cumulative impact on women's months worked is around seven months. By their late twenties, the negative effect on fertility and the positive effects on labor market outcomes start to attenuate. Despite some preliminary evidence of fade-out, the observed changes in the timing of the events might have strong and lasting consequences throughout women's life cycle.

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1 Introduction

Worldwide, governments spend billions of dollars on social safety net (SSN) policies to reduce poverty, especially for vulnerable households with children.[1] Although they represent sizeable public investments and have the potential to affect multiple generations, the empirical economic literature has yet to provide a complete description of whether and how cash transfers affect individuals' life trajectories. On the one hand, there is well-documented evidence about positive short-run effects of cash transfers on children's health and education outcomes (e.g., Hoynes and Schanzenbach, 2018 for policies implemented in the United States, or Bastagli et al., 2019, 2016; Bosch and Manacorda, 2012; Fiszbein et al., 2009 for Conditional Cash Transfer programs in developing countries). On the other hand, the literature that focuses on the long run and explores the effects of cash transfers on adult outcomes for individuals that benefited from the policy during their childhood is still very incipient (Aizer et al., 2022). There are some very recent exceptions such as Barr et al. (2022) for the Earned Income Tax Credit (EITC) or Aizer et al. (2016) for Mother's pensions in the United States; or Araujo and Macours (2021) for PROGRESA in Mexico. These have found promising evidence of positive and lasting effects on educational achievement and labor market outcomes, and reductions in teenage pregnancies. However, there is still a missing piece. Due to data limitations, the existing evidence usually relies on static analyses at specific ages. For instance, the well-documented short-run evidence usually focuses on adolescents around 18 years old, while the incipient long-run literature focuses on young adults around 30 years old. However, to my knowledge, there is still no evidence that comprehensively describes how cash transfers affect individuals' decisions during the transition period that spans between these two stages of life, which has critical consequences for the rest of their lives.

This paper fills this gap by studying how a permanent, large-scale, and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*, affects education, fertility, and labor market decisions for individuals who benefited from the policy during their childhood. These outcomes are strongly correlated with poverty and opportunities for mobility and speak directly to the long-run goal of SSN interventions. Regarding timing, I focus on individuals' decisions during the period that spans between 15 and 30 years old. This is a period that sociologists and psychologists usually refer to as "transition to adulthood" or "emerging adulthood" (Settersten Jr et al., 2008; Arnett, 2000). Adulthood is a distinct and socially recognized stage of life, usually defined by a series of markers related to the culmination of education, labor market participation, residential independence, marriage, and fertility. These are critical for current and future life trajectories (Settersten Jr et al., 2008). Until recently, "adolescence" was the concept used to describe the period between childhood and adulthood. However, in the last fifty years, the transition between these two stages has become more nuanced, and the idea of a uniform "adolescence" is becoming socially and economically inexact (Settersten Jr et al., 2008). Hence, cash transfers might play a key role in shaping this transition. Understanding how is critical to assess if cash transfers effectively fulfill their ultimate goal of reducing structural

[1] The world's average SSN expenditure is 1.93% of the GDP (e.g., in Japan) and ranges between 0.01% (Cote d'Ivoire) and 10.1% (South Sudan). In the US, SSN expenditure is 1.34% of the GDP, including programs such as TANF, Child Support programs, WIC, EITC, or Food Stamps. In OECD countries, the average SSN expenditure is 2.6% (e.g., Germany) and ranges between 0.7% (Turkey) and 4.9% (Denmark). In developing countries, expenditure is considerably lower, with an average of 1.7% (Thailand) and a median of 1.23% (China)

poverty and inequality and increasing opportunities for mobility.

The research question addressed by this paper is relevant for multiple reasons. First, cash transfers are one of the most widespread anti-poverty policy instruments in the world and have the potential to affect household members' behavior through several mechanisms with long-lasting consequences. Through an income effect, cash transfers can change the bundle of goods and services consumed by households. Furthermore, in cases where the benefit is contingent on specific behaviors, such as in Conditional Cash Transfer programs (CCTs), they change the relative opportunity costs of different actions. This might induce responses on several margins through a substitution effect. For instance, they can change parents' time allocation between labor and leisure or children's school enrollment and healthcare decisions, among many others. All these responses affect the current lives of household members but can also have long-lasting consequences, especially for children. Second, recent empirical work that analyzes the effects of in-utero or early childhood interventions has shown that it is common for interventions to have effects with non-linear trajectories. Researchers have found different persistence patterns, fade-out, and re-emerging effects, depending not only on the outcome used but also on when it is measured (e.g., Chetty et al., 2011; Almond et al., 2018). Hence, an intervention's dynamic and long-term effects cannot be predicted by simply extrapolating estimates obtained in the short run and requires a specific analysis. Third, a complete picture describing the trajectory of the effects of cash transfers at different moments of beneficiaries' lives, including a long-run evaluation, can have substantial policy implications when assessing the effectiveness of a policy. For instance, Aizer et al. (2022) discuss the costs and benefits of different components of the US social safety net and argue that the direction of the comparison might change if policy evaluation includes a long-run perspective that considers the effects on individuals that benefited from them during their childhood. A deeper understanding of the effects of policy interventions throughout people's lives, rather than a few age-specific snapshots, might help improve the public debate and the design of public policies. Finally, given that most anti-poverty programs aim to create better future conditions for the beneficiary children, a medium-/long-run dynamic perspective that focuses on them seems to be one, if not the most, relevant dimension for analysis.

The program that I study, *PANES/AFAM-PE*, consists of a cash transfer representing, on average, more than 50% of the self-reported pre-program household income. As typical in CCTs, children of beneficiary households must satisfy school attendance and regular health check-up requirements to remain in the program. *PANES/AFAM-PE* has two main goals. First, in the short term, it aims to provide an additional source of income to help beneficiary households to overcome immediate needs related to their disadvantaged socio-economic status. Second, in the long run, the program aims to encourage human capital accumulation of beneficiary children, mainly through conditionalities, for a more permanent transition out of poverty. Even before its implementation, *PANES/AFAM-PE* was broadly publicized and rapidly became the most generous anti-poverty program in the country's history (Manacorda et al., 2011). At a smaller scale, *PANES/AFAM-PE* is comparable to programs such as *PROGRESA* (Mexico) and *Bolsa Familia* (Brazil) both in terms of relative size, accounting for 0.4% of the Uruguayan GDP, and coverage, reaching more than 10% of Uruguayan households (about 350,000).

Eligibility to participate in *PANES/AFAM-PE* is determined based on a poverty score. Households with a poverty score above a certain threshold are eligible to participate, while households with a score below the threshold are deemed ineligible. Hence, to identify the causal effects of the program, I use a Regression Discontinuity Design (RDD) that exploits the sharp change in the probability of treatment just at the eligibility threshold. Because the program has been in place uninterruptedly since 2005, households might have applied to it more than once and obtained multiple scores. Following the approach proposed by Jepsen et al. (2016), I use the score of the first application to the program as an instrument for treatment status. The rationale of this approach is that the first eligibility score is the one that is less subject to manipulation and provides a cleaner source of exogenous variation.[2]

To conduct the empirical analysis, I have assembled a unique and exhaustive dataset that contains individual-level information both on participation and outcomes of interest for the universe of Uruguayan individuals. This rare dataset is built on a series of administrative records provided by different government agencies that can be merged at the individual level using a unique masked identification variable. In particular, application forms were provided by the Ministry of Social Development, birth certificates by the Ministry of Health, education records by the National Council of Education and the largest University in the country, and labor market records by the Social Security Agency. In all cases, the information spans between 2005 and at least 2017. In some cases, such as labor market histories, it goes until October 2021. To my knowledge, this is the first attempt to analyze the effects of a large, government-run, and means-tested transfer program using fifteen years of longitudinal data with this degree of detail and high-quality administrative records.

This paper's main results can be split into four. In the first part of the analysis, I report the intention to treat (ITT) estimates on outcomes measured at three specific ages: 18, 23, and 30. Fertility outcomes are defined exclusively for women due to missing information about fathers on birth certificates. Education and labor market outcomes are observed both for men and women. The ITT estimates show that program eligibility before 18 years old has a negative and statistically significant effect on teenage pregnancy of -3.1p.p. (or -13.5%). This effect becomes weaker when measured at age 23 (-2.3p.p. or -4.1%), fades out, and even becomes slightly positive, when measured at age 30. The effects on education outcomes also vary depending on the age at which they are measured. The program does not affect education enrollment when measured at age 18, which reflects secondary education enrollment, or when measured at age 23, which reflects tertiary education enrollment. However, eligibility to participate in *PANES/AFAM-PE* before 18 years old reduces the probability of having being enrolled in tertiary education by the age of 30 by 3.3p.p., which measured in percentage terms is up to 48.7% due to the meager baseline enrollment rates for this population. Regarding labor market outcomes, the effects also show differential patterns depending on the age considered. There is a strong positive ITT effect on labor market participation when measured at age 23 of 3.4p.p., representing almost 6% of the baseline mean. However, the effects are null when measured at ages 18 and 30.

In cases where there is not a one-to-one relation between eligibility and treatment, ITT estimates

[2] Jepsen et al. (2016) is one of the first papers to address the issue of multiple scores in the running variable in RDDs. They do this in the context of returns to education, where students can take high-school exit exams numerous times.

should be interpreted as a lower bound for the actual (local) average treatment effects (LATE). The first stage estimates indicate that the probability of ever being a beneficiary of *PANES/AFAM-PE* before turning 18 years old increases abruptly at the eligibility threshold of the score obtained in the first application by 30p.p., or about 50%. However, there is not a one-to-one relation between these two variables. Hence, the second set of results reported in the paper corresponds to estimates based on a Fuzzy RDD, which is the classic approach in these settings. In this fuzzy design, the endogenous treatment variable is instrumented by an indicator of eligibility based on the score obtained in the first application. The results yielded by this strategy are consistent with the ITT estimates in direction and statistical significance. However, in most cases, the effect sizes are larger due to the scale-up factor estimated in the first stage. The LATE effect on the probability of giving birth at or before age 18 is -9.4p.p. (41.2%). However, these are null when estimated at ages 23 and 30. There are also strong and positive effects on the labor market outcomes when measured at age 23. In particular, participating in the program before age 18 increases by 8.3p.p. (14.3%) the probability of having a spell of four consecutive months employed in the formal sector before age 23. These effects are null when estimated at ages 18 and 30. Finally, the negative effects on education outcomes, particularly tertiary education enrollment, are still only observed when measured at age 30. In this case, the effect is -1.8p.p. (-25.17%). Both for ITT and LATE effects, estimates are similar when using a continuous variable that indicates the number of births, the number of years enrolled, or the number of months worked instead of a binary variable.

The third set of results describes the differences in the estimated effects of *PANES/AFAM-PE* on the outcomes of interest when calculated separately for men and women. It is important to note that because birth records usually have limited information about fathers, throughout the paper, the estimated effect of the program on fertility outcomes corresponds exclusively to women and there is no possible comparison with men. In terms of education outcomes, there are no differences in the estimates obtained for men and women. On the contrary, there is suggestive evidence of stronger effects on labor market outcomes for women compared to men. For men, when estimated at age 23, the effect of ever participating in *PANES/AFAM-PE* before age 18 is 3.9p.p., but statistically insignificant. For women, the estimated impact more than doubles the point estimate obtained for men (8.1p.p. versus 3.9p.p.) and is statistically significant at a 10% level. These differences are larger and even statistically significant when the outcome variable is the number of months employed in the formal sector instead of a binary participation variable.

Finally, I report estimates of the effects of *PANES/AFAM-PE* on the different outcome variables measured separately at each age within the range of 15-30 years old. This dynamic analysis helps reveal how the effects appear, fade out, and even revert throughout individuals' lives which can help understand the interactions and mechanisms behind the observed responses. Overall, men seem to be mostly unaffected by the program regardless of the outcome or the time at which it is measured. On the contrary, the dynamics observed for women illustrate a very interesting and suggestive story. The program's effects appear as early as 16 when there are signs of negative effects on teenage pregnancies. These reach their maximum (in absolute terms) at age 18. The negative effects on fertility are followed by substantial increases in labor force participation, which start as early as age 17 and remain the

same until the mid-twenties. At this age, fertility effects start to attenuate, and by age 30, they even become positive. This attenuation coincides precisely with a period when the labor market effects also become much weaker. Furthermore, even with much smaller magnitudes, a similar counter-cyclical pattern can be observed when looking at the fertility end education trends.

These four sets of results provide robust evidence that *PANES/AFAM-PE* indeed affected women's transition to adulthood by reducing teenage pregnancies and increasing labor market participation during the same period. However, the attenuation and reversal of the effects observed at age 30 suggest that responses might be more associated with changes in the events' timing rather than more general changes in fertility or labor market preferences. Despite this attenuation, the results are still relevant from a life-cycle perspective. For instance, delaying birth can have beneficial consequences throughout women's lives in terms of labor income and labor participation stability. Furthermore, early access to benefits from formal employment such as health insurance and a long history of social security contributions can have long-lasting effects. Overall, the results indicate that SSN policies can indeed be effective tools to reduce poverty in the long run.

This paper contributes to three strands of literature. First, it contributes to an incipient literature that analyzes the effects of SSN interventions on adult outcomes for individuals that benefited from them during their childhood. This literature is relatively new not only for developing countries (Molina Millán et al., 2019) but also for the US and other rich countries (Aizer et al., 2022). For the U.S., some recent exceptions show that increased access to Food Stamps or the EITC improves adult outcomes such as human capital accumulation, economic self-sufficiency, labor market outcomes, and quality of the neighborhood, among others (e.g., Barr et al. 2022; Bailey et al. 2020; Bastian and Michelmore 2018; Hoynes et al. 2016). Furthermore, the evidence suggests that these positive effects might be transmitted to future generations (Barr and Gibbs, 2022). For developing countries, there are two notable exceptions: Barham et al. (2018) and Araujo and Macours (2021). These works analyze the intention-to-treat effects of differential exposure to *PROGRESA* (Mexico) and *Red de Proteccion Social* (Nicaragua), respectively, on early adulthood outcomes. Because of data limitations, their analysis focuses on two specific cohorts of children observed several years after treatment. More specifically, Araujo and Macours (2021) find positive intention-to-treat effects of the differential exposure on educational attainment and labor expectations for the younger cohort and increased labor income and geographical mobility for the older cohort. Barham et al. (2018) find that differential exposure to *Red de Proteccion Social* did not have intention-to-treat effects on schooling outcomes but reduced fertility between 18-21 years old and increased the probability of being economically active. Finally, another exception is Attanasio et al. (2021), who analyze the effects of an expansion of a CCT program in Medellin, Colombia, on several outcomes. They find that the expansion of *Familias en Accion* reduced arrest rates, teenage pregnancy, high school dropout, and increased college enrollment.

In this regard, this paper contributes in several ways. First, by providing an additional piece of evidence to this nascent but growing literature. Second, and most importantly, by reporting evidence based on a setting and an empirical strategy that overcomes some of the typical limitations of the existing studies. These limitations usually make the interpretation and generalization of results harder. For instance, while the evidence discussed in the previous paragraph represents significant progress

in the literature, these studies usually rely on geographic or temporal variation in the rollout of a program at a local level to identify the effects of the program. Using aggregated units of analysis where treatment status cannot be determined at the individual level leads to estimates that should be interpreted as intention-to-treats effects rather than as average treatment effects. This could be a significant limitation in contexts where take-up is imperfect.[3] Having access to individual-level data and a research design that exploits changes in treatment status at the individual level allows me to provide estimates that are closer to (local) average treatment effects under some assumptions. Studies conducted in developing countries are usually subject to additional limitations related to the lack of high-quality administrative records and the consequent need to rely on follow-up survey data, traditionally comprised of very few data points for particular cohorts. Jointly with the extended use of research designs based on a staggered rollout of programs, these have hindered the possibility of a comprehensive evaluation of the effectiveness of cash transfers in the long run. To my knowledge, this is the first paper that analyzes the effects of a cash transfer program on three significant response margins (fertility, education, labor market) with a dynamic perspective (i.e., from late teens to adulthood) and using high-quality administrative records with information that varies at the individual-level both for the outcome and participation variables in a unified setting. Finally, it is also important to note that many studies in developing countries correspond to specific contexts, such as remote rural villages in low or middle-low income countries or transitory programs implemented as randomized control trials by local or international NGOs. Furthermore, even in places where these policies were implemented by the government - such as *PROGRESA*, Mexico - the program was focused on rural communities. This limits the lessons that we can learn from the existing evidence. On the contrary, *PANES/AFAM-PE* is a permanent and government-implemented policy at the national level.

Second, this paper contributes to a growing literature that discusses non-linear trajectories in the dynamic effects of different types of policy interventions. For instance, Chetty et al. (2011) present evidence in the context of Project Star where the impact of early childhood education fades out on test scores but reemerges on adult outcomes. In the case of Project Star, this non-linear pattern is associated with the fact that changes in noncognitive skills are strongly correlated with earnings but not with test scores. This shows that both the timing and the specific measure used to analyze an intervention's effectiveness matter. Similarly, in a thorough review of the role of early childhood circumstances and adult outcomes, Almond et al. (2018) present several pieces of evidence about programs that have immediate gains that fade out in the medium run but often show up again several years later. This paper contributes to the literature that discusses non-linear dynamic effects by providing evidence based on a different type of intervention, i.e., cash transfers, at ages not necessarily restricted to early childhood, which is presumably a more general setting. Furthermore, Almond et al. (2018) argue that the potential existence of non-linear patterns is one of the main reasons that justify studying the "missing middle". In Almond et al. (2018), the missing middle refers to the lack of knowledge about the middle years between early childhood and adulthood in terms of developmental

[3]There are some exceptions, such as Aizer et al. (2016) or Price and Song (2018) that do analyze the effects of a cash transfer at the individual level, but they are subject to additional limitations. For instance, Aizer et al. (2016) restrict their analysis to male children, who do not tend to change their surnames and therefore can be tracked over time. Price and Song (2018) propose a matching algorithm that only allows measuring effects on families with more than one child.

trajectories. They discuss that one of the main challenges in studying the middle years is that it requires tracking people over several years. While this missing middle refers to a slightly different context, the idea of an understudied missing middle that prevents us from understanding individuals' life trajectories also relates to the primary motivation for this paper. My paper contributes to this strand of literature by providing novel evidence about a different "missing middle".

Third, this paper contributes to the literature on gender inequality in the labor market (see general surveys in: Altonji and Blank, 1999, Blau and Kahn, 2017, or Olivetti and Petrongolo, 2016), but with a focus in the relation between motherhood and labor market outcomes (e.g., Waldfogel, 1998 or more recent works such as Kleven et al., 2019,?). Regarding this literature, my paper provides complementary evidence highlighting how fertility decisions, particularly during the critical adolescent ages, might have long-lasting consequences in terms of labor market participation, experience, earnings, and overall stability. Furthermore, this paper illustrates how cash transfer policies can be a useful policy instrument to reduce labor market gender gaps, even when they are not specifically designed for this purpose.

A final and tangential contribution of this work exceeds the specific scope of this paper and relies on the dataset assembled for the analysis. While this paper focuses on the broader picture, the unique longitudinal dataset combined with a quasi-experimental setting in which a treatment assignment rule creates a reliable source of exogenous variation for identification sets the ground for future more detailed analysis at a relatively low cost. In particular, the richness of some of the information included in the administrative records opens the door for digging deep into mechanisms behind the observed responses and analyzing other dimensions of individual behavior that could be relevant to understand the overall effectiveness of cash transfer or welfare programs.

The rest of the paper is structured as follows. In Section 2, I describe the main features of *PANES/AFAM-PE* as well as more general characteristics of Uruguay. Then, in Section 3, I discuss the main mechanisms that could drive the effects of cash transfers on the outcomes of interest, with a specific focus on how these mechanisms might interact over time. In Section 4, I describe the data used in the analysis, while in Section 5, I describe the main features of the Regression Discontinuity approach used to estimate the causal effects of the program. In Section 6, I report the main results from the empirical analysis. Section 7 (In progress) and Section 8 (In progress) put the results in the perspective of the existing literature and estimate the welfare effects of the program based on the Marginal Value of Public Funds approach. Finally, Section 9 concludes and discusses the main policy implications.

2 Institutional Background: *PANES/AFAM-PE*

2.1 Context of Implementation

Uruguay is a middle-high-income country in South America with a population of about 3.5 million inhabitants. In 2018, Uruguay had the second largest GDP in the region (USD 23,585), only led by Chile (USD 25,526).[4] In the same year, Uruguay was ranked 55th in the world in terms of Human

[4]See Table A.1 in Appendix A.1 for further details.

Development Index and classified within the very high HDI group. In terms of education indicators, Uruguay's lower secondary completion rate in 2018 was 56.8%, which is comparable to Argentina's but lower than in Mexico, Brazil, and Chile; and way behind richer countries such as the United States, Sweden, or even Italy and Spain. In terms of teenage fertility, Uruguay is also well positioned within the region, with 58.24 births per 1,000 women aged 15-19. While this rate is very similar to the rates observed in Brazil and Argentina, it is slightly higher than in Chile and Costa Rica and substantially higher compared to the United States, Norway, Sweden, Spain, and Italy.

Uruguay has an old tradition of a strong public sector. For instance, in 2018, Uruguay's tax revenue as a percentage of the GDP was 29.2%, the largest in the region, only behind Brazil. Compared to the rest of the world, this share is higher than in the United States and close to the OECD average. In terms of its social protection system, Uruguay has one of the oldest and most developed systems in the region.[5] In 1943, Uruguay implemented family allowances for families with underage children for the first time. However, until the end of the 90s, these benefits were restricted to registered employees.

The program I focus on, *PANES/AFAM-PE*, was implemented in 2005. It was conceived as a temporary social relief program in response to the economic downturn that affected most Latin American countries in the early 2000s, and it remained in place until December, 2007.[6] In the next section, I describe in detail the key elements of its design.

2.2 Design of *PANES/AFAM-PE*

The implementation of *PANES/AFAM-PE* can be divided into two phases. The first one started in 2005 under the name of "*PANES*" and remained in place until the end of 2007. The second, *AFAM-PE*, started immediately after *PANES*. The program was widely publicized and rapidly became the largest anti-poverty program in the country's history (Manacorda et al., 2011). At a smaller scale, *PANES/AFAM-PE* is comparable both in its design and in its relative size to programs such as PROGRESA-Oportunidades (Mexico) and Bolsa Familia (Brazil). The total expenditure on the program has been consistently around 0.4% of the Uruguayan GDP.

The main component of *PANES* was a cash transfer targeted at the poorest 150,000 households in the country. The program had two primary goals. First, in the short run, it aimed to alleviate the high poverty levels caused by the economic crisis.[7] Second, in the medium- and long-run, its goal was to encourage human capital accumulation in poor households to help them move out of structural poverty. The base cash transfer was USD 133, expressed in January 2008 PPP terms.[8] In addition, the program provided a supplementary transfer between USD 29 and USD 78 to households with underage children (70% of the participant households). Overall, the cash transfer represented more than 50% of the average self-reported household income in the application form for the universe of

[5]For instance, old age pensions were established for the first time in 1919; maternity leave was implemented in 1937; sickness and disability insurance in 1950; and unemployment benefits in 1958.

[6]After the economic crisis of the early 2000s, unemployment and poverty sky-rocketed. By the end of 2004, the poverty rate for urban areas reached 40%, and the unemployment rate was close to 15%.

[7]In 2005, the country's poverty rate was close to 21%. However, the child poverty rate was even higher: 36.6% for all children in urban areas and 60% for children between 0-5 years old.

[8]In local currency, this corresponded to UYU 1,360. In what follows, all income variables are converted to 2008 PPP using CPI and PPP conversion factors.

application forms.[9]

Between 2005 and 2007, more than 180,000 different households (about 17.6% of all households in the country) applied to *PANES/AFAM-PE*. Eligibility was determined based on two criteria. First, applicant households must have a per-capita household income below USD 131 (or between 27.9% and 41.7% of the April 2005 poverty line). Second, households must have a poverty score below an arbitrarily defined threshold that varies by region. Households were visited by program officials who conducted a thorough interview to evaluate the socio-economic situation of the household. This information was used to compute the poverty score, which consists of the predicted probability of being below a critical per capita income level.[10] Households with a poverty score above a certain threshold are eligible to participate, while households with a score below the threshold are deemed ineligible. After being accepted, participant households were supposed to satisfy school attendance, regular health check-ups, and monthly per-capita income requirements, but the program did not rigorously enforce these conditions until April 2013.

On January 1st, 2008 *PANES* was expanded and re-branded into *AFAM-PE*. While formally, *AFAM-PE* was a new program that substituted the original *PANES*, in practice, it was implemented as an expansion with very slight differences. The program's main components - i.e., eligibility criteria and type of benefits and conditionalities - remained the same. There were only three differences between *PANES* and *AFAM-PE*. The first one is that *AFAM-PE* established the presence of underage children in the household as a requirement for eligibility. The second is a more lenient poverty score eligibility threshold. This change aimed to increase the coverage of the program. Finally, the program changed the formula used to define the amount to be transferred. The new structure established a baseline payment of USD 57 per child from 0-17 but was subject to an equivalence scale of 0.6. In addition to the base payment, each household would receive an additional USD 24 per child enrolled in the secondary education system, also subject to an equivalence scale of 0.6. Finally, it is important to note that *AFAM-PE* beneficiaries were also supposed to fulfill education and health check-up conditionalities. However, these started to be enforced in April 2013. In subsequent years actual strength of enforcement mechanisms depended on the will of the Ministry of Social Development and other high-ranked officials.

The transition between the two phases for participant households was straightforward. Provided that families had underage children, *PANES* participants were automatically enrolled in *AFAM-PE*. Furthermore, households rejected during the first phase were automatically enrolled in the second phase if they satisfied the new eligibility requirements. Figure 1 presents a summary of the main components of *PANES/AFAM-PE*.

[9] See Appendix **??** for a more detailed description of the characteristics of the universe of application forms. It is important to note that the income used as a reference to calculate this share is self-reported. However, since the program also had an income threshold rule to define eligibility, households may have misreported income to become eligible. Therefore this share must be interpreted as an upper bound. As an alternative reference, in April 2005, the household *per capita* poverty line was USD 314.19 for rural areas and USD 469.95 for urban areas in 2008 PPP terms.

[10] The variables used to calculate the score included the overall quality of the building, the number of people living in the household, the number of rooms, the presence of underage children, average years of education, and type of employment, among others. More details about how the poverty score was computed can be found in Appendix **??** and in Manacorda et al., 2011; Amarante et al., 2016.

3 Conceptual Framework: Cash Transfers and Decisions Within the Household

Cash transfers may cause behavioral responses in several margins across all household members. In its simplest form (i.e., unconditional), cash transfers induce a pure income effect that leads households to increase their demand for normal goods (e.g., consumption goods or leisure). However, cash transfers contingent on certain behaviors (i.e., conditional) are associated with a more complex set of potential behavioral responses. These may trigger reactions associated with a substitution effect due to changes in the relative opportunity costs of the alternatives included in individuals' choice sets. The analysis becomes even more convoluted when decisions are allowed to interact inter-temporarily or when market imperfections such as information frictions or collective household models are considered. In this section, I motivate the research hypotheses by broadly discussing how *PANES/AFAM-PE*, or any other similar CCT program, may affect education, labor market, and fertility decisions of individuals who benefited from the program in their childhood.[11] The list of mechanisms discussed in this section is not intended to be exhaustive. The goal is to provide an overview of what the literature has proposed and discussed when analyzing the effects of CCTs on education, fertility, and labor market decisions.

Income and substitution effects: Consider a simple unitary model where households decide over leisure, school, fertility, and labor market activities. In this setting, CCTs could imply both income and substitution effects. The income effect, associated with additional household resources, would increase the quantity demanded of normal goods (e.g., leisure) to the detriment of labor market activities for all household members. Furthermore, if households obtain direct utility from children's current human capital or schooling (e.g., Todd and Wolpin 2006, 2008; Keane and Wolpin 2010), the income effect could also lead to an increased demand for children's education.[12] Income effects are crucial for poor households in the presence of credit constraints. In such settings, families might decide not to send their children to school because they cannot afford it. The cash transfer would work as a mechanism that relaxes those contrainsts, allowing beneficiary households to increase their expenditure on school-related goods and services. This would enable children to enroll and remain at school (e.g., books, clothing, transportation costs, etc.). A similar mechanism could also explain changes in fertility decisions if, for instance, there is a direct dis-utility associated with early life childbearing, and household members cannot buy contraceptives in the absence of cash transfers.[13]

[11] Developing a theoretical model that contemplates all these potential interactions is beyond the scope of this paper. However, it is important to mention that Keane and Wolpin (2010), for instance, formalize a similar decision process, focusing exclusively on women's decisions. More specifically, they estimate a structural model in which women's choice set is comprised of work, marriage, schooling, fertility, and welfare participation. A very simple but illustrative example of the complexity of this setting is that women make between 18 and 36 mutually exclusive choices in each period, depending on their fecundity stage.

[12] It is beyond the scope of this paper to discuss the non-pecuniary benefits of schooling or if it should be considered a (normal) consumption good. Oreopoulos (2011) and MacLeod and Urquiola (2019) provide in-depth reviews about the status of this discussion in the literature.

[13] One alternative way in which the cash transfer can affect fertility rates of young women through an income effect is when their labor market activities are associated with transactional sex activities (see Baird and Özler 2016; LoPiccalo et al. 2016 for a review of the relation between income and transactional sex)

CCTs can also affect household decisions through a substitution effect since they make participation contingent on specific behaviors, typically school enrollment, attendance, and health check-ups. Education requirements reduce the opportunity cost of schooling, and make it more attractive compared to any other non-education-related activity such as labor market participation or becoming a parent (e.g., Parker and Todd, 2017). A substitution effect could also affect children's education enrollment through parents' time allocation if children's engagement with the education system depends, at least partly, on the time they spend together (e.g., in the spirit of Martinelli and Parker, 2008). In this case, the reduction in parents' time allocated to labor market activities through the substitution effect would free time that could be re-directed toward time spent with children. A reduction in parents' time allocated to labor market activities also increases children's share of supervised time. This reduces the possibility of engaging in risky behaviors that could lead to early-life pregnancies. In sum, both income and substitution effects are expected to reduce children's labor market participation, increase children's education enrollment and reduce young women's fertility when they receive the cash transfer.

Dynamic effects: The effects discussed so far correspond to a static model. When individuals make decisions that have consequences for multiple periods, the set of potential behavioral responses becomes broader and even more complex. One example is what Black et al. (2008) refers to as the "future human capital effect". Consider the income and substitution effects discussed in the previous paragraph as effects in the "current" or today's time. The reduction in the marginal cost of schooling increases current investment in education. However, additional education today also increases the opportunity cost of education tomorrow. The more schooling children accumulate, the higher the wage offers they receive. If there are diminishing marginal returns to schooling, there would be a point where the marginal cost of an additional year of schooling will be larger than the marginal benefit. This would lead some individuals to choose labor market participation instead of more schooling (Behrman et al., 2011).

A similar reasoning can be applied to fertility decisions. There is a strong link between expected future labor market income and fertility decisions. Models that aim to characterize early fertility decisions propose that young women compare the lifetime expected utility of having vs. not having a teen birth (e.g., Duncan and Hoffman, 1990; Wolfe et al., 2001). Because they reduce the marginal cost of schooling, CCTs increase the expected utility of delaying fertility through higher expected adult wages. This leads to more women deciding to delay fertility. While delaying fertility might seem relatively costless in the short run, it is also reasonable to expect these costs to increase in the long run, for instance, due to a reduced probability of having a successful healthy pregnancy. Hence, at some point, even when the opportunity cost of having a child is large due to high wages, the marginal cost associated with keep delaying childbearing might be sufficiently high to more than offset the potential gains in earnings. Under such circumstances, it is reasonable to expect that initial negative effects on fertility might start to fade or even reverse in the long run. In this case, one should be cautious in how the early negative effects are interpreted. These could be more associated with delays rather than actual changes in fertility preferences. However, potential effects of CCTs on overall preferences for fertility cannot be ruled out ex-ante.

Another example of how current decisions might have strong implications on future choices stems from the models of skill formation in the presence of dynamic complementarities (Cunha and Heckman, 2007). In these models, today's investments in education increase returns of education in subsequent stages, which promote a more extended stay in the education system to the detriment of other activities, such as labor market participation and childbearing.

Reinforcing mechanisms: It is important to note that besides the direct effects of CCTs on education, fertility, and labor market decisions, these decisions might also have direct effects on each other. For instance, education could affect fertility decisions if there is a trade-off between quality and quantity of children (Becker and Lewis, 1973); if it improves current women's ability to predict better labor market outcomes associated with delaying childbearing (referred to as "current human capital effect" in Black et al., 2008); if it improves access to contraceptives and family planning and health care services which are critical determinants of fertility decisions (e.g., as in Kearney and Levine, 2009; Bailey, 2006; Lundberg and Plotnick, 1995); or by changing women empowerment, attitudes, and values toward maternity, just to name a few.[14,15] Fertility could also affect education decisions, for instance, through the effect of child care time on the marginal cost of school time (Klepinger et al., 1999). Similarly, education can affect labor market decisions by affecting children's perceptions about how the process of earning better wages works, the current sacrifices required for better future wages, by improving expectations about achievable goals, or by providing different role models, etc.

Other mechanisms: While in a friction-less model, conditionalities associated with cash transfers would cause efficiency losses, they usually aim to correct potential sub-optimal decisions due to market failures, such as information frictions, differences in discount rates, or intra-household bargaining problems (Parker and Todd, 2017; Baird et al., 2014). Hence, under more realistic circumstances, CCTs may also affect households' decisions through mechanisms other than the standard income and substitution effects. For instance, CCTs are usually entitled to the mother of the eligible children. Moving from a unitary to a collective household decision model (e.g., Chiappori, 1988, 1992; Browning and Chiappori, 1998) opens the door for CCTs to change household members' bargaining power (Martinelli and Parker, 2003, 2008; Attanasio et al., 2012), which could re-direct part of the household expenditure toward goods and services that are more favorable to children (e.g. Thomas, 1990; Duflo, 2003, or more specifically about *PANES/AFAM-PE* Bergolo and Galván, 2018).

The information environment and expectations about returns to education are also key determinants of current education decisions (Jensen, 2010). For instance, by participating in a CCT, parents are more exposed to highly educated professionals, which could change their expectations about the opportunities for their children and the investment required to reach them. Parents' improved expecta-

[14]Related literature (e.g., Black et al., 2008) also defines an "incarceration effect" of education on fertility, i.e., more time spent at school reduces the time available to engage in risky behavior. While this mechanism is plausible, in this discussion, it is captured by the idea that education and fertility are mutually exclusive or highly substitutes

[15]Alternatively, attending school might also increase the social interactions of young girls with other potential sex partners that they meet at school or in related environments. However, for this to have an effect, the new interactions should more than offset the existing interactions outside the school that are lost due to the increased time at education institutions.

tions can also be transmitted to their children. This would lead to higher enrollment and permanence in the education system (Attanasio and Kaufmann, 2014; Chiapa et al., 2012). On the contrary, children's expected returns to education can be negatively affected by the CCT if parents substantially increase their time allocated to leisure activities because of the income effect. Perceptions about expected future outcomes are also highly relevant for fertility decisions. Kearney and Levine (2014) propose a model where fertility decisions are determined by the perceived probability of achieving a high utility state, which is only feasible if women delay childbearing. Perceptions of the likelihood of success are a function of current socio-economic status and inequality. Hence, CCTs may also affect fertility decisions by changing the current socio-economic situation of poor women or, more generally, by reducing inequality in their society.

Finally, alternative mechanisms such as a reduction in household economic stress that could create a better environment for child development (Gershoff et al., 2007; Yeung et al., 2002; Conger et al., 1993); or improved children's health outcomes due to better parental socioeconomic conditions (e.g., Currie, 2009); or social interactions and peer effects (e.g. Bobonis and Finan, 2009; Lalive and Cattaneo, 2009) might also affect education, fertility, labor market participation decisions.

In sum, the related theoretical literature provides mostly unambiguous predictions about the short-run effects of CCTs on education, fertility, and labor market decisions for individuals that benefited from a CCT program when they were young. More specifically, CCTs are expected to reduce teenage pregnancies, increase education enrollment, and reduce children's labor market participation. However, in a dynamic setting, the expected effects are ambiguous and depend on individual preferences and institutional characteristics. The fact that these effects can interact in complex and theoretically ambiguous ways illustrates the need for a dynamic analysis to understand how CCTs affect the current and future lives of the beneficiaries and the mechanisms involved. In the end, the effects of CCTs on the trajectories that mark children's transition to adulthood and early adulthood outcomes is mostly an empirical matter.

4 Data Sources, Measurement, and Sample of Interest

The analysis of the effects of CCTs on the transition to adulthood is highly data demanding for two reasons. First, it requires information on a large number of individual characteristics. Because adulthood is defined not just by one but by a series of markers in different life spheres - including education, fertility, and labor market markers among the most important ones - the transition to it also needs to be characterized in terms of such dimensions. Second, because transitions are a dynamic phenomenon by nature, its analysis requires longitudinal information that allows for a complete description of the individual trajectories. Having both is extremely difficult and costly.

The data used in this paper satisfy these two requirements. First, the empirical analysis is based on an exhaustive compilation of administrative records from different sources for the universe of applicants to *PANES/AFAM-PE*. These can be linked at the individual level and contain information about fertility, education, and labor market outcomes. Second, because the data is based on administrative

records, all of these variables are observed for a long span of years and for the universe of interest. In the next section, I explain in detail the main features of the dataset assembled for the analysis, as well as the key outcome variables.

4.1 Data Sources and Measurement

***PANES/AFAM-PE* records: Application and participation variables.** *PANES/AFAM-PE* records are used to measure all the application- and participation-related variables, which are mostly used as treatment or control variables. These records were provided by the Ministry of Social Development, which is the Ministry in charge of implementing the program, and contain information about the universe of successful and unsuccessful applications to *PANES/AFAM-PE* between April 2005 and December 2017 at the form, household, and individual level. The information at the form level includes city, date of application, poverty score, resolution, and in case of acceptance, the participation history. Information at the household level includes the house's building materials, structure, appliances, and access to public services, among other information used to compute the poverty score. Individual level information contains the baseline information about education, employment status, income, date of birth, and gender, for each household member reported in the application form before participating in the program. The total number of application forms included in the raw participation data is 747,204, corresponding to 1,476,696 unique individuals.[16]

Birth Records: Fertility outcomes I use birth records to measure the fertility outcomes reported throughout the paper. These were provided by the Ministry of Health and consist of an individual-level dataset that includes the universe of births in Uruguay between 2005 and 2019. Birth records contain information such as birth date, type of institution where the child was born (public, private, or others), the mother's age, birth weight, and gestation weeks. In addition, they also include identification information of the mother, which allows me to link this information with *PANES/AFAM-PE* participation records at the individual level.

Concerning fertility outcomes, it is important to note that these variables are defined exclusively for women due to the typical limitations in the information reported on birth certificates about newborns' fathers. As for every outcome variable described hereon, I define two types of fertility outcomes. First, I define a binary variable that indicates whether a woman has given birth *at or before* a certain age. Second, I define a continuous variable that reports the number of births that a woman has had by a given age. The binary variable is associated with extensive margin responses, i.e., it will capture the effect of *PANES/AFAM-PE* on giving birth versus not giving birth. The continuous variable will also capture responses in the intensive margin. Because I am interested in depicting the dynamics of the effects, each of these variables will be defined using every age between 15 and 30 years old. This allows me to provide a full description of whether and how the effects of *PANES/AFAM-PE* materialize throughout the usual years in which individuals transition to adulthood. All the outcome variables are defined exclusively based on the post-application period, i.e., they correspond to events that happened any time after the household applied to the program, to ensure that the estimated ef-

[16] Appendix B contains a more detailed description of the participation data.

fects correspond to the actual effects of the program. In the specific case of fertility outcomes, I define the post-treatment period as starting seven months after the application date. As a robustness test, I will also report estimates based on a binary variable defined *at* a given age, as opposed to *at or before*.[17]

Secondary and tertiary education administrative records: Education outcomes. I use the secondary and tertiary education administrative records to measure the by-age education enrollment outcomes. These records come from three different public institutions: 1) National Council of Secondary Education, 2) National Council of Technical and Professional Education, and 3) *Universidad de la Republica*, which is the largest public university in the country. Education records are structured at the individual level and can be linked to *PANES/AFAM-PE* records at the same level. Information from the National Council of Education corresponds to the traditional public secondary education system, which is analogous to grades 1-6 of middle and high-school education in the United States. These records contain yearly information for the universe of students enrolled in secondary public schools in 2006-2012, 2014, 2017, and 2018. National Council of Technical and Professional Education records contain information on vocational and technical public school enrollment for the same period. The programs offered by technical and vocational schools can be categorized into middle school, high school, and tertiary, based on their enrollment requirements.[18] Finally, the information provided by *Universidad de la Republica* consists of individual-level information that identifies the year of enrollment of every student enrolled at the University between 2005 and 2020.[19]

To analyze the effects of *PANES/AFAM-PE* on education decisions, I focus specifically on enrollment outcomes. These variables are the most complete and reliable among the ones included in the administrative records. They are also easily comparable across levels and institutions.[20] Analogous to fertility outcomes, I define education outcomes using binary and continuous variables. The binary variables indicate whether an individual was enrolled at any secondary education institution, either traditional or vocational, at or before a given age. On the other hand, the continuous variable measures the total years in which an individual was enrolled in the corresponding education system. Both variables are computed for every age in the 12-30 range. However, after age 20, these are most likely very uninformative, and the focus probably should be on tertiary education enrollment. I also report robustness tests using variables that indicate whether individuals were enrolled at a secondary level program *at* a specific age.

[17] Appendix C provides summary statistics for each of the outcome variables based on the samples that are defined in Section 4.2

[18] For instance, a middle-school-analogous vocational education program is a program that requires individuals to have completed primary school. A high-school-analogous vocational educational program is a program that requires to have completed middle school and so on.

[19] While I do not have access to education enrollment information in private institutions for any of the education systems or levels, it is important to note that the (free) public education system is probably the relevant choice set of schools for the population of interest, given that private institutions usually offer a limited number of grants and have relatively expensive tuition. For instance Ramírez Leira (2021) shows that the probability of enrolling in a public institution for individuals in the first quintile of the income distribution is larger than 95% in 2017

[20] It is important to note that students who promoted the current grade are automatically enrolled for the next academic year. Hence, enrollment variables do not necessarily represent an explicit decision to sign up for the current academic year. Moreover, to some extent, individual enrollment for a given grade could be interpreted as a signal of academic progress.

Enrollment in tertiary education works differently than enrollment at secondary education institutions. More specifically, while individuals need to enroll at the University the first time they decide to take classes, once registered, they are not required to re-enroll periodically in case they want to continue or retake their studies. For this reason, the information provided by *Universidad de la Republica* only allows me to define a binary variable that indicates if the individual has ever been enrolled at the university or any other tertiary level course at the vocational/technical institutions, by a certain age.[21].

Social Security Agency (SSA) labor histories: Labor market outcomes. I use SSA labor histories to construct the labor market outcomes. Labor market histories contain monthly individual-level information about each job position for the universe of registered employees between 2005-2021. These records have information on wage, hours worked, activity type, and employers' industry sector. SSA labor histories can also be linked to *PANES/AFAM-PE* records at the individual level. The main limitation of using the SSA labor histories as the only data source for analyzing labor market outcomes is that it is restricted to formal employment and does not capture any potential labor market insertion in the informal sector. However, it is important to note that the informal labor sector in Uruguay is currently relatively small compared to other non-high-income countries, and it only represents 17% of total employment. In any case, to interpret the results reported in this paper, one should keep in mind that someone that does not show up in SSA labor histories can be either unemployed or employed in the informal sector. Therefore, the labor market effects reported in this paper must be interpreted exclusively as concerning the formal employment sector.

To analyze the effect of *PANES/AFAM-PE* on labor market outcomes, the baseline binary labor market outcome indicates whether someone has had a registered employment spell that lasted at least four consecutive months at or before a given age. Using spells of four consecutive months rules out potential temporary employment, such as Summer jobs, and reflects more stable links to the labor market. In the case of labor market outcomes, I define two complementary continuous variables. First, the total number of months an individual has worked in the formal sector by a certain age. Second, a similar variable that measures cumulative earnings, i.e., the sum of all labor income earned by a given age. These three variables are calculated for ages between 14 and 30 years old. As in the other dimensions, I report robustness tests using the same variables defined *at* a given age, instead of *at or before*.

4.2 Sample of Interest: Definition

Since this paper focuses on the effects of *PANES/AFAM-PE* on the transition to adulthood for children that benefited from the program during their childhood, the empirical analysis is restricted to individuals of households that applied to the program when they were eighteen years old or younger and had at least fifteen years old by April 2018. The latter restriction ensures that every individual included in the analysis has had the chance to enroll in high secondary education for at least one year. In addition, for simplicity, the analysis is always restricted to individuals of households who applied

[21]Because this could limit the scope of the results, I have already asked access to additional information that allows a more precise measure of whether an individual is actually an active part of the tertiary education system

for the first time between 2005-2012. These represent 95% of this sample.

Describing the dynamics of the effects using "real-time" data presents some additional challenges and trade-offs that are important to consider when defining the sample of interest, especially for a permanent and relatively young program such as *PANES/AFAM-PE*. In particular, when an outcome is last observed, individuals have all different ages.[22] This implies that the samples used to estimate the effects of the program at different ages will be different. For instance, someone 25 years old should not be included in the analysis of an outcome measured at age 30. While using different samples that vary depending on the age at which an outcome is measured maximizes the use of the information available, it also makes comparisons harder. One way to improve the comparability across results is to use a balanced sample where all individuals have already turned a certain age. This allows measuring the program's effects on different outcomes measured at different ages while keeping the sample composition constant. However, this implies renouncing a substantial share of the sample since *PANES/AFAM-PE* is still a relatively new program, and not many individuals applied with less than 18 years old and have at least 30 years old by December 2019. Since there is not one alternative that strictly dominates the other, the empirical analyses will report results both for balanced and unbalanced samples.

Hence, for the empirical analysis, I define three different samples. First, the *main sample*, which is comprised of individuals who were at least 23 years old by December 31, 2019. This sample is used to compute a set of baseline estimates, as will be explained in detail in Section 6. Having a balanced sample defined at age 23 ensures comparability across a relatively extended period while maintaining most of the observations in the universe of analysis. Second, I define a *restricted sample*, which is a subset of the *main sample* but restricted to individuals who were at least 30 years old by December 31, 2019. The *restricted sample* is used jointly with the *main sample* to present the set of baseline estimates. Finally, the dynamic analysis is based on different *extended samples* whose composition depends on the specific age at which an outcome is measured. For instance, estimates of the effect of the program on fertility by age 27 are based on the sample of individuals that had at least 27 years old in December 2019, and so on.

4.3 Sample of Interest: Description

Table 1 describes the main characteristics for the *main* (columns 1 and 2) and the *restricted* (columns 3 and 4) samples.[23] Odd columns include all individuals in each of the samples. Even columns are restricted to individuals with an application score within the optimal RDD bandwidth chosen for the baseline estimates. The procedure used to select the optimal bandwidth is explained in detail in Section 5.[24]

Panel a. focuses on individual characteristics. There are 224,413 individuals in the *main sample*

[22] Appendix C reports the distribution of ages at December 31, 2019. This corresponds to the last day available in birth records.

[23] Appendix C report more detailed descriptive statistics at the form level, including information about the 747,204 application forms filled between 2005 and 2017

[24] For exposition purposes, to describe the samples used in the analysis, I selected the largest optimal bandwidth among the estimates that use each specific sample.

who are equally split between men and women, were on average 26.9 years old by December 31, 2019, and belonged to about 1.8 households. Individuals are typically included in 2.6 application forms. The average age at first application is 13.4 years old. About 84.2% of these individuals were accepted to *PANES/AFAM-PE* at least once before age 18.[25] 78.4% of the individuals show up in at least one application form to the *PANES* phase. At the same time, 96.1% are included in at least one *AFAM-PE* form. Panel b. describes the characteristics of the application forms for these individuals. Because they may be included in multiple application forms, this table reports the characteristics of the earliest application form filled.[26,27] The average centered poverty score is 0.18. This means that the average application corresponds to an eligible form. Consistently, the share of individuals whose first application form was accepted is 71.8%. The first application form for most individuals (78.4%) was filled during the *PANES* phase, and 31% of the applications corresponded to individuals of households in the capital city (Montevideo). This means that individuals from the capital city are under-represented in this sample since about half of Uruguay's population lives in Montevideo. Finally, panel c. describes the household characteristics. Individuals in the sample belong to households that are comprised, on average, of 4.9 individuals, of whom 2.9 are underage children. Slightly less than half of the households correspond to single-parent households, and the average age of household members (including children) is 23.1. Household heads have, on average low education levels, i.e., about seven years, slightly more than the equivalent of completed primary school. 63.4% of them are employed, and their average income is USD 144.33, which is comparable in size to the cash transfer value, as described in Section 2.

The sub-group of individuals that belong to the *main sample* and have a poverty score within the optimal bandwidth has very similar characteristics to the full *main sample* in terms of variables that are not related to the poverty score. These are different by construction. Besides these variables, the exception is on the share of individuals from the capital city, which is smaller than in the full *main sample*. The *restricted sample* also closely resembles the *main sample* except for age-related variables, which are mechanically different. Individuals in the *restricted sample* are, on average, five years older than in the main sample (31.1 compared to 26.9), and were also older when they applied to the program for the first time (16.8 years old compared to 13.4). Because of the age restriction used to define this *restricted sample*, the first application forms for these individuals corresponded exclusively to *PANES* applications. This also implies that they had a less potential time of exposure before turning 18 years old and translates into a lower share individuals ever accepted to *PANES/AFAM-PE* (71.0% compared to 84.2%). Except for the mechanical differences in variables related to the definition of each sample, the subgroup of individuals that comprise the *restricted sample* is very similar to the full *main sample*.

[25] Appendix C provides the full distribution of age at first application and age at first acceptance for both *main* and *restricted* samples.

[26] A more detailed discussion of the reasons for selecting this application form is provided in Section 5, since this is also a critical decision for the empirical design

[27] Appendix C compares the characteristics of *all* application forms versus *first* application forms.

5 Empirical Strategy

As described in Section 2, eligibility to participate in *PANES/AFAM-PE* is based on a poverty score. More specifically, let z be the poverty score centered around the eligibility threshold and D an indicator variable such that positive values indicate eligibility (i.e., $D = 1$) and negative values indicate ineligibility (i.e., $D = 0$). The use of an arbitrary threshold to define whether a household is eligible to participate in *PANES/AFAM-PE* provides a quasi-random source of exogenous variation to identify the causal effects of the program using a Regression Discontinuity Design (RDD) (Thistlethwaite and Campbell, 1960). Intuitively, under perfect compliance and a continuity assumption, (local) average treatment effects of the program can be obtained by comparing the regression functions of the outcome of interest at both sides of the threshold (Hahn et al., 2001).[28]

To illustrate how the *PANES/AFAM-PE* eligibility rule works, Figure 2 describes the relation between a variable that indicates if an application was successful (*y-axis*) and the centered poverty score (*x-axis*). Each observation used to create this figure corresponds to an application form. Panel a. depicts this relation for the full support of the running variable. Each bin in the figure represents the average acceptance rate within the bin.[29] In the background, vertical bars represent the distribution of the poverty score. Figure 2 shows a pronounced change in the acceptance rate just at 0, i.e., at the eligibility threshold. From the program administrator's perspective, the eligibility rule was applied correctly, although not perfectly. Different reasons can explain the fuzziness observed on both sides of the threshold. For instance, to the left of 0, it could be due to applications below the eligibility threshold that were rejected when filed but were automatically enrolled after the threshold became more lenient.[30] On the right-hand side, it could be due to rejections based on reasons other than the poverty score, such as income or no qualifying underage children. Panel b. focuses on the observations that are close to the threshold and will be used in the actual RDD estimates.[31] In addition, the figure also reports the magnitude of the change (0.600). This is calculated as the difference between the average acceptance rate at the right of 0 (0.813) and the average acceptance rate at the left of 0 (0.213). This difference is statistically significant ($p - value < 0.001$). Overall, despite a slight degree of "non-compliance," resolutions about the application were almost entirely determined by the poverty score, which supports using an RDD to estimate the causal effects of the program.

Under imperfect compliance, comparing observations to both sides of the threshold provides

[28] Formally, let Y be any of the outcomes of interest. Under perfect compliance, the key identification assumption in RDD is that Y is continuous at $z = 0$ if the regression functions for the outcome variable - $\mathbb{E}[Y(1)|Z = z]$ and $\mathbb{E}[Y(0)|Z = z]$ - are continuous functions at $z = 0$, then: $\mathbb{E}[Y(1) - Y(0)|Z = z] = \lim_{z \downarrow 0} \mathbb{E}[Y|Z = z] - \lim_{z \uparrow 0} \mathbb{E}[Y|Z = z]$.

[29] To the left of 0, observations are binned in ten quantile-spaced bins. To the right of 0, observations are binned in fifty quantile-spaced bins. The relation in the number of bins used at each side of 0 is based on the relative number of observations between the two sides.

[30] Unfortunately, the administrative records do not identify these cases, and date of application corresponds to the day on which the application was submitted.

[31] The procedure used to select this optimal neighborhood is based on the data-driven approach proposed by Calonico et al. (2019) that optimizes the Mean Squared Error (MSERD) and will be explained in detail before the end of this section. Unlike panel a., which aims to describe the relationship between the application score and the resolution for the full support in a general way, panel b. provides specific formal evidence about the discontinuity in the probability of acceptance at the threshold. Hence, panel b. uses equally spaced bins chosen optimally using an Integrated Mean Squared Error (IMSE) approach (Calonico et al., 2015).

estimates that should be interpreted as intention to treat estimates. In such settings, a Fuzzy RDD, i.e., a design where eligibility is used as an instrument for the endogenous treatment variable, can recover the (local) average treatment effect under a monotonicity or "no-defiers" assumption (Imbens and Lemieux, 2008; Cattaneo et al., 2019). In the *PANES/AFAM-PE* setting, monotonicity implies that an application form with a score z that is rejected when the threshold is set at 0 would also be dismissed for any alternative threshold greater than 0. Conversely, any application form with a score z that is accepted when the cutoff is 0, would also be accepted if the cutoff is $\tilde{z} < 0$.

An additional layer of complexity in this research design is that *PANES/AFAM-PE* has been in place uninterruptedly since 2005, and households might have applied to the program multiple times. This introduces two concerns about key elements of an RDD. The first one corresponds to the definition of the instrument and the treatment variables. In a context where individuals have multiple scores and resolutions, one needs to decide which one to use. The second is how to address the possibility of endogenous sorting around the eligibility threshold induced by re-applications. Re-applications could be a source of endogenous sorting that could threaten the validity of the RDD if re-applicant households that were originally rejected and are close to the threshold are different from non-re-applicant households and if these differences are correlated to the outcomes of interest. In this case, the RDD estimates will be biased.[32] To address these concerns, I follow the approach proposed by Jepsen et al. (2016), who suggest using eligibility based on the first application's score as the instrument for treatment. The rationale that supports this approach is that the first score is presumably the score that is less subject to manipulation.[33] Hence, I define eligibility and participation as follows.

Exogenous variable: eligibility based on the score of the first application form (D^{1st}): I define the first application form (or reference form) as the earliest application form by any of the households that an individual has ever belonged to, as long as the individual had not left the household by the time of application. By going as far back as possible when defining the value of the running variable, I am taking a conservative approach to minimize any possible concern about endogenous sorting.[34] Hence, eligibility based on the first application is a binary variable that takes the value of 1 if the score obtained in the first application corresponds to an eligible form and 0 otherwise.

Endogenous variables: participation in *PANES/AFAM-PE* (T): The baseline treatment variable (T) is a binary variable that indicates whether an individual was ever accepted to *PANES/AFAM-PE* before turning eighteen years old. In addition, I define analogous variables for ages twelve through

[32] Endogenous sorting in settings with multiple application is also an issue in different contexts such as close elections (e.g. Cellini et al., 2010), analysis of returns to education using test scores (e.g. Clark and Martorell, 2014); or evaluation of the effects of remedial education(e.g., Martorell and McFarlin, 2011).

[33] Jepsen et al. (2016) analyze the effects of GED scores on employment and earnings. In this setting, the discontinuity exploited is the passing grade of the exam, and concerns about endogenous sorting arise because students can take the exam multiple times. The issue for identification is that re-takers can be different from non-re-takers in ways that are also correlated with the outcome of interest. If this is the case, using the final score obtained in the GED exam will not provide an adequate source of identification for the effects of the GED.

[34] For instance, household h_1 applied to *PANES/AFAM-PE* with forms $f_{h_1,A}$ and $f_{h_1,B}$. Individual i was born in h_1 after $f_{h_1,A}$ was filed, but before $f_{h_1,B}$ was filed. In this case, $f_{h_1,A}$ is still the reference form for individual i, even when she was not included in $f_{h_1,A}$.

seventeen that will be used in estimates where the outcome is measured before 18 years old. Since this definition does not account for the intensity of the exposure to the program, I present estimates based on two complementary continuous treatment variables as a robustness test. First, the number of months that an individual was treated. Second, the net present value of the cash transfer collected by the household before they turn 18.

In sum, the analysis of the causal effects of *PANES/AFAM-PE* on the different outcomes of interest is based on the following specification:

$$Y_i = \mu + \tau T_i + \beta_1 Z_i^{1st} + \beta_2 Z_i^{1st} T_i + u_i \tag{1}$$

where Y_i is the outcome of interest for individual i, (Z_i^{1st}) is the score obtained in the first application, and T_i corresponds to i's treatment status. Because T_i and Y_i are endogenous, T_i is instrumented using D_i^{1st} based on the folowing first-stage equation:

$$T_i = \alpha + \delta D_i^{1st} + \gamma_1 Z_i^{1st} + \gamma_2 Z_i^{1st} D_i^{1st} \epsilon_i \tag{2}$$

Following Imbens and Lemieux (2008) and Calonico et al. (2014), I estimate this model using local linear regressions fitted separately to each side of the threshold with observations that are sufficiently close to it. The estimation procedure follows Calonico et al. (2014), who provide robust standard errors and confidence intervals. The threshold is defined optimally following the data-driven approach by Calonico et al. (2019) and the default options: selection of bandwidth by optimization of Mean Squared Error (MSERD) and a triangular kernel function that puts more weight on observations that are close to the threshold. To assess the robustness of the results to these arbitrary choices, for each baseline outcome, I present specification curves based on all possible combinations of options. In all cases, standard errors are clustered at the household level.

6 Results

In this section, I present the main results of the empirical analysis. First, I report the first stage results, the manipulation, and the balance tests used typically in RDD. Second, I report the reduced form estimates (ITT) for a set of baseline results measured at three different ages: 18, 23, and 30. The choice of these specific age cutoffs is not random. 18 and 23 are chosen to coincide with the age individuals are supposed to have when finishing the final year of secondary education and college, respectively, if they are on track. 30 is the oldest age at which there are enough individuals in the sample to implement an RDD. Estimates for ages 18 and 23 are based on the *main sample* while estimates for age 30 is based on the *restricted sample*. Third, I report the analogous LATE estimates, focusing on heterogeneity by sex. Finally, I report the full dynamic analysis measuring the effects of *PANES/AFAM-PE* at all possible ages allowed by the *extended sample* structure, as described in Section 4.2.

6.1 Validity of the RDD Design

In this section, I report evidence that supports the use of an RDD to analyze the causal effects of *PANES/AFAMPE*. First, Figure 3 depicts the relation between the score obtained by an individual in her first application form (Z^{1st}) - measured in the *x-axis* - and *PANES/AFAM-PE* participation before eighteen years old (T) - measured in the *y-axis*. Panel a. reports the relation for the full support of Z^{1st}, while panel b. zooms in into the optimal bandwidth, chosen as explained in Section 5. In all cases, as in Figure 2, each circle represents the average value of the treatment variable within the bin. [35] However, since now the focus is on individual participation rather than the program officials' perspective, the unit of analysis is an individual and not an application form.

Overall, Figure 3 shows persuasive evidence of an abrupt discontinuity in the probability of ever being accepted into the program before turning eighteen years old, just at the eligibility threshold. This probability changes by 50% (29.3p.p.), and the difference between both sides of the threshold is statistically significant at traditional levels ($p - value \leq 0.001$). Table 2 presents the analogous regression estimates. Column (1) reports the baseline estimates using a linear polynomial function and a triangular kernel function, while columns (2) through (4) present sensitivity tests based on alternative polynomial degrees and kernel functions.[36]

The main threat to identification in the context of RDD analyses is that there might be some individual characteristics, other than the treatment status, that also present discontinuities at the threshold. If these characteristics are also correlated with the outcome of interest, the RDD estimates would be biased. Next, I present several pieces of evidence that indirectly test the validity of this assumption. First, I present the results of a continuity test for the running variable in the spirit of McCrary (2008).[37] Figure 4 presents robust evidence that rules out manipulation in the running variable. First, panel a. shows the raw histogram of the poverty score obtained in the first application form for all individuals in the *main sample*. The figure suggests that the distribution of the poverty score is smooth around the threshold, which is consistent with the null hypothesis of continuity or no manipulation. Panel b. implements a formal test based on Cattaneo et al. (2018) who extend the approach proposed initially by McCrary (2008) but use an optimal data-driven procedure to select critical parameters for the estimation of density functions, reducing the researcher's margin of arbitrariness. The reported evidence illustrates that the running variable's distribution is smooth around the threshold. Hence, it is consistent with preliminary evidence reported in panel a. and suggests that there is no evidence to reject the null hypothesis of continuity ($p - value = 0.715$).

Finally, Table 3 reports the RDD analogous to a balance table. Table 3 is comprised of a series of falsification tests that replicate the strategy used to estimate the baseline model but using

[35] The number of bins is chosen based on the same criteria used for Figure 2.

[36] Appendix D reports additional tests about the first stage estimates. First, I report two alternative treatment variables: 1) months treated and 2) the net present value of the total cash transfer amount collected by the household. Second, I report the first stage estimates for the *restricted sample*. Finally, I report a series of falsification tests, both on the *main* and *restricted samples*. These illustrate that the discontinuity observed in the treatment variable at the eligibility threshold is not observed in any other arbitrarily defined placebo threshold.

[37] The intuition behind this test is that a discontinuity in the distribution of Z^{1st} just at the eligibility threshold could indicate that some individuals can manipulate the running variable such that they force their way toward the eligible side. If this is true, and the ability to manipulate the score is correlated with the outcome of interest, the RDD estimates will be biased.

different baseline covariates instead of the outcomes of interest as the dependent variable. Testing for continuity of baseline variables is essential for variables strongly correlated with the outcome of interest. As expected, Table 3 shows that baseline variables are continuous at the threshold. When p-values are adjusted by the expected false discovery rate (Anderson, 2008), there are only three variables out of fifteen that are borderline statistically significant ($p - value \sim 0.08$); and in every case, the differences are economically irrelevant. For instance, the average age of household members for eligible individuals in the *main sample* is 0.32 years higher compared to ineligible individuals. Eligible individuals also live in households with, on average, 0.07 fewer members and are also 0.15 years younger by December 2019. Furthermore, when the falsification test is conducted on a variable that predicts the eligibility status based on all the other baseline covariates, there are no differences between both sides of the threshold. The p-value of the difference is 0.680 (q-value = 0.559). This indicates that observable characteristics are not systematically different at both sides of the threshold.

Overall, the tests reported in this section provide robust support for the validity of the identification strategy. However, out of caution, the empirical analysis will report several additional complementary tests to further prove their robustness.

6.2 Baseline Estimates: Effects Measured at 18, 23, and 30 Years Old

Intention to Treat Effects

In this section, I report the main intention to treat estimates. These are based on a reduced form specification of the model described by equations 1 and 2 in Section 5. In particular, estimates are based on the following specification:

$$Y_i = \tilde{\mu} + \tilde{\tau}\mathbb{1}(Z_i^{1st} > 0) + \tilde{\beta}_1 Z_i^{1st} + \tilde{\beta}_2 Z_i^{1st}\mathbb{1}(Z_i^{1st} > 0) + u_i \tag{3}$$

Equation 3 is the reduced form specification for equation 1, but using an indicator variable for eligibility ($\mathbb{1}(Z_i^{1st} > 0)$) instead of the treatment binary variable (T). In this case, the coefficient of interest is $\tilde{\tau}$, which represents the intention to treat effect of eligibility on the outcome variable. $\tilde{\tau}$ measures the difference in the intercepts of the two local linear regressions fitted separately to each side of the eligibility threshold within the optimal bandwidth.

As explained in Section 4.2, the baseline results reported in Table 4 show the intention to treat estimates for three specific age cutoffs: 18, 23, and 30 based on the *main sample* for the first two and the *restricted sample* for outcomes measured at age 30. In each case, I report estimates for the three dimensions of analysis: fertility, education, and labor market outcomes. Education estimates measured at age 18 correspond to secondary education measures, while when measured at ages 23 and 30 correspond to tertiary education. Traditional and vocational/technical school enrollment variables are pooled in both cases. Panel a. reports estimates based on binary variables, while panel b. reports similar estimates but for continuous or discrete outcome variables. For each ITT estimate reported in panel a. of Table 4, Figure 5 reports the corresponding graphical illustration. Because of the importance of visual representations in RDD analyses and to provide the most transparent representation possible, Table 4 and Figure 5 are both based on specifications without additional

covariates.

When measured at age 18, estimates based on equation 3 show that *PANES/AFAM-PE* has an ITT effect on the probability of giving birth of -3.1p.p.. This effect represents 13.5% of the fertility rates observed for women in the ineligible group within the optimal bandwidth and is statistically significant at traditional levels ($p - value = 0.005$). A similar effect is observed in the number of births. In this case, the estimated ITT effect is -0.038 births, which represents 14.9% of the mean of the outcome variable for ineligible women within the optimal bandwidth. It is also statistically significant ($p - value = 0.003$). This effect can be easily observed in panel a. in Figure 5, which illustrates the differences in the intercepts of the fitted local linear regressions. Unlike the strong effects observed for fertility outcomes, education and labor market participation seem unaffected by eligibility. In the case of labor market participation, this is true both for the binary and the continuous variable. In the case of secondary education, the coefficient is statistically insignificant for the binary enrollment variable and positive and statistically significant ($p - value = 0.029$) when the dependent variable is the number of years enrolled. In both cases, the visual evidence shows no signs of large discontinuities at the threshold for either of these two dimensions.

The ITT effects on fertility outcomes measured at age 23 are still negative but are 8p.p. smaller and only statistically significant at a 10% level ($p - value = 0.078$). In this case, the size of the effect is cut by more than one-third compared to the same outcome measured at age 18. Similarly, the ITT effect of *PANES/AFAM-PE* on the number of birth measured at age 23 is still negative but half in size. This suggests that the substantial effects observed at very early ages may have started to fade. The visual evidence is also more nuanced. At age 23, the effects on education are measured on tertiary, instead of secondary, enrollment. However, the results still show that *PANES/AFAM-PE* have not induced strong responses on this margin. Finally, the more intense response when outcomes are measured at age 23 comes from labor market participation. In this case, both the binary and the continuous variable suggest strong positive and statistically significant responses. First, the ITT effect on the probability of having had at least one spell of four consecutive months in the labor market is 3.4p.p., which accounts for 5.91% of the control mean ($p - value = 0.001$). When looking at the number of months worked, eligible individuals have, on average, 2.0 more, which accounts for 10.3% of the mean of the ineligible group ($p - value < 0.001$).

Finally, estimates measured at age 30 show that the negative effects on fertility observed for women at age 18, and to a smaller extent at age 23, have faded out. Furthermore, the coefficient is now positive, which could signal that the effect may have even started to reverse. However, the estimate is highly statistically insignificant ($p - value = 0.822$), and there is not enough evidence to distinguish it from 0. The positive extensive margin effects on labor market participation also seem to have faded out. The estimated effect on the probability of having had a spell of four consecutive months in the formal labor market by age 30 is now 1.6p.p., which is less than half of the estimate reported for age 23, and statistically insignificant ($p - value = 0.559$). A similar decline, but not as strong in magnitude, is observed for the number of months worked in the formal labor market. In this case, the coefficient is now 2.9 months larger for eligible individuals, but it represents only 7.32% of the control mean, less than the 10.3% estimated at age 23. Finally, unlike estimates measured at

age 18 and 23, when measured at age 30, *PANES/AFAM-PE* seems to have a negative ITT effect on tertiary education enrollment of 3.3p.p.. Because of the meager enrollment rates in this population, it accounts for 48.8% of the control mean average.

Appendix E reports additional robustness and sensitivity tests. First, I test whether ITT estimates are robust to include additional baseline variables as control variables. Second, I report estimates measured at 18, 23, and 30 but using the balanced *restricted sample* instead of the *main sample*. Overall, the estimates reported for these alternative specifications are consistent with the ones obtained using the baseline specification. For estimates that include additional covariates, magnitude, size, and statistical significance are almost identical. For the balanced *restricted sample*, estimates are very similar in direction and slightly stronger in size. Furthermore, additional evidence supports the existence of weak positive ITT effects on secondary enrollment at or before age 18. In this case, these effects are detected even when using the binary outcome variable. However, in most cases, the effects are more imprecisely estimated because of a substantial reduction in the sample size, and the $p - values$ are moderately larger. Overall, the sensitivity and robustness tests support the validity of the baseline estimates.

The analysis of the results reported in Table 4 shows that ITT estimates present a different picture for each outcome depending on the age at which the effects are estimated. This illustrates the importance of understanding the whole dynamics of the effects. "Snapshots" taken at different ages would provide different conclusions about the program's effectiveness. However, the results reported so far must be interpreted as lower bounds for the actual treatment effects since they reflect the effect of eligibility but do not account for actual participation. Estimates that consider differences in participation status rather than in eligibility status, i.e. (local) average treatment effects based on the fuzzy RDD specification, are reported in the next section.

Local Average Treatment Effect

In this section, I report estimates based on the preferred 2SLS specification described in equations 1 and 2. The difference between the ITT effects and the LATE effects reported in this section is that LATE effects scale up the ITT effect by the size of the first stage, i.e., by the actual change in the probability of participation at the eligibility threshold. Table 5 is structured in the same way as Table 4 and reports the baseline LATE results.

LATE estimates present similar patterns to calculations based on the reduced form specifications. In general, *PANES/AFAM-PE* negatively affects fertility at early ages, but these effects attenuate at age 23 and even reverse by age 30. The magnitudes estimated for LATE effects are larger than for ITT effects because they are scaled up by the 30p,p, change in the probability of treatment at the threshold. For instance, the LATE effect on the binary fertility variable estimated at age 18 is -9.4p.p., which accounts for 41.2% of the control mean, and 0.1 births (or 41.9%) for the continuous variable. The positive effect on labor market participation at or before age 23 is also stronger. In the case of the binary variable, the point estimate is 8.3p.p. (14.3%), and 3.1 months (16.0%) when using the continuous variable. Finally, the LATE effect on tertiary enrollment at age 30 is -1.8p.p., or 25.2%. In this case, a smaller coefficient is most likely explained by a large change in the optimal bandwidth

(from 0.036 to 0.165).

Appendix F provides a series of complementary tests to inform about the robustness and sensitivity of the results to alternative specifications. First, I replicate Table 4 but using the *restricted sample* exclusively. As discussed for ITT estimates, the results based on the *restricted sample* are very similar to the estimates reported in Table 4 with only a few minor differences. For instance, the effects on fertility are slightly larger when using the restricted sample (-11.7p.p., and -0.123 births). Still, these are not statistically different from the estimates obtained in the baseline specification (9.4p.p. and 0.108 births). Relative to the estimate obtained in the baseline specification, the positive effect on the continuous education variable measured at age 18 is substantially smaller and statistically insignificant. This confirms that, if anything, the evidence about positive effects on secondary enrollment is weak. The effects on labor market participation also resemble the baseline results. At age 18, the effect estimated using the balanced sample is now statistically significant, but the coefficient is almost the same as in the baseline specification (2.5p.p. versus 2.6p.p.). The positive effects observed on the binary labor market variable are also present at age 23, but the magnitude is smaller than the baseline specification. Estimates for outcomes measured at age 30 are the same as in Table 4 because they were already based on the *restricted sample*.

The second sensitivity test replicates the analysis but uses alternative definitions of the endogenous treatment variable. First, I substitute the binary treatment variable for a continuous variable that indicates the number of years in the program before turning eighteen. Estimates based on this alternative definition are almost identical in direction, statistical significance, and size when scaled up by the average value of the treatment variable. The same is true for estimates based on a continuous variable that measures the net present value of the total cash transfer amount collected by the household before the individual turns eighteen years old. The only difference, in this case, is that the negative effects on fertility seem to be slightly smaller. In particular, while the LATE effects on fertility outcomes measured at age 18 are -41.12% and -42.0% for the binary and continuous variable, respectively, when using the binary treatment variable, these are -26.4% and -25.9%. Despite these minor differences, the size of the effects on fertility is still large and both economically and statistically significant.

Finally, to rule out that the effects are driven by specific choices of the parameters involved in the RDD estimate, Appendix F also reports the specification curves for each binary outcome. Specification curves are extremely useful to rule out that the estimated effects are driven by specific arbitrary technical choices in the parameters required to estimate an RDD using local polynomial regressions. Specifically, these specification curves plot the point estimate and 90% confidence intervals for all possible combinations of choices of 1) criteria used to define optimal bandwidth, 2) kernel functions, 3) polynomial degree and 4) use of covariates, sorted by point estimate. Overall, the specification curves illustrate that the size and direction of the effect are consistent across different combinations of parameters and that estimates from the preferred specification (i.e., MSERD, triangular, and including covariates) are usually very close to the median estimates and, if anything, errs toward null effects.

Heterogeneous Responses by Sex

In this section, I report the baseline LATE estimates but split the sample by sex of the individual. Figure 6 summarizes these results and depicts the point estimates and confidence intervals for each estimate reported in Table 4. In addition, the figure also reports a note including the p-value of a test of difference of coefficients between men and women.[38] Panel a. reports estimates for the binary outcome variables, while panel b. reports the estimates associated with the continuous variables. For the latter, to make the comparison between coefficients easier, I report standardized effects. Because fertility variables are only measured for women, the estimates reported in Figure 6 are the same as in Table 4, except for the standardization used in panel b.

The direction of the effects of *PANES/AFAM-PE* on secondary education enrollment measured at age 18 is different for men and women. At age 18, the LATE effect on secondary enrollment is 2.1p.p. (3.1%) for men but -2.8p.p. (-4.1%) for women. In both cases, the effects are statistically indistinguishable from 0 ($p-value = 0.662$ for males, and $p-value = 0.331$ for women). Despite these apparent differences, I cannot reject the equality of coefficients at traditional significance levels ($p-value = 0.338$). A slightly different pattern is observed for estimates obtained using the continuous variables. For males, the estimated effect is 0.13 std. deviations (or 0.28 additional years enrolled) and statistically significant at a 5% level ($p-value = 0.031$). For women, the effect goes in the same direction, and it is similarly sized, i.e., 0.10 std. deviations (or 0.21 years), but again statistically insignificant ($p-value = 0.231$). As for the binary variables, the difference between the two coefficients is statistically insignificant ($p-value = 0.767$). A similar mixed pattern is observed when estimates are computed at ages 23 and 30 and focus on tertiary enrollment instead. At age 23, there are null effects of *PANES/AFAM-PE* on tertiary enrollment for men or women. The differences between them are also negligible ($p-value = 0.392$). However, when examining estimates measured at age 30, both treated men and women show a lower tertiary enrollment rate than their counterparts in the control group. For men, this effect is -3.8p.p. which accounts for 55.9% of the control mean and is statistically significant ($p-value = 0.013$). For women, the effect is larger - 6.5p.p. (95.77%) and also statistically significant ($p-value = 0.032$).[39] However, the equality of coefficients cannot be rejected at traditional levels ($p-value = 0.440$). This is most likely due to the more imprecise estimates obtained when using the *restricted sample* that has substantially fewer observations than the main sample.

Overall, the evidence reported on the effects of *PANES/AFAM-PE* on education by gender goes in the same direction as estimates reported in previous sections. First, it provides mixed evidence of the effects of *PANES/AFAM-PE* on secondary education enrollment. If anything, there are some weak positive effects when the intensive margin is incorporated into the analysis, but the effect is similar between men and women (about 0.1 std. deviations, or 10.9% and 8.60%, respectively). Regarding tertiary education outcome, *PANES/AFAM-PE* seems to reduce the share of individuals ever enrolled at college at or before age 30. While the point estimate is larger for women, the effects are not

[38] Full tables are reported in Appendix F.

[39] To understand the size of the effect measured in percentage terms, it is important to keep in mind that enrollment rates in the university for this population are meager. For instance, in none of the samples used throughout the paper, the share of individuals enrolled in tertiary education is larger than 7%. See Appendix C for additional descriptive statistics about each of the outcomes used in the paper by sample of analysis.

statistically different by sex.

The effects of *PANES/AFAM-PE* on labor market outcomes present differential patterns between men and women for the binary and the continuous variables. Measured at age 18, the LATE effect on having worked four consecutive months is 2.8p.p. (14.4%) for women, but only 0.1p.p. for men (2.77%). The coefficients are not statistically significant in both cases, as neither is the difference between them. Similar but stronger differences are observed when estimates are conducted using the continuous outcome variable. The program's effect on labor market participation is -0.01 standard deviations for men, which is equivalent to -0.027 months or -1.22%, and it is statistically insignificant ($p - value = 0.598$). For women, the estimated effect is 0.12 standard deviations, equivalent to 0.5 months, or 22.36%, and borderline statistically significant ($p - value = 0.103$). However, it still cannot be ruled out that both coefficients are the same ($p - value = 0.333$). When measured at age 23, the differential effects become larger. Using the binary outcome variable, the estimated effect on women is 8.1p.p. (13.9%), which is statistically significant at a 10% level ($p - value =$). For men, this coefficient is less than half, 3.4p.p. (6.7%), and statistically insignificant ($p - value = 0.227$). However, the difference between the two coefficients is still statistically insignificant ($p - value = 0.441$). When using the continuous outcome variable, the differential effects become even larger. For men, the estimated effect on the number of months worked is 0.00 std. deviations (-0.1 months or -0.40%) and statistically insignificant ($p - value = 0.794$). For women, it is 0.20 standard deviations (4.3 months, or 21.8%), and statistically significant ($p - value = 0.014$). Furthermore, the difference between the coefficients is now statistically significant at a 10% level ($p - value = 0.075$). Finally, a similar comparison can be made for estimates measured at age 30. However, the effects are more imprecisely estimated in this case due to the reduced sample size. Despite being less precisely estimated, the effects are still larger for women. A more in-depth discussion about this differential pattern is presented in the next section when the full dynamic effects are described.

6.3 Dynamic Effects

In the previous section, I reported the estimated LATE effects of *PANES/AFAM-PE* on outcomes measured at three specific and arbitrary age cutoffs. The differential patterns of effects depending on the age and dimension illustrate the importance of considering the whole picture when assessing the program's effectiveness. Taking "snapshots" at different ages could yield misleading conclusions if one disregards the complete individual trajectories. For this reason, in this section, I provide a more thorough analysis where I report estimates measured at every age allowed by data. In particular, I report results for outcomes measured as early as age 12 in the case of education outcomes and at age 15 for fertility and labor market outcomes. In all cases, the analysis period spans until age 30, the last age at which there is a sufficiently large number of observations to implement a reliable RDD. To maximize the amount of information used in the empirical analysis, the effects reported in this section are based on the *extended sample*. To rule out that observed trends are driven by changes in the sample's composition rather than by actual changes in the program's effects, Appendix G reports estimates based on the *restricted sample*. In this set of results, the sample composition is held constant. The main caveat when interpreting the *restricted sample* results is that estimates are more imprecise

due to the reduced sample size. Because of the heterogeneous effects reported in the previous section, the dynamic analysis is also presented separately for men and women.

Figures 7 and 8 report the main findings for the binary and continuous variables. To make comparisons easier, all estimates are expressed in standard deviations.[40] In panel a. I report estimates of the effect of *PANES/AFAM-PE* on fertility outcomes measured at different ages.[41] The evolution of the effects on fertility outcomes measured at different ages is similar for both binary and continuous variables. In both cases, the program's effects are strong, negative, and statistically significant when measured at ages around 17 and 18 years old. Estimates are also negative between 20 and 25 years old but slightly smaller in magnitude and statistically insignificant. The only exceptions are the coefficients on number of births measured at ages 23 and 24 (-0.14 and -0.17 births, respective), which are negative and statistically significant ($p-value = 0.067$ and $p-value = 0.031$). While the magnitude of these coefficients measured in percentage points is larger compared to the effects measured at 17 or 18 years old, the size of the effect relative to the control mean is much smaller (16.7% and 18.6%). The effects start to trend toward the positive side for both the binary and the continuous variables starting at age 25. This u-shaped pattern is consistent with women delaying the age at which they have their first birth rather than with changes in overall preferences for fertility.

Panels b. in Figures 7 and 8 report estimates for secondary education outcomes. In this case, the effects are reported only until age 23 since secondary education measures are not very informative after the mid-twenties. The effects on secondary enrollment are mostly null for men and women. There are some exceptions, such as the estimates at age 13 and 21 for men and at 15 and 23 for women. However, there is not a clear, consistent pattern. The visual evidence suggests a slightly different story when the effects are measured using the continuous variable. First, the effects measured between ages 18 and 22 are consistently positive and statistically significant for men. For these four years, point estimates are between 0.15 and 0.2 standard deviations (i.e., a third of a year, or about 14% of the control mean). In all cases, the estimates are statistically significant ($p-values$ range between 0.019 and 0.058). On the contrary, estimates for women are smaller than 0.07 standard deviation for any age in this range. However, I cannot reject the equality of coefficients at the usual significance levels. For instance, $p-values$ are between 0.107 and 0.130 for estimates measured between ages 18 and 20 and between 0.220 and 0.525 for ages 21 and 22. Estimates for the complete age range still yield mixed evidence about the program's effects on secondary education outcomes when estimated separately on men and women. If anything, the program seems to have a weak positive effect on years enrolled in secondary education for men, especially when the outcome is measured in the late teens.

Panel c. in Figure 7 reports the analogous estimates for tertiary education enrollment. In this case, estimates are reported from ages 17 to 30 and only for the binary variable because of the data limitations discussed in Section 4.1. The trajectory of the effects is consistent with the results discussed in Section 6.2, based on the three age cutoffs. *PANES/AFAM-PE* does not seem to affect the probability of ever being enrolled in tertiary education between late teens and late twenties. However,

[40] Appendix G include Tables with point estimates, standard deviations, robust p-values, and p-value of the equality of coefficients tests.

[41] For instance, when the outcome is measured at age 25, the estimated effect of *PANES/AFAM-PE* corresponds, in the case of the binary variable, to the effect on the probability that a woman has given birth at or before age 25.

starting at age 28, the effect becomes negative both for men and women. In terms of size, this effect is between 0.16 and 0.22 standard deviations (i.e., around 5p.p., or 50%) for women's tertiary enrollment measured between ages 28-30, and statistically significant in the three cases ($p-value = 0.036$, 0.091, and 0.049, respectively). For men, the coefficient is close to 0 for ages 28 and 29 but becomes negative and statistically significant at age 30. The point estimates for men and women cannot be statistically distinguished from each other in either of these cases.

Binary labor market outcomes are reported in panel d. of Figure 7. Estimates for the continuous variables are reported in panels c. and d. of Figure 8. It is important to note here that panel d. of Figure 8 presents additional complementary evidence of an outcome variable that measures the cumulative earnings from labor market participation during the period. The differences between men and women are striking in terms of labor market participation. For men, *PANES/AFAM-PE* does not seem to have affected either labor market participation, months worked, or total cumulative earnings. Neither of the estimates for any of the outcome variables measured at any of the 16 ages shows a $p-value < 0.100$. On the contrary, estimates for women provide substantial, robust evidence of positive effects. These start as early as around 17-18 years old and continue relatively stable until mid or late-twenties, depending on the outcome. In particular, the effect of *PANES/AFAM-PE* on women's probability of having at least one four-month spell in the formal labor market measured at or before age 18 is 8p.p. (or 58.9% of the baseline mean). For men, this same effect is -2p.p., but statistically insignificant ($p-value = 0.439$). The test of equality of coefficients for this outcome is rejected at a 5% level ($p-value = 0.031$). A similar difference is observed when the variable is measured at age 19.

The differences are even larger when effects on labor market outcomes are estimated based on continuous variables. The positive effects on the extensive margin observed for women translate into a maximum effect of 0.27standard deviations in the number of months worked (i.e., 7.12 months, or 30% of the control mean) when measured at age 25. At this age, the differential effect relative to men is around 8.3 months. More generally, the differences between the effect on the number of months worked by men and women are statistically significant between ages 17-19 and between 23 and 27. The heterogeneous responses observed when comparing men and women are not as large when cumulative earnings are used to measure labor market outcomes. While some evidence suggests that *PANES/AFAM-PE* might have had a more substantial effect on women's earnings, the differences are not as clear as for months worked. These are only statistically significant for estimates corresponding to ages between 24 and 25 and are somewhat smaller when measured in percentage terms relative to the control mean. Finally, for each of the three labor market outcomes utilized, the effects attenuate by the late twenties and become null when reaching the 30s. Measured at 28, 29, or 30 years old, the program's effects are null both for men and women. Furthermore, by this age, there are no signs of the strong positive differential in favor of women's outcomes observed at the early twenties.

In Section 6.2, I showed that the program has different effects for different outcomes depending on the age at which they are measured. In particular, the program has negative effects on fertility outcomes measured at age 18, positive effects on labor market participation measured at age 23, and negative effects on tertiary education enrollment at or before age 30. The analysis of heterogeneous

responses indicates that labor market responses are driven mainly by women. The results in this section illustrate the trajectory of the effects measured for the 15- to 30-year-old period. Figure 9 presents these results differently by combining them in pairs of two outcomes. This exercise aims to provide visual evidence of regular trends in the effects by age on different outcomes that could suggest the potential mechanisms behind these responses. Panel a. reports estimates for the effect of *PANES/AFAM-PE* on women's fertility and labor market outcomes. Panel b. does the same for fertility and education outcomes. Finally, panel c. focuses on men's tertiary and labor market outcomes.

Panel a. illustrates that effects on fertility and labor market outcomes evolve inversely. Between ages 14 and 20, when the effects of *PANES/AFAM-PE* on fertility become more negative, the effects on labor market outcomes go oppositely and become increasingly positive. When the effects on fertility seem to flatten out between ages 20 and 25, the same happens for estimated effects on labor market outcomes. Finally, when the effects on fertility start to become positive, the positive effects observed in early ages for labor market outcomes start to fade out until they virtually reach 0. A similar inverse evolution is observed when comparing the effects on fertility and tertiary education outcomes. However, the pattern is less clear because most of the estimates on tertiary education enrollment are null until the late twenties. The analysis of men's outcomes also illustrates a negative relation between the labor market and tertiary education effects, but in most cases, they oscillate around null effects.

The complete set of results reported in Sections 6.2 and 6.3 present suggestive evidence about the effects of *PANES/AFAM-PE* on outcomes that define individuals' transition to adulthood and provide convincing evidence about the mechanisms that could explain them. *PANES/AFAM-PE* significantly reduces teenage pregnancies for women that participated in the program. This effect is followed by strong positive effects on labor market participation, months worked, and earnings. However, the negative effects on fertility observed in late teens start to fade out and even become positive by the end of the twenties. This indicates that the program might have delayed first pregnancies but has not affected overall fertility preferences. When women start to have children at later ages, the positive effects on labor market outcomes start to revert. This is also observed, although to a smaller extent, for enrollment in tertiary education. On the other hand, men seem to have been mostly unaffected by the program, except for some potential weak effects on the years enrolled in secondary education.

7 Discussion and Comparison with Existing Literature

IN PROGRESS...

8 Marginal Value of Public Funds

IN PROGRESS...

9 Conclusion

Worldwide, governments spend billions of dollars on social safety net (SSN) policies to reduce poverty and inequality. With different designs depending on the context, cash transfers are one of the simplest and most used policy instruments for this purpose. Cash transfers can affect the lives of beneficiary household members in several ways. For instance, they can change parents' time allocation between labor, leisure, and housework, or children's school enrollment and healthcare decisions. All these changes affect the current lives of individuals but can also have long-lasting consequences, especially for children who benefited from the program in early life. This paper presents evidence of how a large-scale and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*, affects the transition to adulthood of individuals that benefited from the program when they were young.

The main results suggest that *PANES/AFAM-PE* strongly affects women's lives. In particular, it reduces teenage pregnancies by 9.4p.p. (or 41.2% of the control mean), with a subsequent increase in labor market participation, months worked, and earnings. The program's effects on labor market outcomes peak at around 25 years old, when participant women have, on average, worked seven additional months compared to their counterparts in the control group. This translates into cumulative earnings that are USD 4,249 (28.4%) larger. The complete dynamic analysis shows that the negative effects on fertility in late teens start to fade out and even become positive by the end of their twenties. When women start to have children at later ages, the positive effects on labor market outcomes start to revert. This is also observed, although to a smaller extent, for enrollment in tertiary education. Men, on the contrary, seem to have been mostly unaffected by the program, except for some potential weak effects on the years enrolled in secondary education. Overall, *PANES/AFAM-PE* seems to have strongly affected women's transition to adulthood by reducing teenage pregnancies and increasing labor market participation during the same period. However, the attenuation and reversal of the effects observed at age 30 suggest that responses might be more associated with changes in the events' timing rather than more general changes in fertility or labor market preferences.

This paper makes several contributions to the literature. First, it contributes to an incipient strand of literature that analyzes the effects of social safety net policies aimed at households with children based on how those children's adult outcomes (Aizer et al., 2022). To my knowledge, this is the first paper that analyzes the effects of a cash transfer program on three significant margins of response (fertility, education, labor market) with a dynamic perspective (i.e., from late teens to adulthood) in a unified setting using high-quality administrative records with information that varies at the individual-level both for the outcome and participation variables. Second, this paper contributes to a growing literature that discusses non-linear trajectories in the dynamic effects of different types of policy interventions (e.g., Chetty et al., 2011; Almond et al., 2018) by providing evidence from a different type of intervention, i.e., cash transfers, at ages that are not necessarily restricted to early childhood. This is presumably a more general setting than the type of interventions analyzed in the early childhood literature. Third, this paper contributes to the literature on gender inequality in the labor market (Altonji and Blank, 1999, Blau and Kahn, 2017, Olivetti and Petrongolo, 2016 Waldfogel,

1998 Kleven et al., 2019,?).by providing complementary evidence that highlights how fertility decisions, in particular during the critical adolescent ages, might have long-lasting consequences in terms of labor market participation, experience, earnings, and overall stability. A final and tangential contribution of this work exceeds the specific scope of this paper and relies on the dataset assembled for the analysis. While this paper focuses on the broad picture, the richness of some of the information included in the administrative records opens the door for digging deep into mechanisms behind the observed responses and for the analysis of other dimensions of individual behavior that could be relevant to understand the overall effectiveness of cash transfer or welfare programs.

It is also important to note that most existing studies about cash transfer programs' short- and long-term effects are based on interventions carried out in rural villages in low- or middle-income countries or transitory programs implemented by local or international NGOs. Furthermore, even in places where these policies were implemented by the government - such as in the case of *PROGRESA*, Mexico - the program was focused on rural communities. This limits the lessons that we can learn from the existing evidence. On the contrary, *PANES/AFAM-PE* is a permanent and government-implemented policy at the national level. Furthermore, the Uruguayan *PANES/AFAM-PE* provides an interesting case of study because it is a policy typically associated with low and middle-low income countries but is implemented in a relatively richer country. Hence, the evidence reported in this paper could also help understand how similar policies might work in different settings.

Overall, this paper provides strong evidence of the beneficial effects of *PANES/AFAM-PE* on early adulthood outcomes. This indicates that social safety net policies can indeed be effective tools to improve conditions to reduce poverty in the long run. While the evidence seems to suggest that the effects are related more to the timing of the events (e.g., delay in fertility or anticipation of formal labor market insertion) rather than to more general changes in fertility or labor market preferences, these results are still relevant from a life-cycle perspective. For instance, delaying births can have beneficial consequences throughout women's lives in terms of labor income and labor participation stability. Furthermore, early access to benefits from formal employment such as health insurance and a long history of social security contributions can have long-lasting effects. Finally, the evidence about strong heterogeneous effects might be important to discuss whether SSN expenditure can be allocated more efficiently.

Figures

Figure 1: Description of the Program

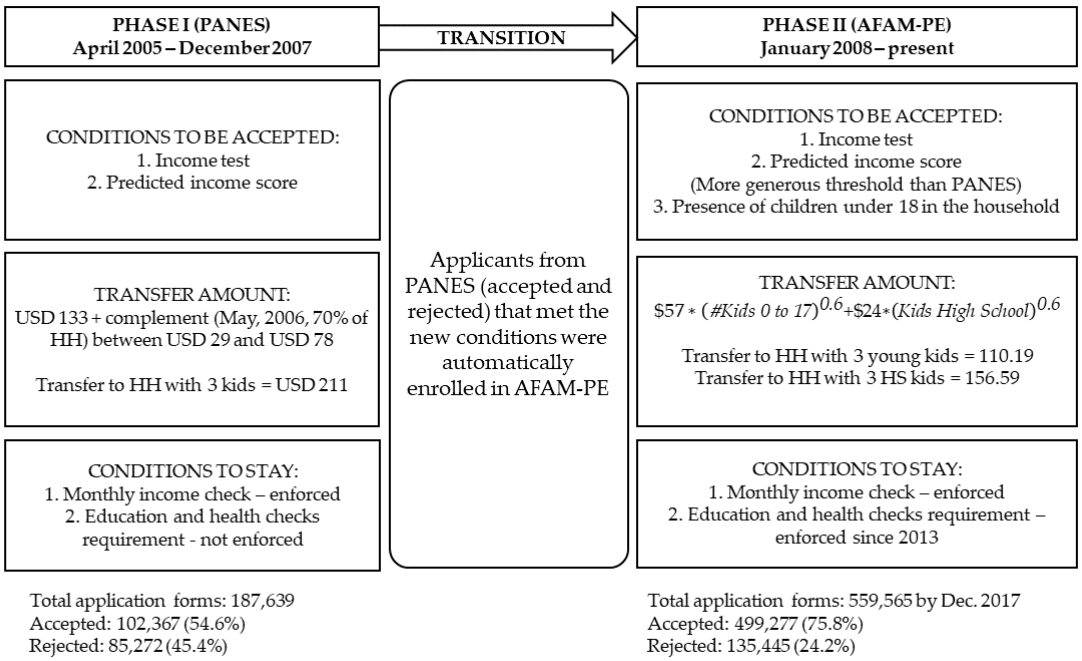
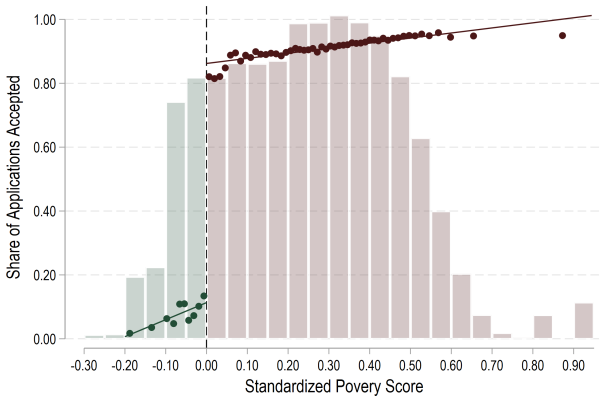
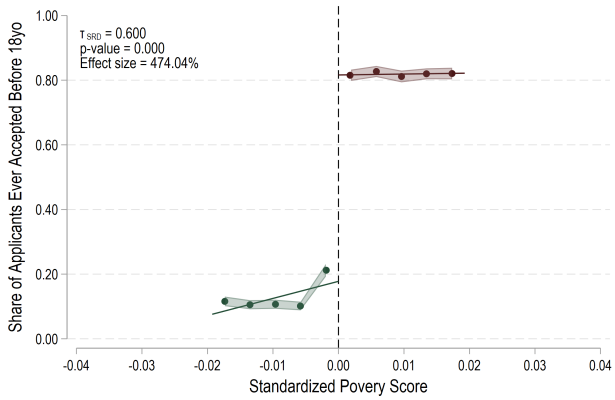


Figure 2: Relation Between Application Form Eligibility and Resoultion - Main Sample

a. Full Support



b. Optimal Bandwidth

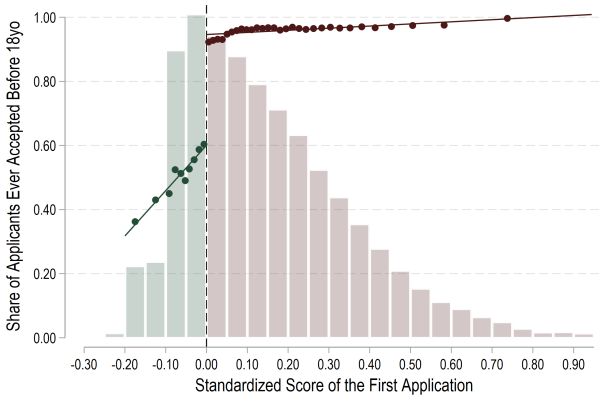


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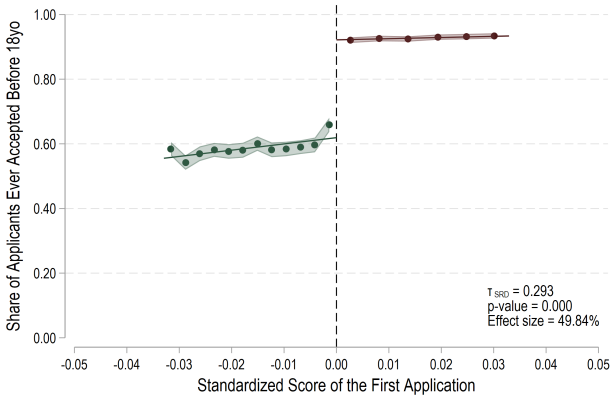
Figure 3: Participation Rule Using First Application Form - Main Sample

Ever Treated Before 18yo

a. Full Support



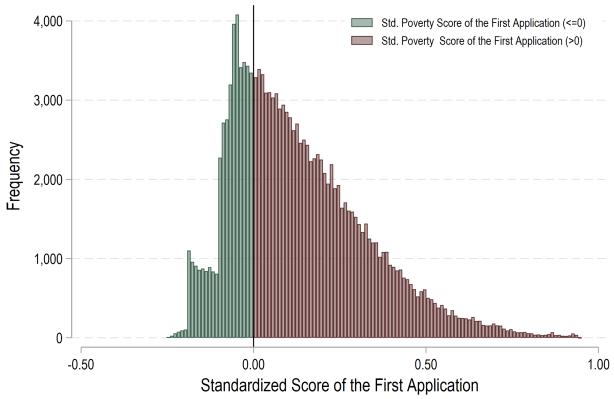
b. Optimal Bandwith



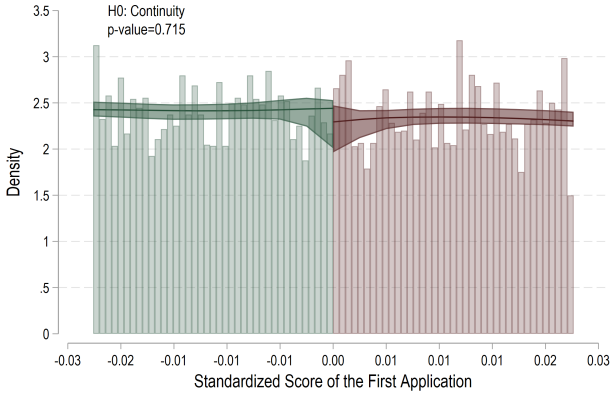
Notes:

Figure 4: Continuity of the Poverty Score in 1st. Application Form - Main Sample

a. Distribution of the Poverty Score



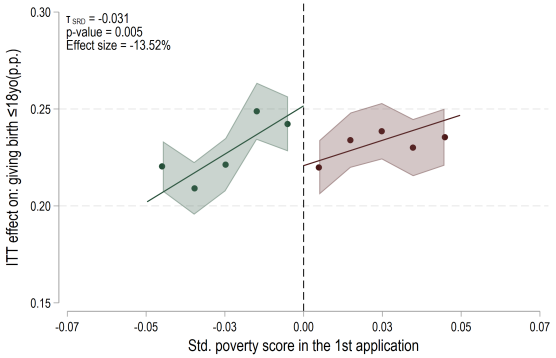
b. Cattaneo, Jansson, and Ma Test

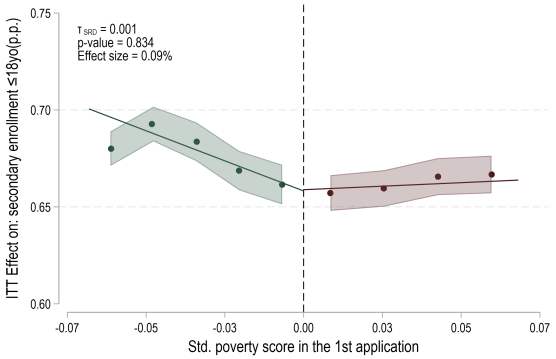


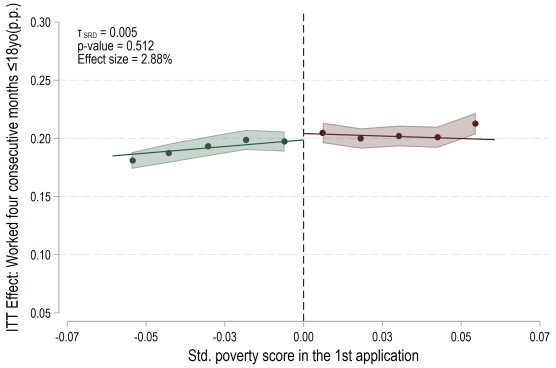
Notes:

Figure 5: Graphic Evidence: Intention to Treat Effects, by Age - Estimates Without Covariates

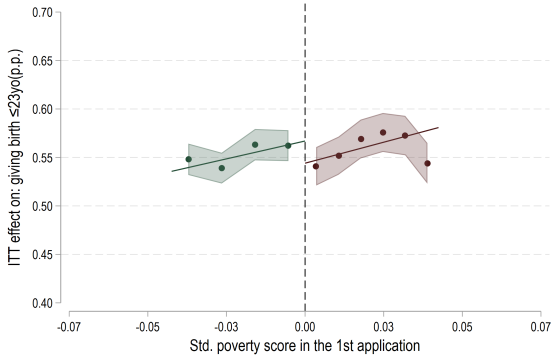
a. 18 years old

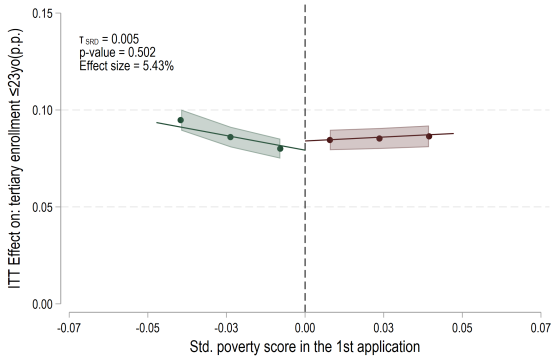


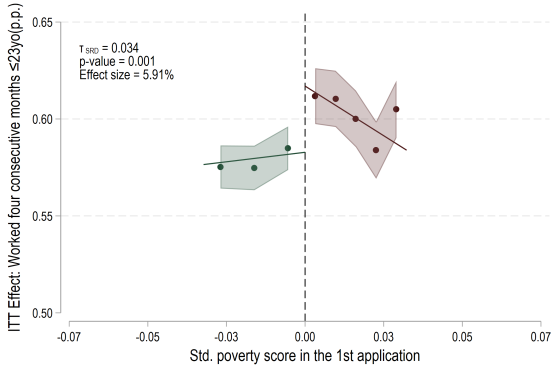




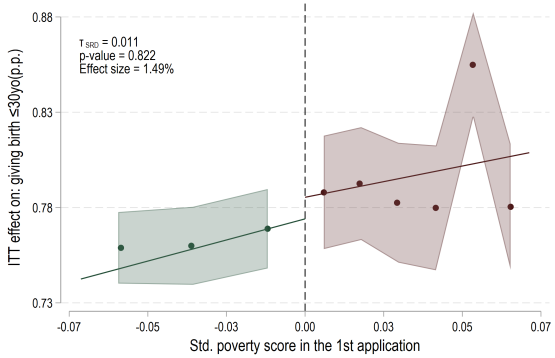
b. 23 years old

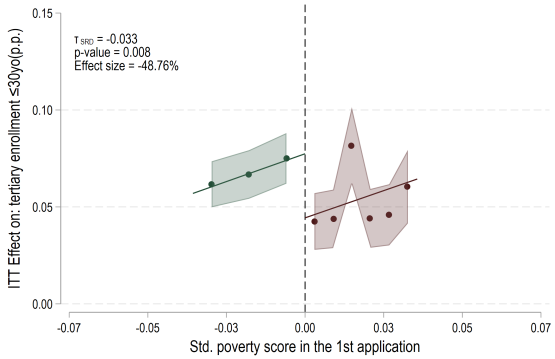


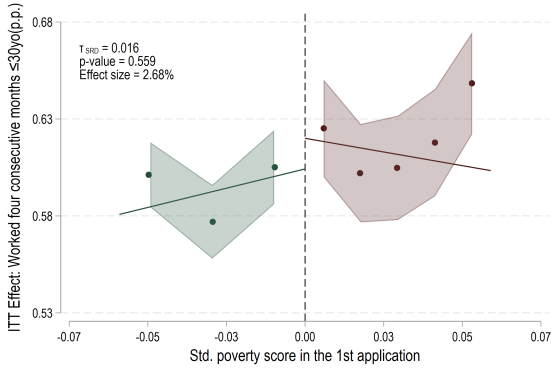




c. 30 years old



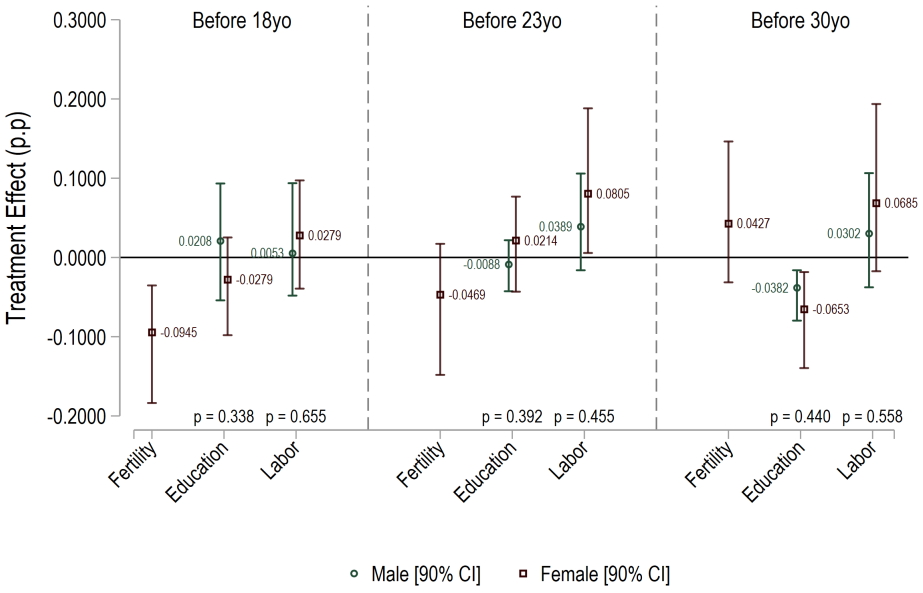




Notes:

Figure 6: Heterogeneity by Gender

a. Binary outcomes



b. Number of events

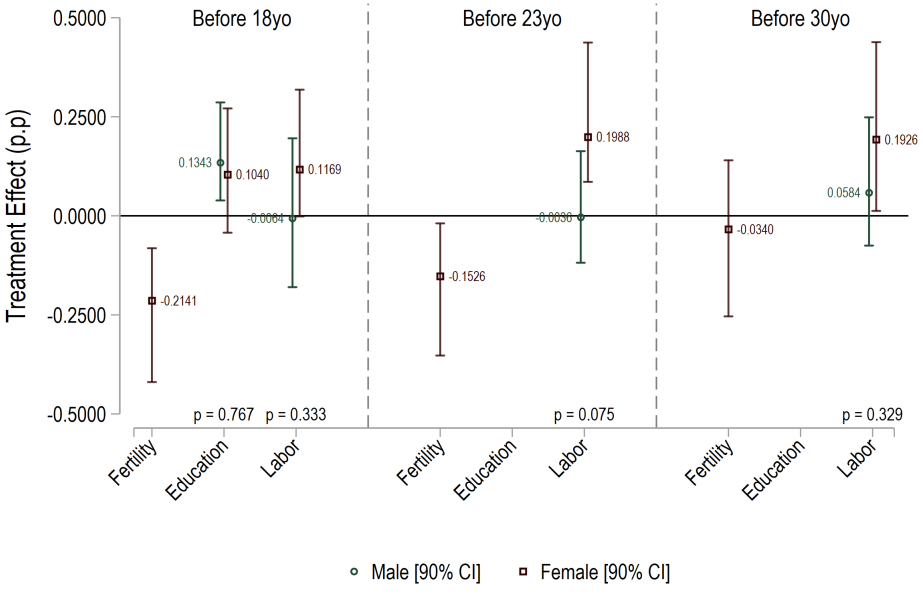
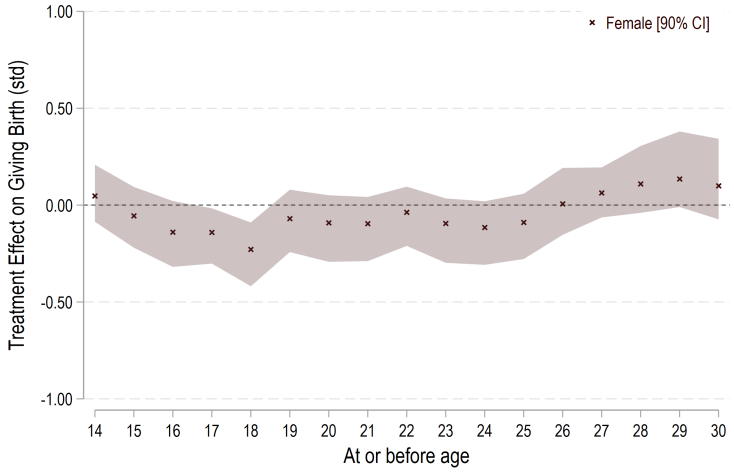
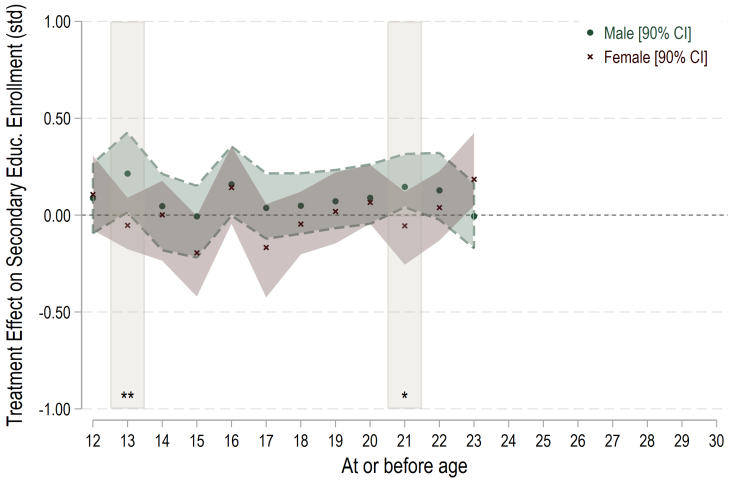


Figure 7: Dynamic Effects, by outcome and gender - Binary Variable

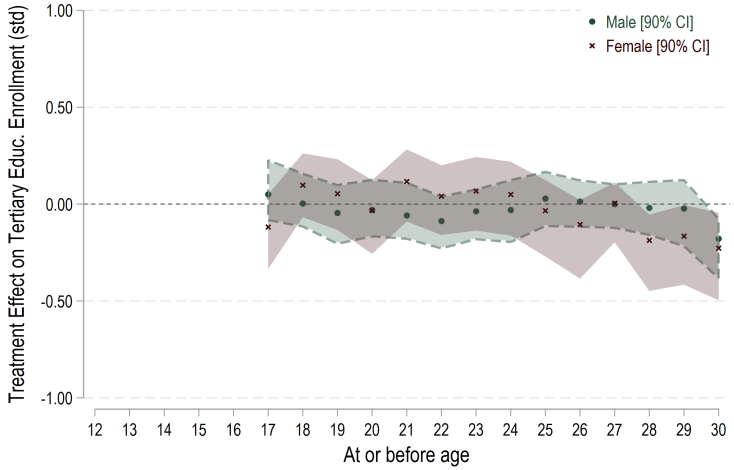
a. Fertility



b. Secondary Education



c. Tertiary Education



d. Labor Market Participation

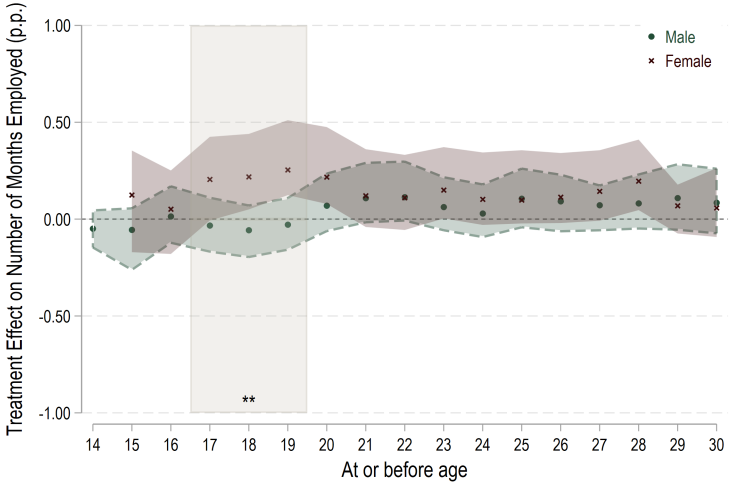
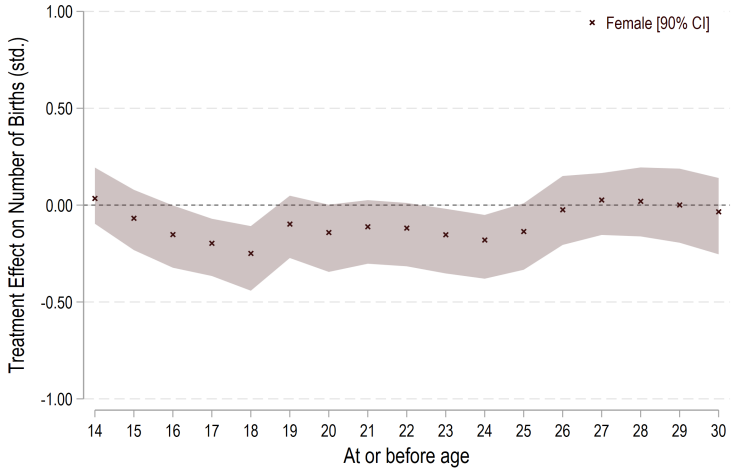
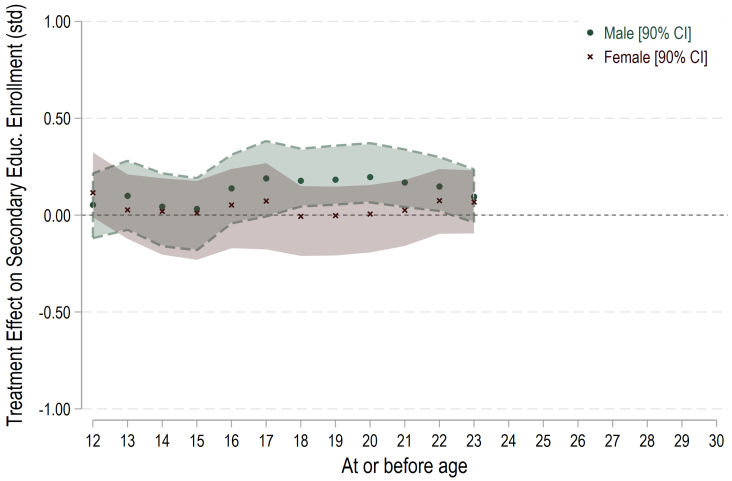


Figure 8: Dynamic Effects, by outcome and gender - Continuous Variable

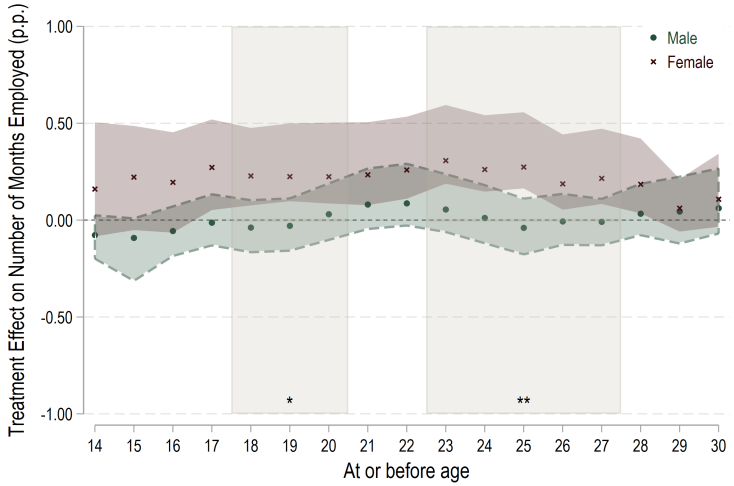
a. Fertility



b. Secondary Education



c. Cumulative Months Worked



c. Cumulative Earnings

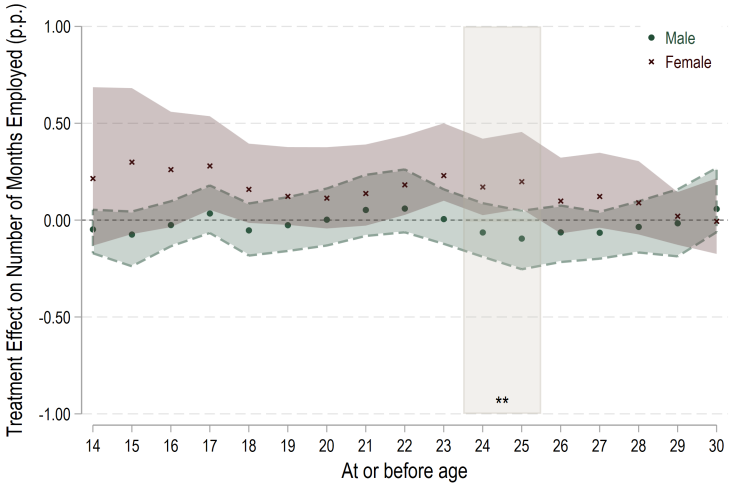
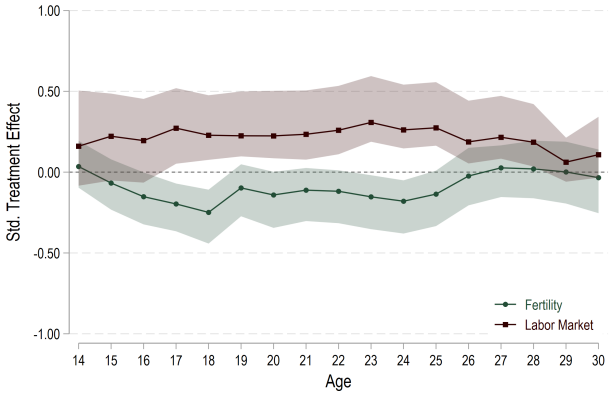


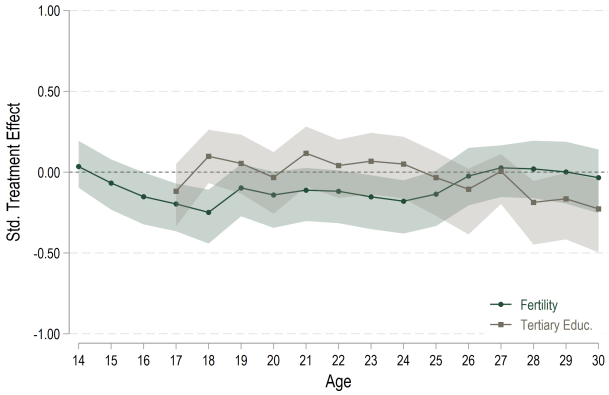
Figure 9: Dynamic Effects, Combined

Women

a. Fertility & Labor Market

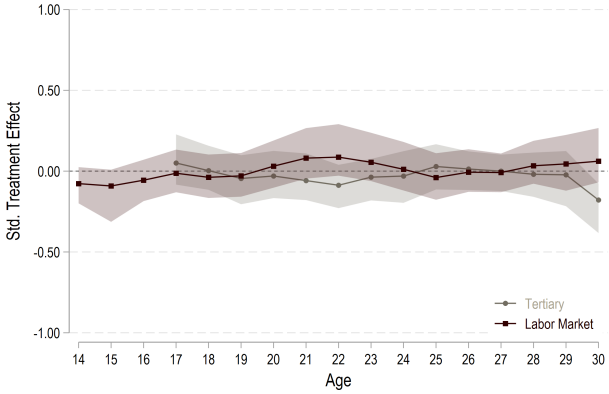


b. Fertility & Tertiary Education



Men

c. Tertiary & Labor Market



Tables

Table 1: Descriptive Statistics: Individual Characteristics - By Sample

	Main Sample: ≥ 23 years old At Dec, 2019		Restricted Sample: ≥ 30 years old Dec, 2019	
	All (1)	Opt. Bandwidth (2)	All (3)	Opt. Bandwidth (4)
a. Individual Characteristics				
Female (%)	50.32	50.70	49.88	50.86
	(50.00)	(50.00)	(50.00)	(49.99)
Number of HH.	1.77	1.63	1.93	1.72
	(1.10)	(0.97)	(1.20)	(1.04)
Age at 31 Dec. 2019	26.91	27.11	31.11	31.09
	(2.56)	(2.61)	(0.68)	(0.66)
Age of 1st application	13.42	13.40	16.84	16.87
	(2.59)	(2.61)	(0.69)	(0.67)
Accepted before 18yo (%)	84.16	73.62	71.04	57.01
	(36.52)	(44.07)	(45.36)	(49.51)
Number of app. forms	2.56	2.47	2.44	2.29
	(1.44)	(1.35)	(1.31)	(1.24)
In *PANES* form (%)	78.39	86.89	100.00	100.00
	(41.16)	(33.75)	(0.00)	(0.00)
In *AFAM-PE* form (%)	96.08	93.21	91.40	88.20
	(19.42)	(25.16)	(28.04)	(32.27)
b. Reference Form				
Std. Score	0.18	-0.00	0.16	0.01
	(0.25)	(0.05)	(0.27)	(0.08)
App. Accepted (%)	71.82	49.08	69.58	54.82
	(44.99)	(49.99)	(46.01)	(49.77)
PANES (%)	78.39	86.89	100.00	100.00
	(41.16)	(33.75)	(0.00)	(0.00)
Capital City (%)	31.25	18.18	29.64	21.51
	(46.35)	(38.56)	(45.67)	(41.09)
c. Household characteristics (ref. form)				
Single Parent (%)	46.73	48.96	48.30	49.91
	(49.89)	(49.99)	(49.97)	(50.00)
Number of members	4.92	4.38	4.98	4.45
	(1.98)	(1.82)	(2.12)	(1.88)
Number of children	2.95	2.41	2.91	2.41
	(1.69)	(1.45)	(1.78)	(1.46)
Avg. age	23.13	25.13	24.35	26.01
	(7.60)	(8.00)	(7.85)	(7.97)
Household Head: Ed. years	6.83	7.20	6.72	7.02
	(3.41)	(3.55)	(3.47)	(3.53)
Household head: Employed (%)	63.43	64.55	65.31	65.43
	(48.16)	(47.84)	(47.60)	(47.56)
Household head: income	143.33	159.37	128.79	142.72
	(172.54)	(171.86)	(140.96)	(148.44)
Observations	224,413	76,593	34,754	21,441

Notes:

Table 2: First Stage - Main Sample

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
a. Dep. Var: Ever Treated Before 18 Years Old				
Eligibility	0.293***	0.291***	0.297***	0.296***
	(0.009)	(0.010)	(0.009)	(0.011)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	49.84%	52.37%	50.08%	51.72%
Bwd.	[0.033;0.033]	[0.054;0.054]	[0.026;0.026]	[0.043;0.043]
Observations	31,413	52,538	24,551	40,813
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Uniform	Uniform

Notes:

Table 3: Balance of Baseline Covariates - Main Sample

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	First Stage Coeff.	τ_{FRD} Coeff.	p-value Robust	Sharpened FDR q-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Predicted Eligibility	0.66	0.66	0.003	0.294	0.010	0.680	0.559
HH - Avg. Age	25.10	25.42	0.318	0.288	1.101	0.099	0.256
HH - Avg. age adults	39.92	40.53	0.607	0.288	2.111	0.015	0.082
HH - Capital City	0.19	0.17	-0.012	0.296	-0.039	0.150	0.260
HH - Number of people	4.29	4.22	-0.063	0.297	-0.212	0.011	0.082
HH - Number of children	2.37	2.34	-0.028	0.297	-0.093	0.137	0.260
HH - Single Parent	0.53	0.55	0.025	0.282	0.089	0.051	0.181
HHH - Income (IHS)	4.34	4.36	0.025	0.297	0.084	0.597	0.559
HHH - Employed	0.61	0.61	0.004	0.299	0.014	0.797	0.559
HHH - Years of Educ.	7.03	7.06	0.029	0.296	0.100	0.752	0.559
Age at 1st. App.	13.43	13.40	-0.025	0.310	-0.082	0.215	0.357
Age (Dec. 31, 2019)	29.04	28.89	-0.157	0.291	-0.540	0.005	0.082
Number of Apps.	2.80	2.81	0.013	0.295	0.045	0.742	0.559
Female	51.19	50.44	-0.745	0.300	-2.486	0.384	0.392
Number of HH.	1.64	1.61	-0.029	0.297	-0.098	0.111	0.256

Notes:

Table 4: Intention to Treat Effects

	18 years old			23 years old			30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
				a. Dep. Var.: Dummy Variable					
Ever Treated	-0.031***	0.001	0.005	-0.023*	0.005	0.034***	0.011	-0.033***	0.016
	(0.011)	(0.008)	(0.006)	(0.014)	(0.005)	(0.011)	(0.022)	(0.012)	(0.020)
Robust p-value	0.005	0.834	0.512	0.078	0.502	0.001	0.822	0.008	0.559
Effect Size (%)	-13.52%	0.09%	2.88%	-4.14%	5.43%	5.91%	1.49%	-48.76%	2.68%
Bwd.	[0.050;0.050]	[0.068;0.068]	[0.061;0.061]	[0.042;0.042]	[0.047;0.047]	[0.032;0.032]	[0.071;0.071]	[0.036;0.036]	[0.059;0.059]
Observations	24,078	59,390	59,378	20,292	44,827	30,800	6,504	6,247	10,623
				b. Dep. Var.: Number of Events					
Ever Treated	-0.038***	0.086**	0.054	-0.060**		2.019***	-0.041		2.858*
	(0.013)	(0.039)	(0.069)	(0.026)		(0.516)	(0.068)		(1.715)
Robust p-value	0.003	0.029	0.524	0.016		0.000	0.381		0.081
Effect Size (%)	-14.93%	3.41%	2.49%	-7.36%		10.31%	-2.96%		7.32%
Bwd.	[0.052;0.052]	[0.046;0.046]	[0.057;0.057]	[0.040;0.040]		[0.028;0.028]	[0.057;0.057]		[0.064;0.064]
Observations	25,258	43,514	56,322	19,294		26,547	5,245		11,292
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table 5: Local Average Treatment Effects

	$\leq$ 18 years old			$\leq$ 23 years old			$\leq$ 30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
				a. Dep. Var.: Dummy Variable					
Ever Treated	-0.094**	0.009	0.026	-0.047	0.013	0.083***	0.043	-0.018**	0.016
	(0.039)	(0.030)	(0.025)	(0.042)	(0.014)	(0.029)	(0.045)	(0.009)	(0.018)
Robust p-value	0.015	0.608	0.161	0.193	0.518	0.004	0.288	0.031	0.292
Effect Size (%)	-41.19%	1.31%	13.60%	-8.52%	14.69%	14.29%	5.62%	-25.17%	2.60%
Bwd.	[0.044;0.044]	[0.042;0.042]	[0.042;0.042]	[0.050;0.050]	[0.080;0.080]	[0.048;0.048]	[0.040;0.040]	[0.165;0.165]	[0.163;0.163]
Observations	20,033	32,948	37,375	22,711	72,395	42,834	3,343	20,098	19,976
				b. Dep. Var.: Number of Events					
Ever Treated	-0.108**	0.253**	0.312	-0.137*		3.135***	-0.039		1.054
	(0.044)	(0.114)	(0.275)	(0.078)		(1.335)	(0.114)		(1.572)
Robust p-value	0.015	0.027	0.113	0.067		0.008	0.635		0.294
Effect Size (%)	-41.95%	10.07%	13.99%	-16.86%		16.00%	-2.78%		2.63%
Bwd.	[0.049;0.049]	[0.041;0.041]	[0.041;0.041]	[0.046;0.046]		[0.045;0.045]	[0.046;0.046]		[0.168;0.168]
Observations	22,536	36,257	36,266	20,776		40,591	3,760		20,307
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

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Online Appendix

The Medium-/Long-run Effects of a Conditional Cash Transfer Program on Early Adulthood Outcomes

Matías Giaccobasso

September 16, 2022

Appendix

A Further Details on the Institutional Background

A.1 Uruguay's Background and Comparison to Other Countries

Table A.1: Uruguay's Background and Comparison to Other Countries

Country	GDP PPP	HDI	HDI Rank	Tax Revenues (% gdp)	Completed Lower Secondary	Adolescent Fertility (per 1000s)	Unemployment rate
Argentina	23,290	0.842	46	28.78	57.16	62.57	9.22
Brazil	15,018	0.762	84	33.08	60.01	57.89	12.33
Chile	25,526	0.849	43	21.07	79.60	40.14	7.23
Colombia	15,044	0.764	83	19.45	54.44	65.48	9.11
Costa Rica	21,319	0.808	61	24.04	53.11	52.52	9.63
Italy	43,036	0.890	29	42.05	78.46	5.11	10.61
Mexico	20,096	0.776	74	16.13	63.26	59.45	3.28
OECD (Avg.)	45,008			34.26		19.73	5.29
Spain	40,696	0.905	25	34.40	77.72	7.49	15.25
Sweden	53,521	0.943	8	43.93	91.07	5.12	6.35
United States	62,805	0.925	17	24.33	96.03	18.56	3.68
Uruguay	23,585	0.816	55	29.17	56.83	58.24	8.34

A.2 Phase I: PANES

PANES was created in April, 2005 by the new center-left government as a response to the economic downturn that affected most Latin American countries in the early 2000s and it remained in place until December, 2007. The main component of this phase was a cash transfer targeted to the poorest 150,000 households in the country.

As most CCTs, *PANES* had two main goals. The first one, more related to a short run critical socio-economic context, was to alleviate the extremely high poverty levels in the country. The overall poverty rate in urban areas for 2005 was 36.6%. Specifically for children, poverty incidence was even more extreme being 61.30% for children between 0-5, 58.00% for children between 6-12, and 49.30% for children between 13-17. The second goal was more related to the medium- and long-run, and consisted of encouraging human capital accumulation in poor households, in particular for the children in those households. In this case, the cash transfer was supposed to ease the way out from poverty circles for many of these households who might had very few resources to dedicate to human capital investment of their children.

PANES was widely publicized, and it rapidly became the most generous anti-poverty program in the country's history up to 2005. The most important component of PANES was the cash transfer, but it was also comprised by other small-sized programs such as temporary public employment programs, education and training for the job market, and other minor interventions such as access to public utilities, building materials, and free dental and eye health care. While 96.7% of the participant households received the cash transfer, less than 20% participated in the remaining components. Hence, *PANES* can be interpreted as mostly a cash transfer program, despite that for a few households it could have represented a wider set of benefits.[42]

The base cash transfer consisted of around USD 133.[43] In addition, a complementary transfer that ranged

[42] See Manacorda et al. (2011) for more details about the program.

[43] In local currency, this corresponded to UYU 1,360. In what follows all income variables are converted to 2008 PPP using CPI and PPP conversion factors.

between USD 29 and USD 78 was provided to households with underage children (70% of the participant households). Overall, the cash transfer represented more than 50% of the average self-reported pre-program income. It is important to note that the income used as a reference to calculate this share is self-reported income. However, since the program also had an income threshold rule to define eligibility, households may have under-reported income to become eligible, and therefore this share must be interpreted as an upper bound. As an alternative reference, in April, 2005 the household per capita poverty line was USD 314.19 for rural areas and USD 469.95 for urban areas, in 2008 PPP terms.

The total number of applications to phase I – that were filled mostly between April, 2005 and May, 2005 - was 187,639, corresponding to 625,208 individuals. This represents about 17% of Uruguayan households and about 20% of the total population. Eligibility to participate on the program was determined based on two criteria. First, applicant households must had a per-capita income below USD 131 PPP. This restriction affected 10% of the applications. Second, households must had a poverty score below an arbitrarily defined threshold that varied by region. Regarding the first requirement, household per-capita income was calculated as the sum of each individual's income divided by the number of household components. Individual's income was defined as the maximum between the self-reported income at the moment of the household interview and the income registered with the Social Security Agency. Regarding the second requirement, households were visited by program officials who conducted a thorough interview to evaluate the socio-economic situation of the household. The information collected in this interview is used to compute a poverty score, which consists of a prediction of the probability of being below a critical per capita income level. If the value of this score was below a certain arbitrarily defined threshold, the household is eligible to receive the cash transfer. The use of a poverty score and an arbitrarily defined threshold to define eligibility to participate in the program was proposed and designed by researchers of the largest university in the country. Next, I list the variables used to calculate the poverty score as well as the regions used to define the eligibility threshold. It is important to note that neither the income nor the poverty score threshold were informed to the participants or publicly revealed. The participants were not informed about their poverty score either.

Predicted income based on probit model using the following covariates:

- Indicator for public employees in the households
- Indicator for pensioners
- Average years of education for individuals over 18 years old
- Number of members of the HH
- Indicator for children between 0-5 and 12 and 17
- Indicator for private health insurance
- Residential overcrowding
- Toilet facilities
- Wealth index based on household durables

Eligibility thresholds were set for five regions:

- Montevideo (capital city)
- North: Artigas, Salto and Rivera
- Center-North: Paysandu, Rio Negro, Tacuarmebo, Duranzo, Treinta y Tres and Cerro Largo
- Center-South: Soriano, Florida, Flores, Lavalleja and Rocha
- South: Colonia, San Jose, Canelones, Maldonado

After accepted, participant households were supposed to satisfy school attendance, regular health checks and per-capita income requirements. However, these conditions were not enforced at all due to administrative constraints. Furthermore, there is no evidence of participants being excluded of the program due to non-compliance with the requirements established by the program. Further details about the model used to calculate the poverty score as well as other details about the program implementation can be found in (Manacorda et al., 2011; Amarante et al., 2016).

A.3 Phase II: AFAM-PE

AFAM-PE is the name given to the program that replaced *PANES* in January 2008. While *PANES* was conceived as a temporary program, *AFAM-PE* was implemented as a part of a larger and more structural bundle of public sector reforms. These reforms included changes to the social safety net structure, but also to the tax structure and health insurance system. Unlike *PANES*, *AFAM-PE* was thought of, and legislated as, a permanent component of the social safety net in the country.

In practice, *AFAM-PE* was implemented as an expansion of *PANES*. The total number of applications – until December, 2017 - was 559,565, corresponding to about 1,349,292 different individuals. This represents a substantial increase compared to the population covered by *PANES* both because the program was intended to be expanded, but also because it was in place for a significantly larger period of time. There were only three differences between PANES and AFAM-PE. The first one is that AFAM-PE established as a requirement for eligibility the presence of underage children in the household, while PANES did not include any restriction related to household composition. The second was that the eligibility poverty score threshold became more lenient, aiming to expand the coverage of the program. Finally, the formula used to define the transfer amount was changed. The new structure established a baseline payment of USD 57 per children from 0-17 but subject to a equivalence scale of 0.6. In addition to the base payment, each household would receive an additional USD 24 per children enrolled in the secondary education system, also subject to an equivalence scale of 0.6.

Conditionalities to remain in the program also remained unenforced during the first years of implementation. However, in April, 2013 the government started to require household to present proof that they were actually fulfilling the requirements. Just as an example, in April, 2013 more than 30,000 children were unenrolled from the program because of non-compliance with the education enrollment requirements. In subsequent years, enforcement strongly depended of who was the person in charge and it was relatively intermittent.

B Data Construction and Methodological Decisions

B.1 Description of Raw Data

Participation data

Participation data contains form-level information about successful and unsuccessful applications to PANES/AFAM-PE between April 2005 and December 2018. Information comes from a detailed socio-demographic questionnaire implemented by program officials to households that applied to the program. The information collected in this questionnaire was used to compute the poverty score that defines the eligibility condition. Table B.1 describes the raw variables included in these administrative records.

Table B.1: Description of Information Contained in Participation Records

Type	**Variable**
Application forms	Application number
	Resolution
	Application data
	Self-reported per-capita household Income
	Poverty score
	Application status
	History of participation
Household Characteristics	Application Number
	Department
	Housing characteristics and quality
	Ownership and value
	Access to utilities
	Appliances
Individual Characteristics	Application and masked national ID number
	Birth date
	Gender
	Education (current level and attendance)
	Activity and occupation status
	Income
Household Roster	Application and Masked national ID numbers
	Relation with household head

Phase I participation records contain information of 187,881 application forms. Of these, 102,436 were accepted applications and 85,291 were rejected. The total number of observations in the individual characteristics dataset is 708,622, which includes repeated individuals. In phase II, the total number of application forms is 639,167. This also includes households that were transferred automatically from phase I to phase II. Of the 639,167 phase II application forms, 499,277 were accepted and 139,890 were rejected. Phase II individual level data contains information of 1,249,466 individuals in the accepted applications data and 392,207 in the rejected data. Since individuals can show up in both datasets, the final number of unique individuals will be smaller.

In the process of cleaning the data some application forms were discarded due to different reasons. First, 190 application forms in phase I are excluded because they cannot be linked to a household or individual level information, are special application forms or do not contain information of the score, place or date in where the application form was submitted. In phase II, I exclude 4,031 forms for similar reasons. In addition, there are 29,101 duplicated phase II forms that show up both in the accepted and rejected applications datasets. Second, since I observe rejected applications only until December, 2017, I drop all accepted applications forms corresponding

to 2018 and 2019 (29,692 forms). In addition, 6,223 applications forms are dropped because they only contain individuals with missing id number, which is the variable used to link participation and other administrative records used to build the outcome variables. Finally, there are some cases where application forms for the same household are almost identical - including the application date - but they differ in some very specific variables. In cases where the only difference is the number of application, I keep one of them randomly and drop 6,922 forms. As to identical application forms with different identification numbers and rejection reason, I collapse the reasons and then drop 743 duplicated forms. Finally, for the remaining same day applications I drop 3,048 forms keeping the application form with the largest score and another 3 forms that have multiple applications the same day with different form number but the same score. The resulting number of forms is 747,134 and the corresponding number of unique individuals is 1,812,495

Birth Records

The birth certificates records come from the Ministry of Health and include the universe of births in the country (780,490) between 2003 and 2019. [44] The variables included in the raw dataset are: birth date, type of institution where the child was born (public, private or others), the age of the mother at the time of birth, birth weigth and gestation weeks. The number of mothers included in the dataset is 473,483 and 60% of them only gave birth to a single child, 29% gave birth two children, 8% gave birth three children and the remaining 3% to 4 or more children.

Education records

Education records come from three different sources: 1) National Council of Secondary Education, 2) National Council of Technical and Professional Education and 3) *Universidad de la Republica*, which is the largest public university in the country (more than 80% of total university enrollment).

Information from the National Council of Education provides enrollment data for 2006-2012, 2014, 2017 and 2018 for middle- and high-school levels in the traditional secondary education system. For 2008-2009, the information provided by the officials is incomplete and total enrollment is about 25% smaller compared to previous and subsequent years. The reason is most likely that an additional filter was used by the officials at the moment of extracting the data, which for these two years is restricted to students enrolled in more recent study plans. In any case, this is unrelated to treatment status or the running variable. If anything it represents measurement error that will bias the estimated treatment effects toward zero.

Information from the National Council of Technical and Professional Education contains enrollment data in vocational schools for the same period of time. In both cases, for 2004-2011 and 2014 there is also additional information about promotion, courses passed and total absences during the school year.

Information from *Universidad de la Republica* contains enrollment data for the universe of students that ever enrolled to any major between 2005 and 2020. In this case, the only information available is whether the student was enrolled, and no additional information about major, progress, performance and completion status is available at this moment.

Labor market records

Labor market records come from the Social Security Agency and contain information about registered employment between 2005-2015. This dataset consists of individual-month-job level information and contains employment

[44] Actually, the original dataset contains information of 780,490 births but I drop 1,127 that correspond to 2002 and 3 which are mislabeled

status, wage/income, and hours worked for all registered employees in the country.

In this case, the one-to-one matching that uses the masked ID number can only be performed for individuals that ever applied or were transferred to phase II of PANES/AFAM-PE. The reason is that the masked ID number required to do this merge is only available for individuals that ever showed up in phase II participation records. Out of the 1,900,000 masked national ID numbers that show up in participation records of PANES/AFAM-PE, 75% of them show up at least once in phase II records. Of these more than 95% have a masked ID number that can be used to link participation records and SSA data.

B.2 Creation of household identifier for Phase I data

Unlike phase II data, phase I only contains application form and personal identifiers, but it does not include a household identification number, which is key to detect which households applied multiple times to the program and to define the first application at the household level. One way to detect if two application forms correspond to the same household is to compare the personal identifiers of the individuals included in the applications. If the two forms include the exact same personal identifiers, they can be attributed to a same household. However, since household composition is dynamic (e.g. a new child could have being born or someone may leave), the process of constructing a household identifier is more challenging. For this reason, I create a phase I household identifier based on the following procedure that establishes a set of rules to define when application forms correspond to the same household. After identifying same-household applications in phase I, the next step is to match the phase I household identifier with phase II household identifiers. This procedure is explained in section B.3

1. Identify individuals individuals who are included in more than one application form:
    - Individuals with the same ID number
    - Individuals with the same name and birth date (some IDs corresponding to recently newborns are missing)
2. Create a list of forms that do not include any individual whose personal identifier shows up in multiple forms. These are the vast majority of forms (91.3%) and represent households with only one phase I application.
3. Check if there are forms with identical composition. 1.55% of the forms matched other forms with identical composition only, and 0.12% had at least one identical match but also some other non-identical match.
4. For the 7.15% forms remaining, I created an algorithm that defines whether two forms correspond to the same household. This algorithm compares each form with all the forms that contain at least one repeated individual and defines that two forms correspond to the same household if at least one of the following conditions hold:
    - Forms have the same household head, and the matching rate of individuals between the forms is larger than 50%
    - Forms have different household head, include two persons or more, and the matching rate of individuals between the forms is larger than 80%
    - Forms have different household head, one is included in the other (i.e., all individuals in the smaller form are included in the larger form) and the matching rate is larger than 60%
    - Forms have different household heads, one is not included in the other but some members intersect, the number of members is 3 or more, and matching rate is larger than 60%
5. After running the algorithm, 8,933 forms satisfied at least one of these rules and 4,493 did not. The latter correspond to individuals that are repeated across forms, but forms cannot be linked to the same household. This could be the case of an individual that left the original household, created a new one and apply to phase I.

B.3 Matching household identifiers between phases I and II

After creating the household identifiers for phase I, I harmonize the household identifiers of phase I and phase II. To do this, I merge the participation data of the two phases using the unique national identification number. For every individual included in the phase II data, I observe a list of phase I household identifiers where the national id number was included. 64.32% of all the national ID numbers observed in the pooled data correspond to phase II data only, while 35.68% show up both in phase I and phase II data. 99.66% of the national ID numbers that are included both in phase I and phase II data are associated to a unique household in phase I data. This is reasonable since phase I only lasted two years. 27.62% of the individuals show up only in phase I data. 99.66% of them are associated to a unique household.

The key challenge to link phase II and phase I households IDs correspond to cases where different individuals in the same phase II household can be linked to different phase I households. The actual percentage of households that are in this situation is very small (1.31% or 4,980 out of 380,040 household ids in phase II). For this 1.31% of the households I implemented the following rules:

1. For households in phase II that merge with multiple households in phase I, I assign the match to the household match that appears the most (of the total number of households in phase II this rule lefts 1,872 remaining cases. Note that this rule includes households with only one match, which as mentioned before are 98.17% of the households)
2. For ties, I assign the match observed for the household head. This rule left 750 households to match
3. In case of ties and when there is not match for a household head, I assign the match observed for the wife/husband/spouse of the household head. After this match, there are 537 households left to match
4. For the remaining cases I pick one of the matches randomly

B.4 Baseline Covariates

Based on the information available in the application form used the first time that a household applied to PANES/AFAM-PE, I create two sets of baseline variables that will be used as control variables to increase precision in the estimates and to test balance on observables in the baseline period: 1) household characteristics, 2) household head characteristics.

Table B.2: Description of Baseline Covariates

Group	**Variable**
Household Characteristics	Number of household members
	Number of children
	Single parent household
	Avg. age of the adults
Household Head Characteristics	Educ. years
	Working status
	Income reported head/partner

C Further Results on Descriptive Statistics

Column (1) reports the information corresponding to all application forms that have ever been filed between April, 2005 and December, 2017. The share of application forms in the full sample that correspond to 2005-2007 - i.e., phase 1 of *PANES/AFAM-PE* - is 25.11%. The average standardized poverty score is 0.21. Because

the standardized score is centered around the eligibility threshold, positive values mean that individuals are eligible. Hence, the average individual in the full sample is an eligible individual. This is also reflected in the average acceptance rate, which is 70.46. 35.58% of the applications correspond to the capital city, which is under-represented compared to the distribution of the whole population. Regarding household structure, the average number of individuals in an application form is 3.92, of whom 1.96 are children. 51.44% of the application forms correspond to single-parent families. The average age of all household members is 24.01. Finally, household heads can be described as mostly low-educated individuals, with an average years of education that is just 0.77 years more than a level of complete primary education. Household heads have also low rates of labor market participation with an average employment rate of 55.40%. The average total household income is USD 313.77, while the average household head income is USD 164.80.

Column (2) reports the summary statistics corresponding to application forms that have at least one individual that belongs to the main sample (i.e., individuals that belong to households who applied for the first time when they were younger than 18 years old, have at least 15 years old by April, 2018, and applied for the first time to the program before between 2005-2012). In general, the overall characteristics of the application forms for this sub-sample are very similar to the full sample. However, there are some differences which are related to the selection criteria. For instance, the average number of individuals is 0.49 larger and this is mostly explained by a larger presence of children. Similarly, the average age of the household members is 2 years younger, compared to the full sample. Mechanically, the share of applications between 2005-2007 is also larger (6p.p.) since the main sample is restricted to applications between 2005-2012. In terms of all the other variables, the forms are very similar between samples. This is also true for Column (3) that describes the characteristics of the forms that contain individuals from the balanced sample.

Table C.1: Descriptive Statistics: Outcome Variables at the Individual Level - By Sample

	Main Sample			Restricted Sample		
	All	$[-0.08; 0]$	$(0; 0.08]$	All	$[-0.165; 0]$	$(0; 0.165]$
	(1)	(2)	(3)	(4)	(5)	(6)
a. Fertility Outcomes						
Birth before 18yo	25.18	22.26	23.22	17.73	15.24	16.03
	(43.40)	(41.60)	(42.23)	(38.19)	(35.95)	(36.69)
Birth before 23yo	59.28	55.08	56.30	61.73	55.66	60.00
	(49.13)	(49.74)	(49.60)	(48.61)	(49.68)	(48.99)
Birth before 30yo	80.46	76.21	79.53	80.46	76.25	80.13
	(39.66)	(42.58)	(40.35)	(39.66)	(42.56)	(39.91)
Number of births before 18yo	0.29	0.25	0.26	0.18	0.16	0.17
	(0.53)	(0.50)	(0.50)	(0.41)	(0.38)	(0.39)
Number of births before 23yo	0.90	0.80	0.83	0.90	0.78	0.84
	(0.93)	(0.89)	(0.89)	(0.89)	(0.85)	(0.85)
Number of births before 23yo	1.57	1.42	1.47	1.57	1.42	1.50
	(1.20)	(1.16)	(1.13)	(1.20)	(1.15)	(1.13)
b. Education Outcomes						
Enrolled sec. educ. before 18yo	62.58	67.94	66.11	38.06	46.05	40.26
	(48.39)	(46.67)	(47.33)	(48.55)	(49.85)	(49.05)
Enrolled tert. educ. before 23yo	7.16	8.96	8.44	3.82	5.99	4.02
	(25.79)	(28.56)	(27.80)	(19.16)	(23.74)	(19.64)
Enrolled tert. educ. before 30yo	4.49	6.91	5.22	4.45	7.01	4.55
	(20.72)	(25.37)	(22.25)	(20.62)	(25.53)	(20.84)
Number of years enrolled sec. educ. by 18yo	2.30	2.58	2.48	0.57	0.70	0.61
	(2.01)	(2.08)	(2.05)	(0.83)	(0.87)	(0.85)
c. Labor Market Outcomes						
Worked four cons. months before 18yo	21.30	19.41	20.57	13.16	12.70	12.68
	(40.94)	(39.55)	(40.42)	(33.81)	(33.30)	(33.27)
Worked four cons. months before 23yo	60.98	58.03	60.82	49.83	47.25	49.62
	(48.78)	(49.35)	(48.82)	(50.00)	(49.93)	(50.00)
Worked four cons. months before 30yo	67.50	62.96	65.75	63.93	59.89	64.21
	(46.84)	(48.29)	(47.46)	(48.02)	(49.01)	(47.94)
Months worked before 18yo	2.39	2.21	2.30	1.41	1.41	1.33
	(4.22)	(4.24)	(4.18)	(3.14)	(3.29)	(2.95)
Months worked before 23yo	20.33	19.69	20.26	14.25	14.16	14.07
	(21.27)	(21.52)	(21.25)	(17.84)	(18.58)	(17.71)
Months worked before 30yo	43.89	43.12	43.42	40.08	39.98	40.72
	(44.23)	(45.56)	(44.55)	(42.87)	(44.53)	(43.08)
Earnings by 18yo (1000s)	1.04	0.98	1.02	0.45	0.46	0.43
	(2.21)	(2.20)	(2.21)	(1.21)	(1.28)	(1.17)
Earnings by 23yo (1000s)	12.83	12.63	13.00	7.12	7.41	7.15
	(17.64)	(17.77)	(17.76)	(11.42)	(12.17)	(11.45)
Earnings by 30yo (1000s)	32.42	33.25	32.78	28.42	29.74	29.44
	(45.51)	(47.89)	(46.49)	(42.18)	(44.90)	(43.78)
Observations	224,413	40,602	35,991	34,754	10,446	10,995

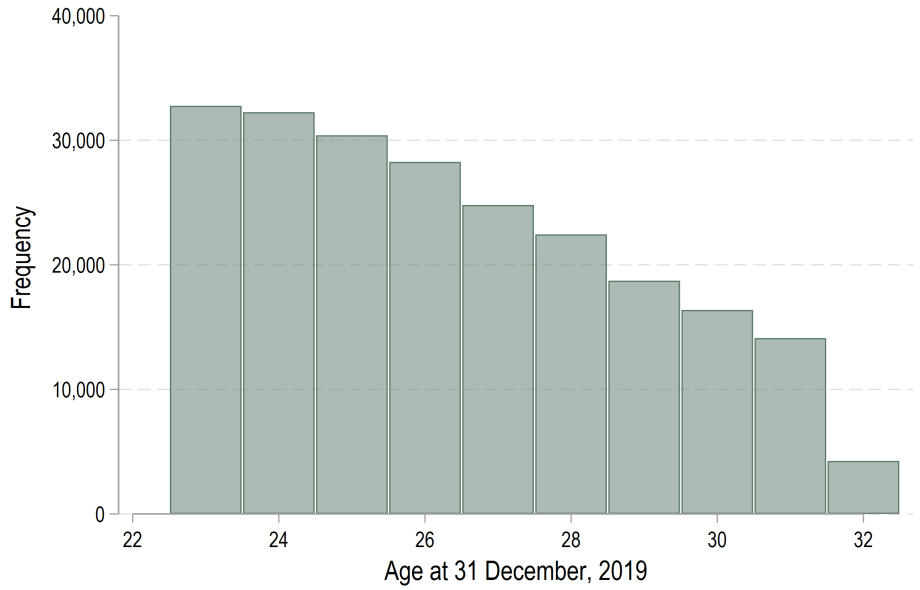
Notes:

Table C.2: Descriptive Statistics at the Application Form Level - By Sample

	All Forms			Reference Forms				
	Full Sample (1)	Main Sample (2)	Restricted Sample (3)	Full Sample (4)	Main Sample (5)	Main Sample Opt. Bdw. (6)	Restricted Sample (7)	Restricted Sample Opt. Bdw. (8)
PANES Form (%)	25.11	32.33	43.39	52.72	70.09	81.80	100.00	100.00
	(43.37)	(46.77)	(49.56)	(49.93)	(45.79)	(38.59)	(0.00)	(0.00)
Std. Score	0.21	0.25	0.27	0.12	0.15	-0.00	0.15	0.01
	(0.23)	(0.22)	(0.23)	(0.22)	(0.23)	(0.05)	(0.26)	(0.08)
Capital City (%)	35.58	32.98	32.37	34.14	31.33	18.46	29.14	21.16
	(47.87)	(47.01)	(46.79)	(47.42)	(46.38)	(38.80)	(45.44)	(40.85)
App. Accepted (%)	70.46	77.80	78.97	59.55	66.98	47.08	68.27	54.30
	(45.62)	(41.56)	(40.75)	(49.08)	(47.03)	(49.92)	(46.54)	(49.82)
Single Parent (%)	51.44	49.31	47.56	50.87	47.76	50.87	48.58	50.25
	(49.98)	(50.00)	(49.94)	(49.99)	(49.95)	(49.99)	(49.98)	(50.00)
Number of members	3.92	4.71	5.10	3.48	4.35	3.92	4.81	4.34
	(1.86)	(1.96)	(2.28)	(1.61)	(1.74)	(1.58)	(2.05)	(1.82)
Number of children	1.96	2.64	2.62	1.63	2.41	2.01	2.77	2.32
	(1.45)	(1.57)	(1.73)	(1.32)	(1.45)	(1.22)	(1.70)	(1.41)
Avg. age	24.01	22.75	22.18	26.85	24.37	26.18	24.58	26.17
	(11.83)	(7.70)	(7.48)	(14.33)	(8.30)	(8.57)	(8.05)	(8.14)
Household Head: Ed. years	6.77	6.52	6.42	7.06	6.89	7.18	6.73	7.01
	(3.26)	(3.25)	(3.24)	(3.49)	(3.43)	(3.55)	(3.49)	(3.54)
Household head: Employed (%)	55.40	58.30	57.94	57.44	62.51	63.65	65.08	65.06
	(49.71)	(49.31)	(49.37)	(49.44)	(48.41)	(48.10)	(47.67)	(47.68)
Household head: income	164.80	141.32	127.83	162.11	147.30	155.83	128.13	140.90
	(240.05)	(182.56)	(161.72)	(246.83)	(182.06)	(173.66)	(140.49)	(147.67)
Total household income	313.77	266.44	267.93	293.26	257.91	269.76	229.82	251.01
	(781.79)	(289.59)	(285.55)	(378.03)	(262.89)	(255.71)	(209.54)	(219.49)
Observations	747,204	265,350	62,734	342,412	126,041	46,827	29,713	18,795

Notes:

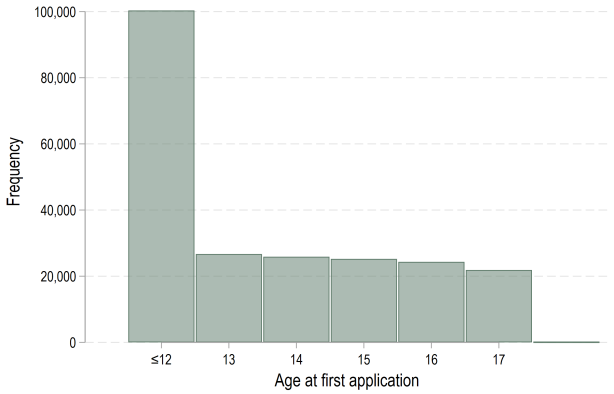
Figure C.1: Distribution of Age at 31 December, 2019



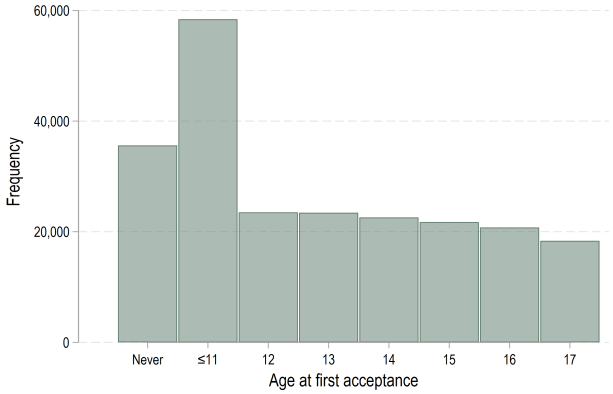
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Figure C.2: Descriptive Statistics - Application and Treatment - Sample 23yo at Dec, 2018

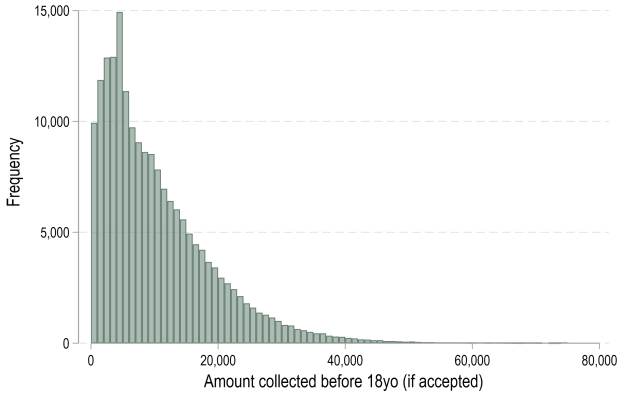
a. Age at first application



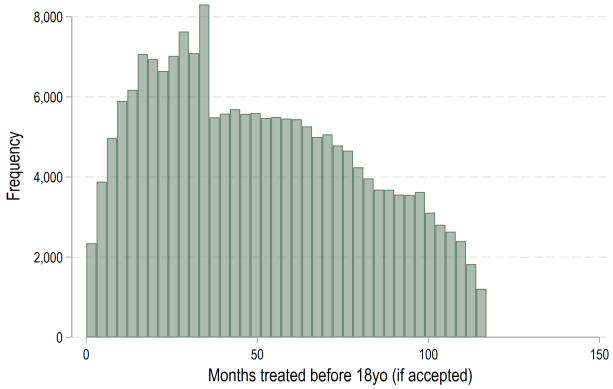
b. Age at first acceptance



c. Amount collected before 18yo



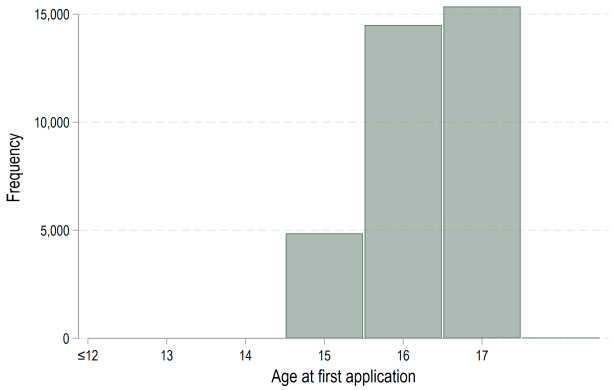
d. Months treated before 18yo



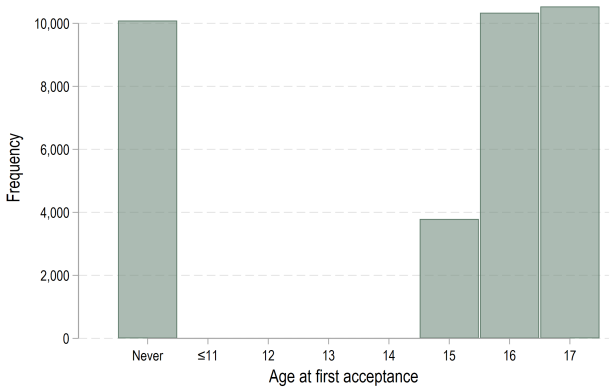
Notes:

Figure C.3: Descriptive Statistics - Application and Treatment - Sample 30yo at Dec, 2018

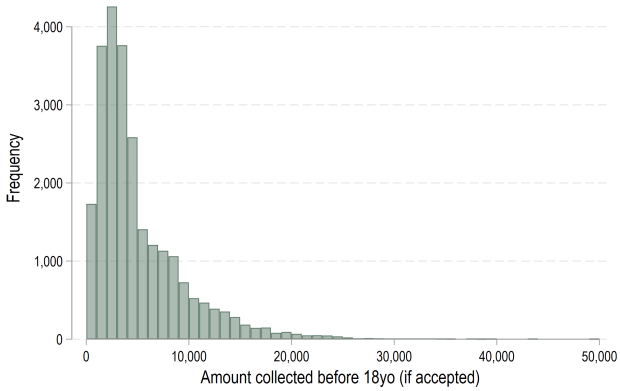
a. Age at first application



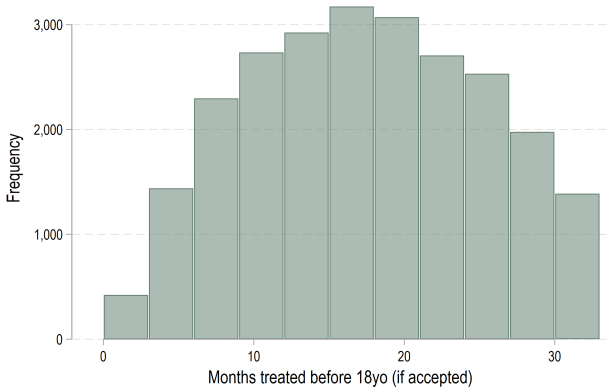
b. Age at first acceptance



c. Amount collected before 18yo



d. Months treated before 18yo



Notes:

D Further Details on Validity of the RDD

Overall, Figure **??** in the main text shows persuasive evidence of an abrupt discontinuity in the treatment variable just at the eligibility threshold. The change in the probability of being ever accepted into the program before turning eighteen years old changes by 50% (29.3p.p.) just at the centered value of the first application poverty score, and this difference is statistically significant at traditional levels ($p - value \leq 0.001$). Table **??** presents the analogous regression estimates. Column (1) reports the baseline estimates using a linear polynomial function and a triangular kernel function, while columns (2) through (4) present sensitivity tests based on alternative polynomial degrees and kernel functions.

A similarly sized discontinuity can be observed if we consider either the number of months treated or the total amount collected variables. For the number of months, individuals who are just to the right of the threshold have been exposed to the program before the age of 18 on average 15 months more compared to individuals just to the right. Expressed as a percentage of the average number of months for individuals to the left of the threshold, this is a difference of 47.38%. If we consider the *amount_treated* variable, the jump observed is of a slightly larger magnitude (58.93%), but qualitatively identical.

Table D.1: First Stage Estimates, Multiple Endogenous Variables - Main Sample

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
	a. Dep. Var: Ever Treated Before 18 Years Old			
Eligibility	0.297***	0.296***	0.293***	0.291***
	(0.009)	(0.011)	(0.009)	(0.010)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	50.08%	51.72%	49.84%	52.37%
Bwd.	[0.026;0.026]	[0.043;0.043]	[0.033;0.033]	[0.054;0.054]
Observations	24,551	40,813	31,413	52,538
	b. Dep. Var: Months Treated Before 18 Years Old			
Eligibility	15.174***	14.829***	15.130***	15.298***
	(0.649)	(0.735)	(0.620)	(0.811)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	55.27%	57.20%	56.67%	58.63%
Bwd.	[0.028;0.028]	[0.049;0.049]	[0.037;0.037]	[0.047;0.047]
Observations	26,893	46,967	35,527	44,592
	c. Dep. Var: Amount (USD 1,000) Received Before 18 Years Old			
Eligibility	3.243***	2.928***	3.291***	3.290***
	(0.136)	(0.145)	(0.136)	(0.171)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	69.19%	67.54%	72.01%	75.24%
Bwd.	[0.029;0.029]	[0.059;0.059]	[0.036;0.036]	[0.048;0.048]
Observations	28,133	57,694	34,028	45,177
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Uniform	Uniform	Triangular	Triangular

Notes:

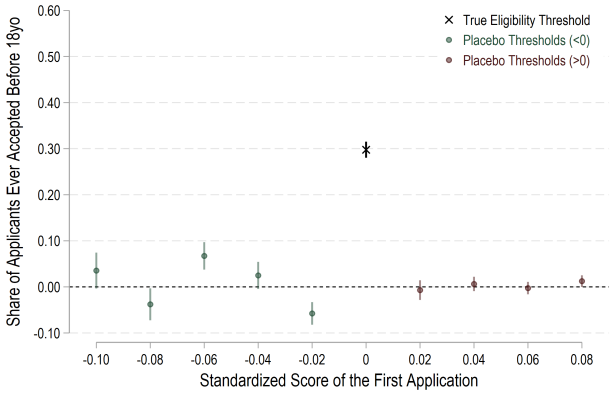
Table D.2: First Stage Estimates - Restricted Sample

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
a. Dep. Var: Ever Treated Before 18 Years Old				
Eligibility	0.607***	0.571***	0.550***	0.592***
	(0.026)	(0.029)	(0.028)	(0.026)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	255.16%	272.47%	231.93%	325.01%
Bwd.	[0.017;0.017]	[0.030;0.030]	[0.017;0.017]	[0.043;0.043]
Observations	3,006	5,341	3,069	7,513
b. Dep. Var: Months Treated Before 18 Years Old				
Eligibility	10.413***	9.855***	9.846***	10.006***
	(0.473)	(0.581)	(0.552)	(0.540)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	294.65%	318.58%	271.73%	370.93%
Bwd.	[0.025;0.025]	[0.038;0.038]	[0.023;0.023]	[0.051;0.051]
Observations	4,377	6,577	4,098	8,886
c. Dep. Var: Amount (USD 1,000) Received Before 18 Years Old				
Eligibility	2.415***	2.382***	2.419***	2.397***
	(0.167)	(0.210)	(0.173)	(0.207)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	253.64%	295.18%	273.41%	300.29%
Bwd.	[0.032;0.032]	[0.045;0.045]	[0.037;0.037]	[0.055;0.055]
Observations	5,633	7,881	6,436	9,966
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Uniform	Uniform	Triangular	Triangular

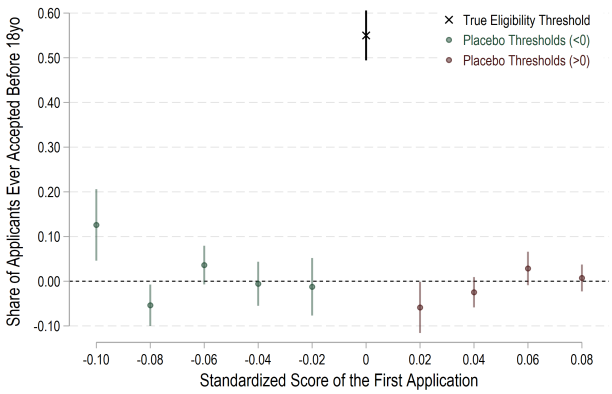
Notes:

Figure D.1: First Stage Falsification Tests

a. Sample 23yo at Dec, 2018



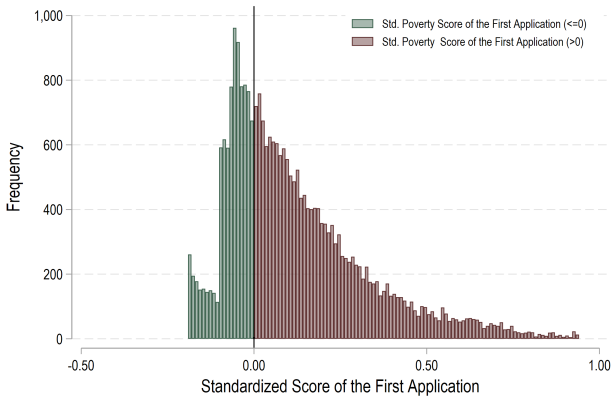
b. Sample 30yo at Dec, 2019



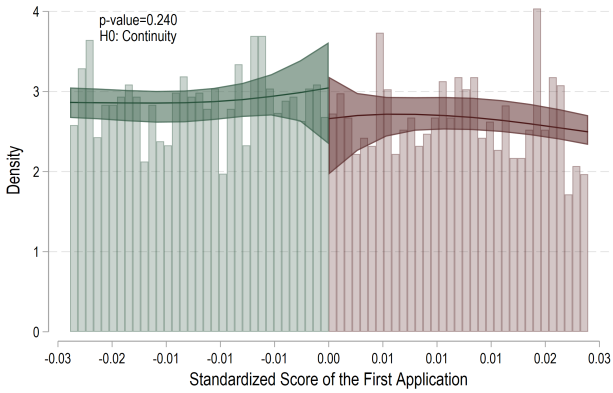
Notes:

Figure D.2: Continuity of the Poverty Score in 1st. Application Form - Restricted Sample

a. Distribution of the Poverty Score



b. Continuity Test



Notes:

E Further Results - Intention to Treat

Table E.1: Intention to Treat Effects, by Age - Estimates With Covariates

	18 years old			23 years old			30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
				a. Dep. Var.: Dummy Variable					
Ever Treated	-0.024**	0.000	0.005	-0.013	0.003	0.028***	0.019	-0.042***	0.018
	(0.010)	(0.008)	(0.007)	(0.011)	(0.005)	(0.010)	(0.021)	(0.013)	(0.018)
Robust p-value	0.017	0.867	0.366	0.227	0.758	0.003	0.465	0.001	0.281
Effect Size (%)	-10.56%	0.05%	2.80%	-2.30%	3.18%	4.88%	2.54%	-62.63%	2.96%
Bwd.	[0.052;0.052]	[0.059;0.059]	[0.050;0.050]	[0.056;0.056]	[0.058;0.058]	[0.038;0.038]	[0.078;0.078]	[0.032;0.032]	[0.069;0.069]
Observations	23,877	48,438	45,654	26,519	53,777	33,859	6,491	5,330	11,644
				b. Dep. Var.: Number of Events					
Ever Treated	-0.028**	0.075**	0.061	-0.033*		1.065***	-0.023		3.283**
	(0.012)	(0.034)	(0.074)	(0.019)		(0.423)	(0.072)		(1.712)
Robust p-value	0.019	0.032	0.370	0.073		0.009	0.553		0.038
Effect Size (%)	-10.88%	2.99%	2.79%	-4.11%		5.44%	-1.65%		8.41%
Bwd.	[0.055;0.055]	[0.043;0.043]	[0.051;0.051]	[0.061;0.061]		[0.042;0.042]	[0.049;0.049]		[0.064;0.064]
Observations	26,074	38,012	46,198	28,857		37,007	4,118		10,578
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table E.2: Intention to Treat Effects, by Age - Estimates With Covariates - At least 30 years old at 31 December, 2019

	18 years old			23 years old			30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
				a. Dep. Var.: Dummy Variable					
Ever Treated	-0.066***	0.038*	0.017	-0.030	-0.020*	0.033*	0.011	-0.033***	0.016
	(0.024)	(0.023)	(0.014)	(0.032)	(0.012)	(0.019)	(0.022)	(0.012)	(0.020)
Robust p-value	0.005	0.062	0.346	0.248	0.075	0.087	0.822	0.008	0.559
Effect Size (%)	-42.82%	8.33%	14.29%	-5.41%	-35.28%	7.24%	1.49%	-48.76%	2.68%
Bwd.	[0.044;0.044]	[0.046;0.046]	[0.051;0.051]	[0.051;0.051]	[0.033;0.033]	[0.070;0.070]	[0.071;0.071]	[0.036;0.036]	[0.059;0.059]
Observations	3,888	7,956	8,998	4,622	5,828	12,479	6,504	6,247	10,623
				b. Dep. Var.: Number of Events					
Ever Treated	-0.065***	0.086**	0.008	-0.090*		0.847	-0.041		2.858*
	(0.025)	(0.045)	(0.113)	(0.055)		(0.668)	(0.068)		(1.715)
Robust p-value	0.009	0.049	0.915	0.066		0.205	0.381		0.081
Effect Size (%)	-40.66%	12.57%	0.57%	-11.51%		6.28%	-2.96%		7.32%
Bwd.	[0.046;0.046]	[0.036;0.036]	[0.064;0.064]	[0.050;0.050]		[0.070;0.070]	[0.057;0.057]		[0.064;0.064]
Observations	4,034	6,292	11,422	4,527		12,448	5,245		11,292
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

F Further Results - LATE

Table F.1: LATE Effects, by Age - Estimates With Covariates- Balanced 30

	≤ 18 years old			≤ 23 years old			≤ 30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
a. Dep. Var.: Dummy Variable									
Ever Treated	-0.117**	-0.003	0.025**	-0.029	-0.010	0.022*	0.043	-0.018**	0.016
	(0.046)	(0.025)	(0.014)	(0.038)	(0.008)	(0.018)	(0.045)	(0.009)	(0.018)
Robust p-value	0.011	0.858	0.028	0.377	0.244	0.094	0.288	0.031	0.292
Effect Size (%)	-72.92%	-0.66%	20.09%	-5.17%	-16.59%	4.63%	5.62%	-25.17%	2.60%
Bwd.	[0.031;0.031]	[0.079;0.079]	[0.111;0.111]	[0.069;0.069]	[0.186;0.186]	[0.162;0.162]	[0.040;0.040]	[0.165;0.165]	[0.163;0.163]
Observations	2,599	12,858	16,506	5,915	21,563	19,900	3,343	20,098	19,976
b. Dep. Var.: Number of Events									
Ever Treated	-0.123**	-0.025	0.079	-0.136		0.273	-0.039		1.054
	(0.049)	(0.031)	(0.117)	(0.100)		(0.636)	(0.114)		(1.572)
Robust p-value	0.013	0.273	0.212	0.236		0.255	0.635		0.294
Effect Size (%)	-73.26%	-3.52%	5.70%	-16.92%		1.93%	-2.78%		2.63%
Bwd.	[0.031;0.031]	[0.152;0.152]	[0.139;0.139]	[0.036;0.036]		[0.165;0.165]	[0.046;0.046]		[0.168;0.168]
Observations	2,586	19,285	18,467	2,985		20,134	3,760		20,307
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.2: LATE Effects, by Age - Estimates Using Years Treated as Endogenous Variable

	≤ 18 years old			≤ 23 years old			≤ 30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
a. Dep. Var.: Dummy Variable									
Ever Treated	-0.023**	0.003	0.009	-0.012	0.002	0.023***	0.029	-0.026***	0.019
	(0.009)	(0.008)	(0.007)	(0.011)	(0.004)	(0.008)	(0.032)	(0.009)	(0.017)
Robust p-value	0.018	0.615	0.118	0.203	0.873	0.004	0.357	0.006	0.239
Avg. Treatment	3.75	3.77	3.75	3.75	3.81	3.77	1.29	1.32	1.32
Effect Size (%)	-37.00%	1.54%	17.83%	-8.39%	8.07%	14.79%	4.96%	-49.92%	4.20%
Bwd.	[0.042;0.042]	[0.038;0.038]	[0.032;0.032]	[0.043;0.043]	[0.052;0.052]	[0.036;0.036]	[0.042;0.042]	[0.072;0.072]	[0.088;0.088]
Robust p-value	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
b. Dep. Var.: Number of Events									
Ever Treated	-0.027**	0.061**	0.138*	-0.034*		1.342***	-0.043		2.834*
	(0.011)	(0.028)	(0.079)	(0.019)		(0.406)	(0.096)		(1.696)
Robust p-value	0.015	0.047	0.054	0.075		0.001	0.564		0.070
Avg. Treatment	3.76	3.78	3.75	3.75		3.75	1.29		1.32
Effect Size (%)	-39.58%	9.19%	22.86%	-15.38%		25.79%	-3.94%		9.51%
Bwd.	[0.044;0.044]	[0.038;0.038]	[0.029;0.029]	[0.042;0.042]		[0.029;0.029]	[0.035;0.035]		[0.072;0.072]
Observations	19,946	34,264	26,485	19,022		26,033	2,954		11,918
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

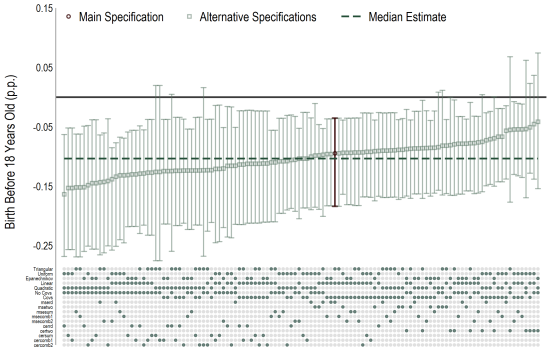
Table F.3: LATE Effects, by Age - Estimates Using NPV of Total Amount Collected (USD 10,000) Treated as Endogenous Variable

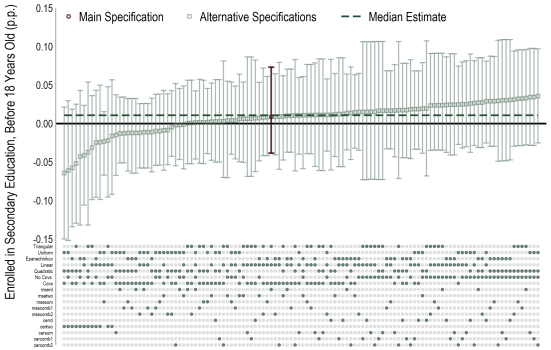
	≤ 18 years old			≤ 23 years old			≤ 30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
				a. Dep. Var.: Dummy Variable					
Ever Treated	-0.066**	0.012	0.033	-0.030	0.001	0.094***	0.107	-0.064**	0.064
	(0.028)	(0.030)	(0.025)	(0.030)	(0.017)	(0.032)	(0.111)	(0.029)	(0.057)
Robust p-value	0.016	0.599	0.137	0.292	0.829	0.003	0.307	0.023	0.221
Avg. Treatment	0.90	0.86	0.86	0.90	0.87	0.86	0.39	0.40	0.40
Effect Size (%)	-26.37%	1.49%	14.72%	-4.93%	1.26%	13.94%	5.52%	-35.72%	4.31%
Bwd.	[0.053;0.053]	[0.035;0.035]	[0.033;0.033]	[0.063;0.063]	[0.043;0.043]	[0.030;0.030]	[0.037;0.037]	[0.095;0.095]	[0.096;0.096]
Robust p-value	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
				b. Dep. Var.: Number of Events					
Ever Treated	-0.073**	0.222**	0.417*	-0.117*		2.867**	-0.135		8.046*
	(0.032)	(0.100)	(0.275)	(0.065)		(1.195)	(0.305)		(5.125)
Robust p-value	0.018	0.018	0.089	0.061		0.020	0.566		0.087
Avg. Treatment	0.90	0.87	0.86	0.89		0.87	0.39		0.40
Effect Size (%)	-25.85%	7.69%	16.06%	-12.63%		12.72%	-3.76%		8.07%
Bwd.	[0.059;0.059]	[0.041;0.041]	[0.032;0.032]	[0.042;0.042]		[0.044;0.044]	[0.036;0.036]		[0.097;0.097]
Observations	27,866	36,342	29,083	19,043		39,692	2,994		15,320
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

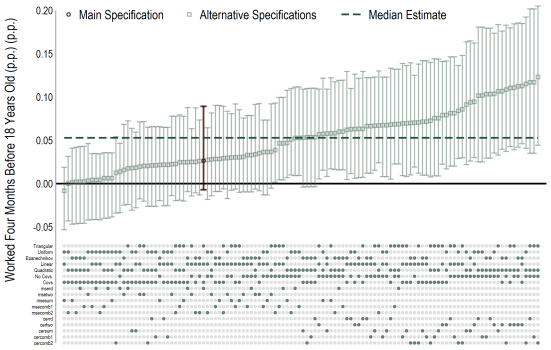
Notes:

Table F.4: Specification Curves - LATE Effects

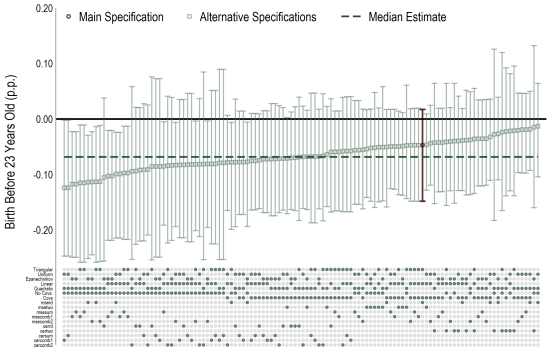
a. 18 years old

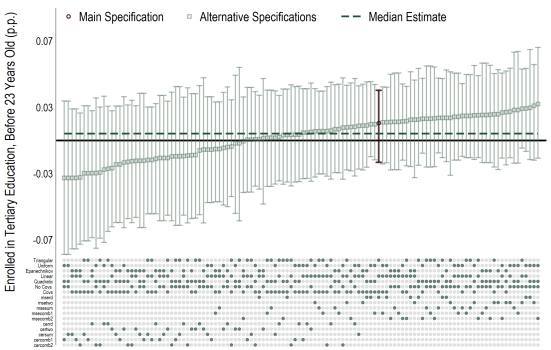


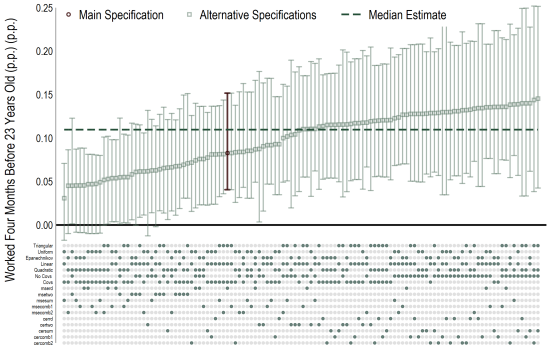




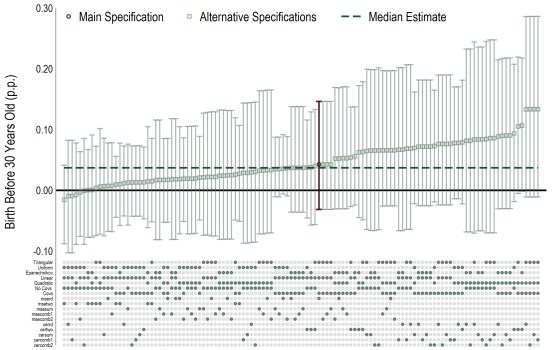
b. 23 years old

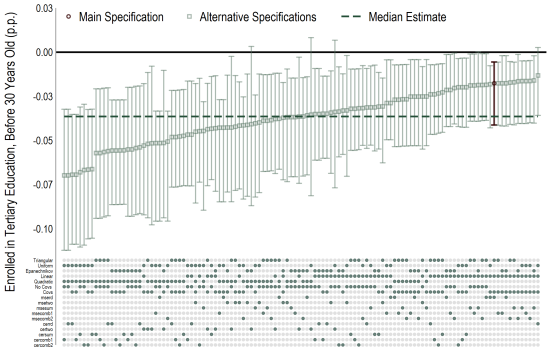


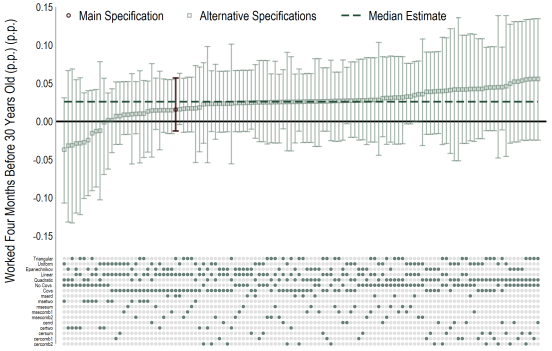




c. 30 years old







Notes:

Table F.5: LATE Effects, by Age - Estimates With Covariates - Male

	18 years old		24 years old		30 years old	
	Education	Labor	Education	Labor	Education	Labor
			a. Dep. Var.: Dummy Variable			
Ever Treated	0.021	0.005	-0.009	0.039	-0.038**	0.030
	(0.040)	(0.036)	(0.017)	(0.031)	(0.017)	(0.036)
Robust p-value	0.662	0.598	0.592	0.227	0.013	0.433
Effect Size (%)	3.09%	2.77%	-9.84%	6.70%	-55.95%	5.06%
Bwd.	[0.050;0.050]	[0.053;0.053]	[0.072;0.072]	[0.082;0.082]	[0.060;0.060]	[0.076;0.076]
Observations	19,681	23,691	32,574	36,608	4,870	6,028
			b. Dep. Var.: Number of Events			
Ever Treated	0.280**	-0.027		-0.078		2.590
	(0.135)	(0.418)		(1.527)		(3.590)
Robust p-value	0.031	0.946		0.794		0.378
Effect Size (%)	10.89%	-1.22%		-0.40%		6.54%
Bwd.	[0.061;0.061]	[0.048;0.048]		[0.080;0.080]		[0.074;0.074]
Observations	27,878	21,102		35,789		5,919
Parameter Selection:						
Pol. Degree	1	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.6: LATE Effects, by Age - Estimates With Covariates - Female

	18 years old			24 years old			30 years old		
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Education	Labor
				a. Dep. Var.: Dummy Variable					
Ever Treated	-0.094**	-0.028	0.028	-0.047	0.021	0.081*	0.043	-0.065**	0.069
	(0.039)	(0.032)	(0.035)	(0.042)	(0.031)	(0.046)	(0.045)	(0.031)	(0.055)
Robust p-value	0.015	0.331	0.486	0.193	0.646	0.081	0.288	0.032	0.170
Effect Size (%)	-41.19%	-4.10%	14.39%	-8.52%	24.54%	13.86%	5.62%	-95.77%	11.63%
Bwd.	[0.044;0.044]	[0.085;0.085]	[0.045;0.045]	[0.050;0.050]	[0.048;0.048]	[0.048;0.048]	[0.040;0.040]	[0.039;0.039]	[0.037;0.037]
Observations	20,033	34,414	20,218	22,711	22,073	22,068	3,343	3,227	3,100
				b. Dep. Var.: Number of Events					
Ever Treated	-0.108**	0.216	0.496	-0.137*		4.268**	-0.039		8.407*
	(0.044)	(0.166)	(0.343)	(0.078)		(1.906)	(0.114)		(4.752)
Robust p-value	0.015	0.231	0.103	0.067		0.014	0.635		0.082
Effect Size (%)	-41.95%	8.60%	22.36%	-16.86%		21.75%	-2.78%		21.83%
Bwd.	[0.049;0.049]	[0.045;0.045]	[0.046;0.046]	[0.046;0.046]		[0.046;0.046]	[0.046;0.046]		[0.037;0.037]
Observations	22,536	20,375	20,890	20,776		20,772	3,760		3,032
Parameter Selection:									
Pol. Degree	1	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

G Further Results - Dynamic Effects

Table G.1: Full LATE Effects, by Age - Fertility

	Women			
	Estimate	Std. Error	Robust p-value	Eff. Size (%)
At or before age:				
14	0.00	(0.007)	0.495	50.36%
15	-0.01	(0.014)	0.509	-30.14%
16	-0.04	(0.026)	0.150	-47.77%
17	-0.05*	(0.027)	0.066	-35.60%
18	-0.09**	(0.037)	0.011	-44.41%
19	-0.03	(0.040)	0.405	-10.80%
20	-0.04	(0.045)	0.247	-11.84%
21	-0.05	(0.043)	0.220	-10.65%
22	-0.02	(0.039)	0.534	-3.69%
23	-0.05	(0.042)	0.193	-8.52%
24	-0.06	(0.040)	0.148	-9.49%
25	-0.04	(0.041)	0.284	-6.72%
26	0.00	(0.041)	0.853	0.46%
27	0.03	(0.030)	0.404	4.12%
28	0.05	(0.040)	0.206	6.75%
29	0.06	(0.042)	0.119	7.62%
30	0.04	(0.045)	0.288	5.62%

Notes:

Table G.2: Full LATE Effects, by Age - Number of Births

	Women			
	Estimate	Std. Error	Robust p-value	Eff. Size (%)
At or before age				
14	0.00	(0.007)	0.580	37.39%
15	-0.01	(0.015)	0.418	-37.14%
16	-0.04*	(0.026)	0.096	-53.38%
17	-0.08**	(0.032)	0.015	-51.78%
18	-0.12***	(0.045)	0.007	-51.24%
19	-0.06	(0.051)	0.251	-16.45%
20	-0.09	(0.062)	0.105	-20.49%
21	-0.08	(0.065)	0.166	-14.38%
22	-0.10	(0.071)	0.125	-14.05%
23	-0.14*	(0.078)	0.067	-16.86%
24	-0.17**	(0.079)	0.031	-18.60%
25	-0.14	(0.087)	0.120	-13.27%
26	-0.02	(0.095)	0.799	-2.21%
27	0.03	(0.086)	0.952	2.37%
28	0.02	(0.099)	0.878	1.72%
29	0.00	(0.109)	0.980	0.10%
30	-0.04	(0.114)	0.635	-2.78%

Notes:

Table G.3: Full LATE Effects, by Age - Secondary Education Enrollment

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
12	0.04	(0.048)	0.428	9.40%	0.05	(0.050)	0.326	9.27%	-0.01	0.898
13	0.10*	(0.052)	0.079	15.68%	-0.02	(0.031)	0.594	-3.18%	0.13	0.038
14	0.02	(0.044)	0.898	2.80%	0.00	(0.045)	0.814	0.09%	0.02	0.754
15	-0.00	(0.040)	0.761	-0.33%	-0.08*	(0.045)	0.094	-9.21%	0.07	0.229
16	0.07	(0.038)	0.105	8.67%	0.06	(0.044)	0.202	7.03%	0.01	0.860
17	0.02	(0.038)	0.648	2.14%	-0.07	(0.055)	0.207	-8.84%	0.09	0.203
18	0.02	(0.037)	0.529	3.16%	-0.02	(0.037)	0.678	-2.67%	0.04	0.418
19	0.03	(0.035)	0.365	4.72%	0.01	(0.041)	0.737	1.14%	0.02	0.648
20	0.04	(0.036)	0.246	5.98%	0.03	(0.034)	0.242	3.95%	0.01	0.804
21	0.07**	(0.034)	0.033	10.20%	-0.02	(0.044)	0.552	-3.44%	0.09	0.092
22	0.06	(0.044)	0.157	9.58%	0.02	(0.041)	0.662	2.50%	0.04	0.467
23	-0.00	(0.043)	0.977	-0.42%	0.09**	(0.044)	0.038	12.28%	-0.09	0.155

Notes:

Table G.4: Full LATE Effects, by Age - Years Enrolled in Secondary Education

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
12	0.03	(0.042)	0.636	6.34%	0.06	(0.042)	0.124	11.37%	-0.03	0.594
13	0.08	(0.073)	0.344	8.76%	0.02	(0.068)	0.665	2.00%	0.06	0.562
14	0.04	(0.098)	0.809	3.22%	0.02	(0.103)	0.953	1.23%	0.03	0.858
15	0.04	(0.119)	0.967	2.17%	0.01	(0.126)	0.826	0.62%	0.03	0.877
16	0.20	(0.138)	0.212	9.45%	0.08	(0.156)	0.787	3.10%	0.13	0.541
17	0.32	(0.176)	0.113	13.66%	0.12	(0.190)	0.735	4.55%	0.20	0.444
18	0.33**	(0.146)	0.033	13.72%	-0.01	(0.172)	0.782	-0.41%	0.34	0.130
19	0.36**	(0.154)	0.026	14.40%	-0.00	(0.180)	0.777	-0.14%	0.37	0.120
20	0.41**	(0.165)	0.019	15.94%	0.01	(0.187)	0.859	0.42%	0.40	0.107
21	0.37**	(0.171)	0.035	14.16%	0.06	(0.191)	0.916	1.82%	0.31	0.220
22	0.34*	(0.169)	0.058	12.90%	0.17	(0.195)	0.486	5.60%	0.16	0.525
23	0.22	(0.168)	0.233	8.69%	0.16	(0.197)	0.489	5.35%	0.06	0.819

Notes:

Table G.5: Full LATE Effects, by Age - Tertiary Education Enrollment

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
17	0.00	(0.006)	0.445	65.56%	-0.01	(0.013)	0.226	-95.53%	0.02	0.204
18	0.00	(0.014)	0.802	1.61%	0.03	(0.025)	0.333	34.08%	-0.03	0.371
19	-0.01	(0.017)	0.564	-19.81%	0.02	(0.031)	0.657	16.10%	-0.03	0.458
20	-0.01	(0.018)	0.815	-12.32%	-0.01	(0.031)	0.566	-9.22%	0.00	0.926
21	-0.01	(0.018)	0.693	-22.95%	0.04	(0.030)	0.395	32.29%	-0.05	0.142
22	-0.02	(0.016)	0.244	-34.18%	0.01	(0.030)	0.850	11.15%	-0.03	0.313
23	-0.01	(0.015)	0.499	-14.64%	0.02	(0.032)	0.644	18.57%	-0.03	0.387
24	-0.01	(0.020)	0.709	-12.43%	0.02	(0.032)	0.808	13.88%	-0.02	0.537
25	0.01	(0.016)	0.755	11.75%	-0.01	(0.032)	0.542	-9.52%	0.02	0.633
26	0.00	(0.015)	0.969	5.42%	-0.03	(0.032)	0.141	-30.88%	0.04	0.318
27	0.00	(0.014)	0.880	0.05%	0.00	(0.024)	0.651	1.48%	-0.00	0.956
28	-0.00	(0.015)	0.792	-8.32%	-0.06**	(0.031)	0.036	-55.89%	0.05	0.134
29	-0.00	(0.018)	0.655	-10.03%	-0.05*	(0.030)	0.091	-52.27%	0.04	0.225
30	-0.04**	(0.016)	0.014	-84.07%	-0.06**	(0.033)	0.049	-73.24%	0.03	0.443

Notes:

Table G.6: Full LATE Effects, by Age - Labor Market Participation

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
14	-0.00	(0.001)	0.383	-366.27%	0.00	(0.001)	0.283	.%	-0.00	0.162
15	-0.00	(0.005)	0.286	-93.77%	0.00	(0.002)	0.565	1,069.81%	-0.00	0.348
16	0.00	(0.009)	0.790	11.78%	0.00	(0.008)	0.786	77.07%	-0.00	0.882
17	-0.01	(0.014)	0.734	-18.06%	0.03	(0.015)	0.111	158.07%	-0.03	0.122
18	-0.02	(0.029)	0.439	-10.78%	0.07**	(0.033)	0.039	58.95%	-0.09	0.031
19	-0.01	(0.033)	0.762	-3.88%	0.11***	(0.042)	0.007	46.68%	-0.12	0.026
20	0.03	(0.039)	0.337	7.59%	0.10**	(0.049)	0.022	31.34%	-0.07	0.286
21	0.05	(0.041)	0.142	10.17%	0.06	(0.052)	0.190	14.49%	-0.01	0.938
22	0.06	(0.039)	0.118	9.59%	0.06	(0.050)	0.242	11.58%	0.00	0.991
23	0.03	(0.035)	0.341	4.93%	0.07*	(0.047)	0.094	13.99%	-0.04	0.446
24	0.01	(0.033)	0.600	2.17%	0.05	(0.047)	0.167	8.68%	-0.04	0.523
25	0.05	(0.038)	0.235	7.61%	0.05	(0.047)	0.147	7.82%	0.00	0.972
26	0.04	(0.036)	0.348	6.67%	0.05	(0.045)	0.144	8.61%	-0.01	0.849
27	0.03	(0.030)	0.410	5.20%	0.07	(0.046)	0.116	10.65%	-0.03	0.527
28	0.04	(0.033)	0.284	5.99%	0.09**	(0.046)	0.039	14.41%	-0.05	0.339
29	0.05	(0.042)	0.266	8.25%	0.03	(0.030)	0.498	5.07%	0.02	0.705
30	0.04	(0.041)	0.356	6.71%	0.03	(0.044)	0.428	4.29%	0.01	0.822

Notes:

Table G.7: Full LATE Effects, by Age - Months Worked

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
14	-0.01	(0.009)	0.200	-389.37%	0.00	(0.004)	0.239	1,383.49%	-0.01	0.118
15	-0.05	(0.043)	0.120	-128.66%	0.03	(0.017)	0.184	629.89%	-0.08	0.103
16	-0.07	(0.089)	0.461	-44.63%	0.10	(0.071)	0.217	215.66%	-0.17	0.140
17	-0.03	(0.169)	0.986	-6.88%	0.36**	(0.174)	0.044	180.23%	-0.39	0.103
18	-0.18	(0.330)	0.700	-7.02%	0.74**	(0.336)	0.023	55.23%	-0.92	0.050
19	-0.23	(0.555)	0.780	-4.15%	1.34**	(0.619)	0.014	41.22%	-1.57	0.059
20	0.35	(0.894)	0.624	3.79%	2.02**	(0.986)	0.020	35.13%	-1.67	0.210
21	1.22	(1.262)	0.246	9.05%	2.87**	(1.399)	0.025	32.69%	-1.65	0.381
22	1.64	(1.582)	0.175	9.08%	4.04**	(1.741)	0.012	33.41%	-2.40	0.307
23	1.26	(1.764)	0.332	5.56%	5.87***	(2.008)	0.002	37.19%	-4.61	0.084
24	0.31	(2.051)	0.737	1.12%	5.88***	(2.255)	0.004	29.82%	-5.57	0.068
25	-1.21	(2.286)	0.705	-3.78%	7.12***	(2.585)	0.003	30.16%	-8.33	0.016
26	-0.23	(2.276)	0.962	-0.61%	5.51**	(2.921)	0.036	20.07%	-5.74	0.121
27	-0.33	(2.414)	0.886	-0.81%	7.03**	(3.280)	0.019	22.91%	-7.36	0.071
28	1.40	(2.797)	0.495	3.20%	6.66*	(3.683)	0.052	19.76%	-5.27	0.255
29	2.01	(3.982)	0.622	4.41%	2.41	(2.655)	0.352	6.62%	-0.40	0.934
30	3.01	(4.136)	0.334	6.31%	4.46	(3.994)	0.179	11.75%	-1.46	0.800

Notes:

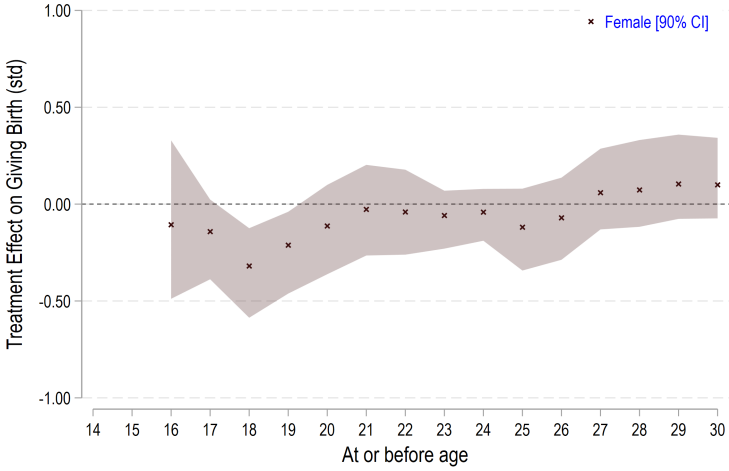
Table G.8: Full LATE Effects, by Age - Cumulative Earnings

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
14	-4.47	(5.788)	0.389	-301.25%	1.39	(1.306)	0.265	2,143.79%	-5.86	0.324
15	-19.72	(20.364)	0.259	-126.61%	11.33	(7.118)	0.183	1,019.03%	-31.05	0.150
16	-16.34	(41.249)	0.789	-24.50%	42.06	(26.741)	0.149	317.12%	-58.39	0.235
17	40.76	(76.825)	0.452	22.59%	138.03**	(66.408)	0.045	209.70%	-97.27	0.338
18	-141.21	(190.700)	0.551	-11.39%	241.95	(158.869)	0.126	44.48%	-383.16	0.123
19	-135.17	(380.161)	0.808	-4.51%	401.49	(335.445)	0.149	27.21%	-536.66	0.290
20	21.58	(671.997)	0.859	0.39%	619.45	(589.547)	0.191	21.98%	-597.87	0.504
21	638.88	(1,013.266)	0.428	7.54%	1,109.43	(886.138)	0.155	24.19%	-470.55	0.727
22	963.74	(1,358.454)	0.314	8.05%	1,983.76*	(1,157.680)	0.063	29.65%	-1,020.02	0.568
23	116.03	(1,574.929)	0.832	0.74%	3,250.82**	(1,438.767)	0.014	35.25%	-3,134.79	0.142
24	-1,574.10	(1,747.196)	0.544	-7.91%	3,010.47*	(1,760.158)	0.063	25.06%	-4,584.57	0.065
25	-2,852.92	(2,372.923)	0.263	-11.85%	4,249.10**	(2,148.972)	0.033	28.41%	-7,102.02	0.027
26	-2,245.44	(2,643.075)	0.426	-7.97%	2,516.38	(2,569.713)	0.285	13.98%	-4,761.82	0.196
27	-2,692.45	(2,736.919)	0.288	-8.36%	3,622.67	(2,993.000)	0.190	17.43%	-6,315.12	0.119
28	-1,637.99	(3,181.089)	0.666	-4.62%	3,038.67	(3,423.452)	0.316	13.00%	-4,676.67	0.317
29	-785.22	(4,392.190)	0.891	-2.10%	758.92	(2,507.189)	0.917	2.99%	-1,544.13	0.760
30	3,129.38	(4,539.250)	0.303	7.87%	-228.68	(4,020.859)	0.862	-0.86%	3,358.06	0.580

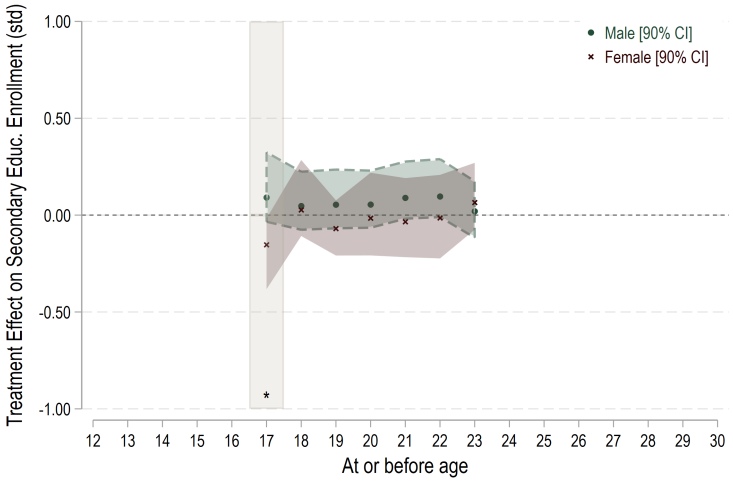
Notes:

Figure G.1: Dynamic Effects, by outcome and gender - Binary Variable - Balanced Sample

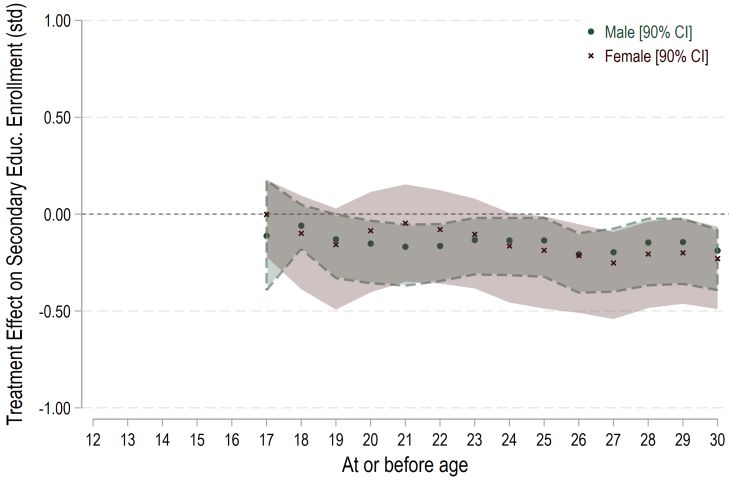
a. Fertility



b. Secondary Education



c. Tertiary Education



d. Labor Market Participation

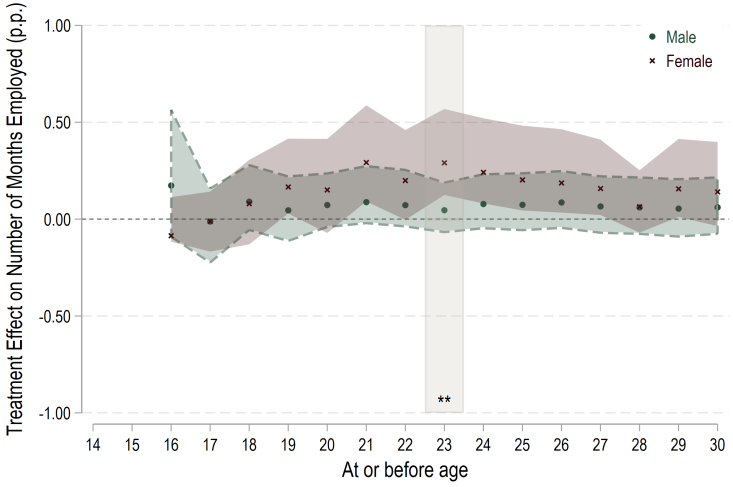
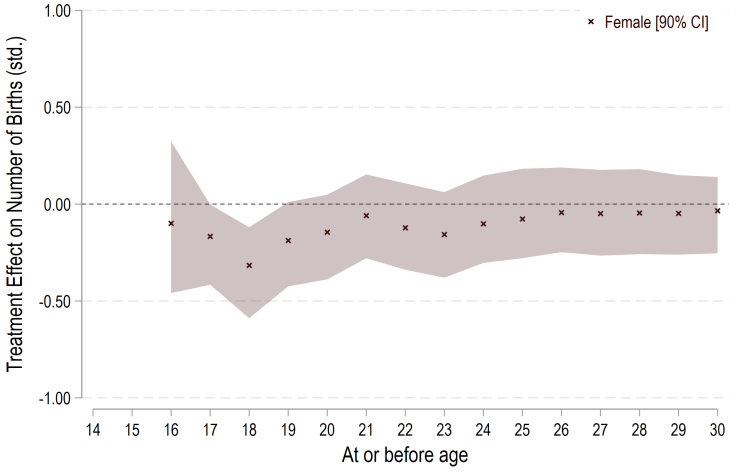
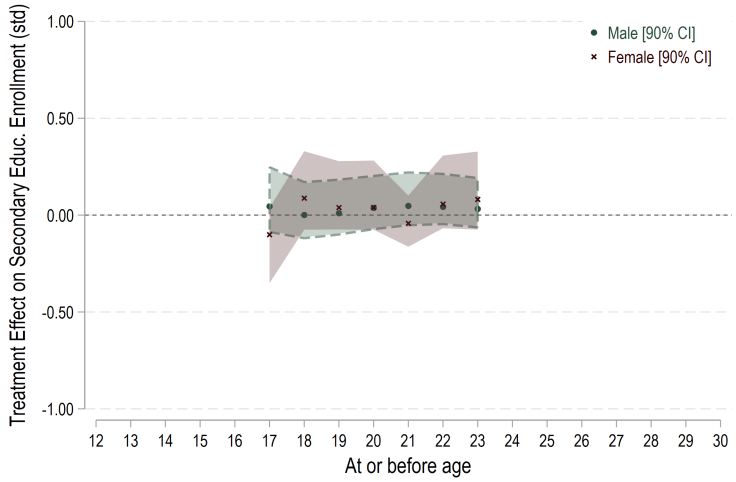


Figure G.2: Dynamic Effects, by outcome and gender - Continuous Variable - Balanced Sample

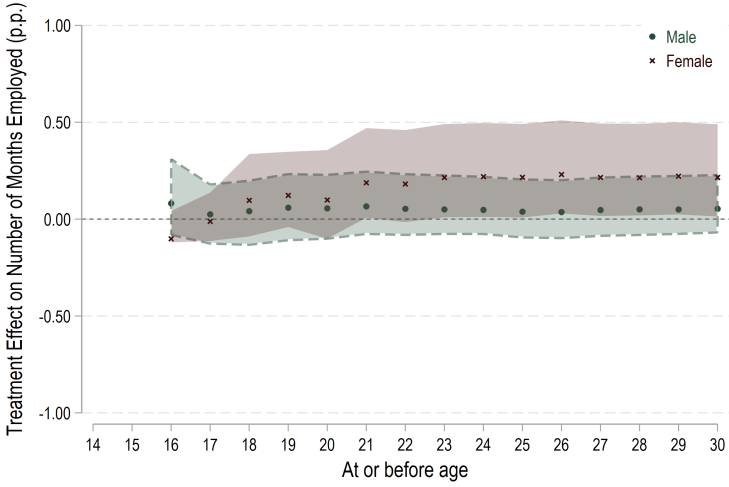
a. Fertility



b. Secondary Education



c. Cumulative Months Worked



c. Cumulative Earnings

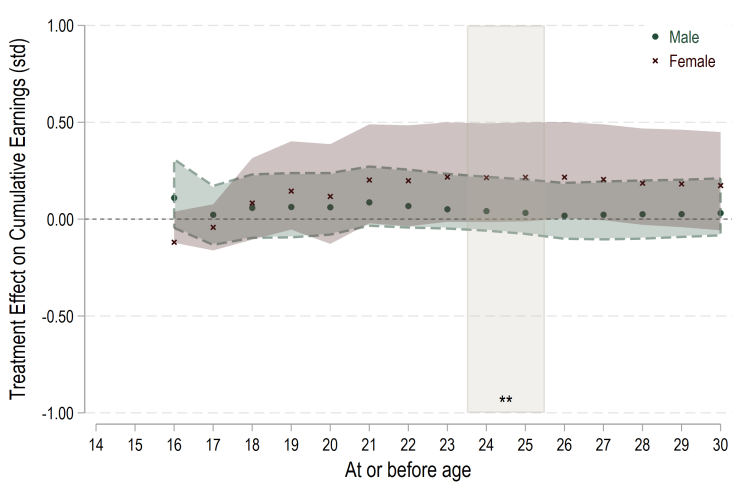
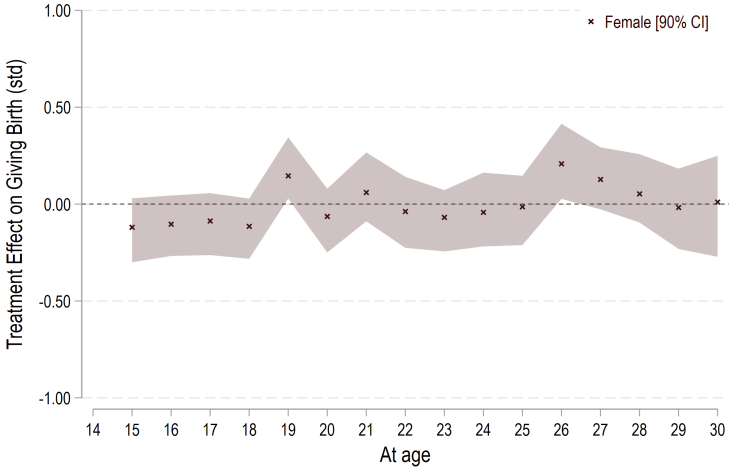
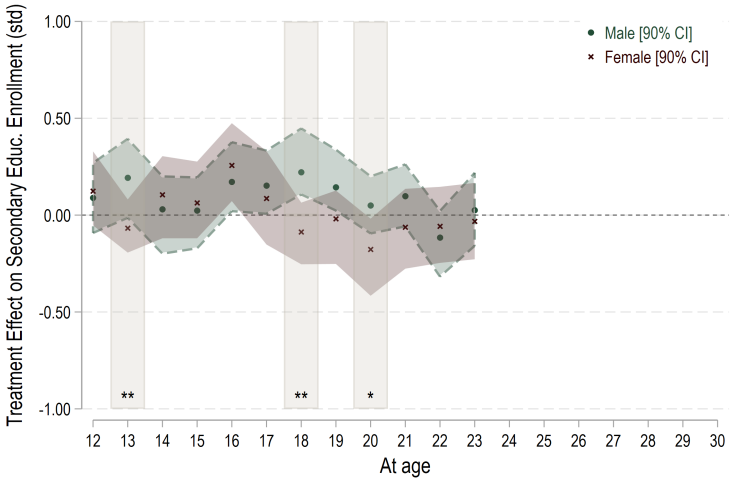


Figure G.3: Dynamic Effects, by outcome and gender - Estimate at age

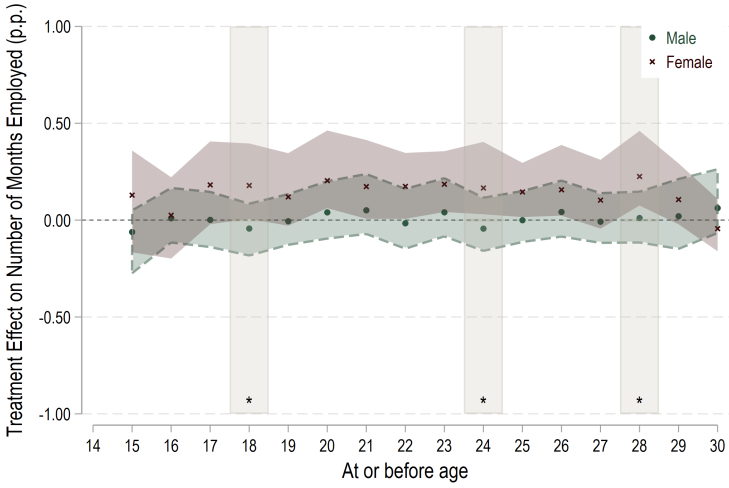
a. Fertility



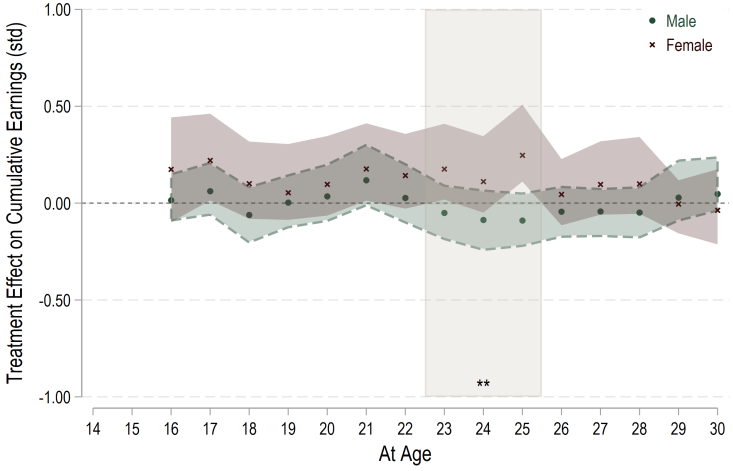
b. Secondary Education



c. Labor Market Participation



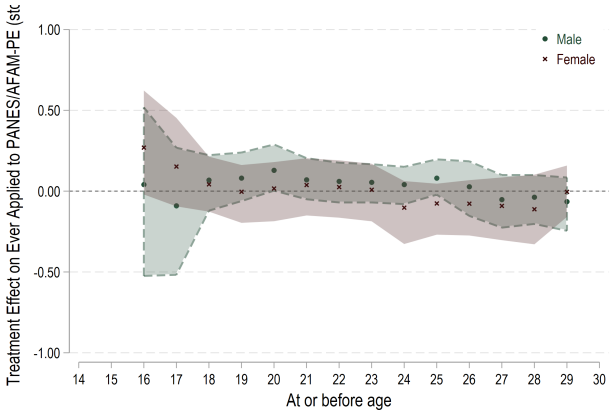
d. Earnings



H Further Results on Welfare Application and Participation

Figure H.1: Dynamic Effects, by outcome and gender - Welfare Participation

a. Applied



b. Accepted

